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EDITORIAL

Why Do Ghosts Wear Clothes?DOI: <https://doi.org/10.31275/2019/1637>

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In my previous Editorial, I looked briefly at our theoretical options for explaining the most compelling cases of apparitions. And I argued that—at least for most collective cases—our best option might be to regard apparitional figures as objective, localized entities, perhaps continuous with other materialization phenomena. I’d like now to look briefly at a related set of issues, raised by the somewhat notorious question: Why do ghosts wear clothes?

That question is sometimes posed as a glib, skeptical swipe at reports of ghosts or apparitions, who (we’re told) often appear fully (and sometimes elaborately) clad. For example, in cases of apparitions of the dead, the figures are often described as wearing costumes appropriate to the period in which the deceased lived. But despite its customary snarky intention, the question “Why do ghosts wear clothes?” does in fact point to interesting and apparently serious theoretical obstacles for certain accounts of out-of-body experiences (OBEs) and (to a lesser extent) some explanations of apparitions.

As far as OBEs are concerned, explanations tend to divide into two broad classes. According to the first, the *externalist hypothesis*, out-of-body consciousness is somehow physically separable from the body; the OBEr’s mind or mental states are somehow detachable from the body and are literally *at* the sites from which the OBEr seems to perceive. According to the second, the *internalist hypothesis*, nothing of that sort happens; the experience of being outside the body is always illusory. In short, internalists contend that the apparent OBE is merely a misleadingly vivid, imagery-rich type of clairvoyance. The internalist can also point to the extensive literature on clairvoyance to remind externalists that we have plenty of evidence of ESP success where it’s clearly preposterous to posit an out-of-body consciousness to do the clairvoyant work—say, for correct identification of cards in a sealed deck or images in a sealed opaque envelope. In these cases, the items available for clairvoyant awareness can’t be viewed from *any* position in space. But then, it’s not necessary to posit a traveling

consciousness in other cases. Internalists can always explain the varying subjective experiences of psychic “percipients” in terms of idiosyncratic manifestations of an ESP ability. (For more on psi and the nature of abilities, see Braude 2014.)

Most OBE-externalists adopt the view that one’s mental capacities can exist only so long as they’re *grounded in* or *supported by* a kind of underlying substrate. So, if our mental capacities and traits can operate apart from the body during an OBE (and perhaps persist even after bodily death and dissolution), the temptation is to posit some substrate in addition to the normal physical body to explain how that’s possible. At this point, externalists typically assert that the human mind “is essentially and inseparably bound up with some kind of extended quasi-physical vehicle, which is not normally perceptible to the senses of human beings” (Broad 1962:339). It’s this vehicle that some identify as the *secondary* or *astral body* they experience during OBEs, and which observers at remote locations apparently perceive in so-called *reciprocal* cases—that is, cases taking the following form: Agent *A* experiences an OBE in which *A* ostensibly “travels” to percipient *B*’s location and is subsequently able to describe features of the state of affairs there that *A* could not have known by normal means. *B*, meanwhile, experiences an apparition of *A* at that location. (In a few instances, others on the scene also experience *A*’s apparition.)

As far as explanations of apparitions are concerned, the main contenders are a *telepathic* approach (positing nothing but ESP-interactions either between postmortem and ante-mortem individuals or between ante-mortem individuals), and an *objectivist* account, according to which apparitions are distinct entities (perhaps psychokinetically produced) actually located at the place where they’re perceived. Of course (as I noted last issue), the totality of apparitional cases needn’t be handled by just one approach to apparitions. Some cases may be most neatly explained telepathically, while others—collective apparitions especially—seem to be handled most easily by an objectivist approach.

The Problem of Apparitional Clothing

We may now consider how the old question about why ghosts wear clothes highlights a problem for both the externalist account of OBEs and the objectivist account of apparitions (extended to cover all cases—not just collective apparitions). In my book, *Immortal Remains*, I explained the problem as follows:

Suppose that, while decked out in my new Armani suit, I try to project myself in an OBE to a friend, who then has an apparition of me in my sartorial splendor. If we explain my friend's ability to describe me accurately by positing a traveling "secondary body," how do we explain my friend's experience of my new suit? Does my Armani suit also have a double? It seems absurd to think so. But if we can—and indeed, *should*—explain the apparition of my Armani suit without appealing to a secondary or astral suit (e.g., if we explain the apparition of my suit in terms of "ordinary," non-traveling ESP), it seems far less compelling to explain the apparition of *me* in terms of a detachable part of consciousness or secondary body. (Braude 2003:266–267)

The case of Miss Johnson illustrates the issue nicely. Early on the morning of January 27, 1957, "Martha Johnson" (a pseudonym) from Plains, Illinois, had a dream in which she traveled to her mother's home, 926 miles away in northern Minnesota. In a statement sent to the American Society for Psychical Research (ASPR) the following May, she wrote,

After a little while I seemed to be alone going through a great blackness. Then all at once way down below me, as though I were at a great height, I could see a small bright oasis of light in the vast sea of darkness. I started on an incline towards it as I knew it was the teacherage (a small house by the school) where my mother lives. . . . After I entered, I leaned up against the dish cupboard with folded arms, a pose I often assume. I looked at Mother who was bending over something white and doing something with her hands. She did not appear to see me at first, but she finally looked up. I had a sort of pleased feeling and then after standing a second more I turned and walked about four steps. (Dale, White, & Murphy 1962:29)

Martha woke from her dream at 2:10 a.m. (1:10 a.m. in Minnesota). The dream "nagged" her mind for several days, at which point she received a letter from her mother, who wrote that she'd seen Martha. Martha then replied, describing her experience and asking her mother to identify what she'd been wearing. A second letter from Mrs. Johnson answered that question and provided further details about her experience.

In the first of her two letters, dated January 29, Martha's mother wrote,

Did you know you were here for a few seconds? I believe it was Saturday night, 1:10, January 26th, or maybe the 27th. It would have been 10 after two your time. . . . I looked up and there you were by the cupboard just standing smiling at me. I started to speak and you were gone. I forgot for a minute where I was. I think the dogs saw you too. They got so excited and wanted out—just like they thought you were by the door—sniffed and were so tickled. (Dale, White, & Murphy 1962:30)

Mrs. Johnson's second letter was written on February 7, 1957. She wrote,

I was bending over the ironing board trying to press out a seam. . . . You were standing with your back to the cupboard (the front of it) between the table and the shelf, you know, just sort of sitting on the edge of the lower part of the cupboard. . . . I looked at the dogs and they were just looking at you. I'm sure they saw you longer than I did. . . . I turned to go in the bedroom and you must have started to go out the door then. That's when the dogs went wild.

Your hair was combed nice—just back in a pony tail with the pretty roll in front. Your blouse was neat and light—seemed almost white. . . . You were very *solid*—JUST like in life. Didn't see you from the lower bust down—that I can remember, anyway. (Dale, White, & Murphy 1962:30)

Martha confirmed in correspondence that during her "visit" she had indeed experienced her hairstyle and clothing as her mother described.

It should be clear why this case poses a problem for the OBE-externalist and to a lesser extent (if at all) for an objectivist explanation of the reciprocal apparition. The clothing and hairstyle of the apparitional figure were not those of the sleeping Miss Johnson. They corresponded, instead, to the way Miss Johnson experienced herself during her OBE. So assuming that telepathic explanations are at least sometimes appropriate, one such explanation comes immediately to mind. Presumably, Miss Johnson's hairstyle and clothing during her OBE are thoroughly subjective constructs, just as they would be if her experience were merely a dream. But then it certainly looks as if Miss Johnson telepathically communicated those features of the OBE to her mother, as well as influencing Mrs. Johnson to experience her with arms folded, near the cupboard, etc.

Of course, an apparitional experience could be a mixture of genuine perception (say, of a spatially located apparitional figure) with a telepathically induced quasi-perception (of the figure's attire, etc.), just as genuine and quasi-perceptions would combine if I were to hallucinate a hippo in the real corner of the room. But if we must appeal to ESP (telepathic influence) to explain parts of the apparitional experience, then it may simply be gratuitous to suppose (along with the OBE-externalist) that a fundamental but detachable part of consciousness (or astral body) was actually present at the remote location.

The situation is not as grim for proponents of objectivist accounts of apparitions. In principle, at least, Mrs. Johnson might have psychokinetically created a temporary entity corresponding to the mental images telepathically received from her daughter—that is, rather than keeping the entire

experience “in her head.” And in fact, Miss Johnson herself might have psychokinetically created an entity at her mother’s location. Either of those conjectures might help explain why the dogs were looking where Mrs. Johnson perceived the apparitional figure to be. (Of course, the dogs might also have been telepathically influenced by either Miss or Mrs. Johnson.) At any rate, this case, and others like it, show clearly why an OBE-externalist appeal to *the OBEr’s fundamental but detachable and traveling secondary body* seems less plausible than an explanation entirely in terms of ESP or in terms of PK on the part of either agent (OBEr) or percipient. (Moreover, as I discuss below, if the apparitional figure is a localized entity but its attire, etc., are not, does that mean the apparitional figure/astral body goes forth naked into the world?)

Furthermore, in some reciprocal cases, it’s the percipient, rather than the OBEr, who seems to supply the apparitional clothing, etc. In one such case (summarized in Myers 1903, vol. 1:688–690), the Reverend Clarence Godfrey tried to appear to a friend at the foot of her bed. He made the mental effort in the late evening after retiring to bed, and he fell asleep after about eight minutes. He then dreamed that he met his friend the next morning, and she confirmed that he had appeared to her. This dream woke him, and he noticed that his clock showed 3:40 a.m. When his friend actually confirmed the experiment’s success the following day, she noted that it occurred at about the time the servant put out all the lamps, which usually took place around 3:45. In her written account, she says that Godfrey “was dressed in his usual style.” Frank Podmore recognized the significance of this. He wrote that the apparition’s dress

was that ordinarily worn in the day-time by Mr. Godfrey, and that in which the percipient would be accustomed to see him, not the dress which he was actually wearing at the time. If the apparition is in truth nothing more than an expression of the percipient’s thoughts, this is what we should expect to find, and as a matter of fact in the majority of well-evidenced narratives of telepathic hallucination this is what we actually do find. The dress and surroundings of the phantasm represent not the dress and surroundings of the agent at the moment but those with which the person is familiar. (Quoted in Myers 1903, vol. 1:689–690)

In a similar case, Mr. G. Sinclair tried mentally to “visit” his ailing wife, whom he had left back at home while he was traveling (Myers 1903, vol. 1:697–698). At the time of Mr. Sinclair’s attempt, he was undressed and sitting on the edge of his bed. Mrs. Sinclair later wrote, “I saw him as plain as if he had been there in person. I did not see him in his night clothes, but in a suit that hung in the closet at home.” Because the apparitional clothing in

these cases seems to be supplied by the percipient's mind, the cases clearly support the view that the apparition itself is likewise (as Podmore put it) "an expression of the percipient's thoughts" and not an astral body perceived normally and visually.

We should note again, however, that the percipient can express those thoughts either by means of a purely subjective hallucination or by psychokinetically creating a temporary, spatially located entity. And as before we can't rule out the possibility that Mr. Godfrey or Mr. Sinclair created a (naked?) entity at the percipient's location. So the Godfrey and Sinclair cases, like that of the Johnsons, create some uncertainty for the interpretation of the apparitional figure. That figure might still be a PK-construct—in fact, by either the percipient or the OBEr/psychic "traveler." But because it's the percipient who seems to supply the apparitional clothing, etc., those cases certainly discourage an OBE-externalist appeal to the *OBEr's* essential but traveling secondary body. In these cases, an explanation in terms of percipient-ESP (with or without additional PK of a localized entity) seems clearly to be more straightforward, and the considerations discussed below about astral nakedness reinforce the point.

Here's the problem. If an apparition's clothing is constructed subjectively in response to telepathic influence, then what (according to OBE externalists or apparition objectivists) would observers perceive if the telepathy were unsuccessful or—as is often noted—deferred to a later time? We should remember that many reports of apparitions (perhaps especially from so-called "crisis" cases) suggest that there may be a period of latency between the "sending" of a telepathic message (attempted communication) and the subsequent telepathic experience of the percipient. In fact, the evidence suggests that the emergence into consciousness of (or the behavioral response to) a telepathic stimulus frequently occurs when that event is convenient or otherwise appropriate relative to ongoing background events or the subject's state of mind.

So if OBE-externalists want to say that only the secondary body—but not clothing, etc.—is genuinely observed by the percipient, and if apparition-objectivists insist that the perceived figure is a genuinely localized PK-construct, are we to suppose that this body is unclad and that the clothing is supplied telepathically? What would happen, from that point of view, if the telepathy were unsuccessful or delayed? Would there be, in those cases, perceptions of naked secondary bodies or apparitional figures? In fact, if OBE-externalists contend that our secondary bodies go forth into the world unclad, one would expect to find at least some reports of naked apparitions. For that matter, considering the vagaries of successful ESP and PK, one

might even expect the genuine perception of naked secondary bodies to occur more reliably than the associated quasi-perceptions of their clothing, etc. However, the sizeable literature on apparitions contains almost no reports of naked human figures. According to Irwin, “in Crookall’s extensive case collection only four such cases occur and in some of these the astral body quickly became clothed” (Irwin 1985:229).

At this point, OBE-externalists might argue that one’s secondary body has a certain degree of malleability, so that it can alter its age, size, and other features (e.g., whether or not it has a beard, or long hair). So perhaps this malleability can also extend to the simulation of clothing, etc. However, certain cases make that externalist strategy seem particularly incredible. Consider the following example, cited by Gauld (1982). The two persons in this case had agreed to experiment with producing OBE apparitions.

JAKOB: The day after our decision I drove my daughter to her job, the time was 6 p.m. I was suddenly reminded of this agreement with Eva. Then I transported myself astrally to her home and found her sitting on the sofa, reading something. I made her notice my presence by calling her name and showing her that I was driving my car. She looked up and saw me. After that I left her and was back in the car which I had been driving all the while without any special awareness of the driving.

EVA: I was sitting alone in the room in an easy chair. . . . Suddenly I saw Jakob sitting in front of me in the car, saw about half the car as if I were in it with him. He sat at the wheel: I only saw the upper part of his body. I also saw the clock in the car, I think it was a couple of minutes before six. The car was not headed towards our house but in another direction. (Gauld 1982:228)

Presumably, positing the existence of a duplicate car is at least as implausible as positing the existence of duplicate clothes. And as Gauld notes, even if the OBE-externalist manages to explain how a secondary body might transform its outer parts into semblances of clothing, etc., it seems excessive to suppose that our subtle bodies might also shape-shift into a half car with a clock showing the correct time. If (as it appears) a telepathic explanation is more compelling in this case, that would seem to weaken considerably the externalist recourse to secondary bodies in other cases. Apparition-objectivists are perhaps somewhat freer to propose that PK-created figures are malleable in their appearance. They’re not committed to the positing of fundamental-but-detachable duplicate or secondary bodies. But again, explanations of these cases entirely in terms of ESP seem appealingly straightforward.

Conclusion

Of course (as I've noted), the variety of OBE accounts and apparitional cases accommodates—in principle at least—a variety of explanatory options. And there's no reason to insist that all cases must be explained along the same general lines. Nevertheless, the problem of apparitional clothing serves as a useful reminder that some popular externalist accounts of OBEs might be considerably more simplistic than is usually appreciated.

—STEPHEN E. BRAUDE

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GUEST EDITORIAL

On the Potential Role of Psi in an Expanded Science of the Physical, Experiential, and Spiritual

CHARLES T. TART

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*People who say it cannot be done
should not interrupt those who are doing it.*
—George Bernard Shaw

Note: This paper is an experiment for me, so there may be rough spots. Its basic goal is to share my excitement about possibilities for parapsychological applications to help investigate reality, hopefully to stimulate readers who will advance these ideas. . . . I'm relatively retired now. It was originally scripted as an invited address. I'm a good speaker, and it's easy for me to share possibilities by the way my voice and gestures communicate my excitement, and the controlled pace of a lecture lets me build the right atmosphere. Too, when I'm a well-known scientist and colleague to the audience, I can deepen rapport by being relatively informal and personal while outlining the basic science. But converting this to a journal article eliminates much of the above advantages inherent in speaking, and a journal article should more formally demonstrate some of the scientific roots of my proposals, especially to younger investigators who don't know my previous work. So the experiment I'm trying here is to stick close to the somewhat personal, informal, enthusiasm-conveying tone of a lecture but demonstrate its scientific roots with extensive Notes. I invite you to read just the text, pick up on the feelings, put questions and criticism aside for the time being. "Take the trip" with me, while skipping the Notes (I know you'll peek at the first few anyway). Then if you have picked up enough enthusiasm to want to go deeper, go back through reading the Notes to get the references, methodological deepenings, questionings, etc. And then . . . maybe you'll be one of those who advances our fields along these lines!

In 2018 I was honored by being asked to give an invited lecture at the annual meeting of the Parapsychological Association. What would I talk about?

I think of my primary professional identity as being that of a transpersonal psychologist, interested in the wider aspects of the human mind—actually I'm interested in too many things, but that's how I attempt to focus—but parapsychology has usually taken up about a third of my professional time. The problems posed by the existence of psi are especially interesting, and psi phenomena and their implications are very important for people in general,¹ as well as for scientists. Also, since it is still largely a taboo topic in science, for various irrational reasons, parapsychology has remained a very tiny scientific field, with very few people working in it, even part-time, and very few resources available for wide-scale experimentation. So my first thought of what to talk about in my lecture time was that in the half-century—plus that I've worked in parapsychology, I've seen a lot of promising, initial developments that didn't really get followed up adequately, so perhaps I could devote my hour to discussing methodological problems and promising leads that had slipped between the cracks?

I am very empirically oriented. At least that's what I discipline myself to be: What are the observable facts in a particular area? That's the primary thing, indeed the essential thing to work from. I say *discipline* myself to be that way, because most of my life I've been what I call, in a term I coined, a *thoughtaholic*. I love clever thoughts! If I were to go to something like an Alcoholics Anonymous meeting and was asked what my drug of choice was, I would say something like "Ideas! Concepts! Theories! They get me so high! I get so attached to them!" Thinking about things and coming up with what seem to be insights about them has been a major quality of my consciousness since I was a child.

That's why science is a wonderful personal discipline for me. If I had to characterize essential science very simply, I would say we're curious and want to find out things. We create clever ideas as to *why* things are the way they are, *but must constantly ask if our clever theories fit the current observable data and predict future, observable data?* If they don't fit the observable data, or make any testable predictions, the theory is a nice idea but it's got to be modified or dropped in favor of something more adequate if it's to be part of essential science. Back to the drawing board! Data are *always* more important, more basic than theory!

As important as accounting for observable facts is, I didn't want to give a mainly negative talk about methodology and problems we haven't solved, things that didn't work. Yes, there are real problems, but we can potentially solve them! Negative focus would also tie in too well with the fact that the field of parapsychology has been stuck in the more proof, more proof, . . . more proof ad infinitum stage, ignoring the fact that most of the opposition to its findings is irrational, stemming from common human shortcomings

of people rather than from their scientific skills. To the contrary, I believe that *parapsychology now has the potential to build investigative tools that will be central to an expanded scientific and human/spiritual understanding of the mind*. What I've learned working with parapsychology, altered states of consciousness, transpersonal psychology, and related fields has been a personal inspiration to me throughout my life, and I want to share that inspiration!

Inspiration, sharing of a vision, is a lot easier in a lecture than in a journal article (Longwinded 1954, Criticality & Delusion 2004, Poverty, Bureacracy, & Politics 2001, Grammarsansmerci 1967, Proofreader 2113), for although I love footnotes (Presentation Committee for Professional Ennui Generators 2001, Tedium & Ennui 1986) when I'm perusing a detailed experimental report (Multivariate Inverse Bayesian Statistics Standards White Paper, 2001, 2004, revised 2013, Fussbudget, Beancounter, & Doubter, 1987),² as you, dear reader, are probably already noticing rather directly in making sense of this sentence, a journal article is not good for conveying and sharing inspiration. Thus the attempt, described below, to reduce the chances of scholarship habits burying inspiration . . .

Now into my 80s, I don't know how much longer I'll still be active in parapsychology and related fields, so I wanted to create some inspiration for my colleagues, especially the younger ones. There are exciting and valuable things we can learn to do! So vision with me a while, let the practical difficulties be in abeyance for now . . . I believe that the kinds of investigators who read the *Journal of Scientific Exploration* will actually be more open to this possible vision than many parapsychologists, for, clever, devoted, and methodologically advanced as most parapsychologists are, they have been on the defensive from irrational attacks from pseudo-skeptics for so long that their thinking has gotten trapped in that more proof, more proof . . . more proof mentality, instead of visioning where we could go . . . and starting to take us there.

Beginning back in the 1970s, we went somewhere important! I was fortunate to be a consultant for the initial research project on developing and using *remote viewing*, carried out at a major research institution, SRI International, in Menlo Park, California. I worked closely with the main project researchers, physicists Russell Targ, Harold Puthoff, and, a little later, Ed May. Besides seeing what struck me as powerful and useful psi happening in many of the remote viewing experiments and applications, I learned an interesting thing about the way physicists have worked through recent history, namely that they don't always stick just to the data, just to the facts. In spite of my emphasis on it above, scientists sometimes got excited about ideas just because they were "interesting." Albert Einstein is a well-

known example. Who in the world would be crazy enough to spend a lot of time wondering what it would be like to ride through space alongside a light beam? This had nothing to do with things you could clearly observe and measure in the laboratory at that time, and yet it led Einstein to some very interesting theories, theories later receiving excellent empirical confirmation. There are many other instances in the history of physics of “interesting” ideas leading to valuable discoveries.

Remote viewing was the primary procedure involved in three major parapsychological advances during my career: (a) the initial development of the procedure and its successful intelligence-gathering applications at SRI, (b) the usefulness of remote viewing in archaeological work done by Stephan Schwartz, and (c) the several instances of using associative remote viewing to guide investment strategies to make money.³ *I'll take it now as an established, baseline fact that remote viewing can sometimes gather, by psi, useful information about the present and future state of reality.* And, optimist that I am, I'll assume that the “sometimes” can undoubtedly be improved with further study of the phenomena.

So I'm going to present two ideas to you, which I find quite “interesting.” They are beyond what's practical for parapsychology to begin working on in detail yet, but I find them very, very interesting in their potential. *If* my resulting proposals work out, they may take the field of parapsychology, indeed the fields of psychology and science in general, a long way forward, introducing new tools and ways of understanding. Parapsychology could become a central scientific field and psi used practically in many fields of knowledge acquisition.

And if they are just intoxicating ideas, but don't really reflect reality, are they factually wrong? Well we haven't been making progress all that fast anyway, so suppose we take a gamble? I'm inviting you, as we often said in the 70s, to take a “trip” with me, to look at some possibilities for parapsychology that could make it a vitally important part of science instead of an endlessly rejected field, with the pseudo-skeptics ignoring what has been discovered and always asking for more evidence, more evidence, more evidence, none of which could possibly make them change their mind about the existence or importance of psi, with too many parapsychologists still caught up in finding more and more evidence for the reality of psi—that is going to be irrationally ignored and rejected anyway . . .

So I'm asking you, dear reader, to relax and open your mind for a while. As Henry Ford is reputed to have said, those who think they can and those who think they can't are both right . . .

As to the difficulties? The *Journal* editor Steve Braude commented on an initial presentation of this proposal that the stimulating value of the

ideas might be lost if people thought that I was unaware or naïve about the difficulties involved, so I ought to briefly say something to the effect of knowing what some of the many difficulties are. Indeed, I do know of them! But I want to take you on this interesting trip, not to immediately bog you down in a swamp of endless methodological haggling. As a personal analogy, in the 1950s I taught myself much about electronics while I was still a teenager, and passed the Federal government licensing tests to qualify for and work as a Radio Engineer. I liked to actually build radio receivers and transmitters, too, not just theorize about them, and I always thought it would be neat to have my own portable transceiver, what was nicknamed a walkie-talkie in the Second World War, that would do the kind of things a cell phone routinely does now. But with the technology back then, what I or much more skilled engineers could build would weigh, even in its simplest form, way too much to be carried in anyone's pocket, and have only a very short range! I did a little work toward building one, but contemplation of the methodological difficulties stopped me from attempting to actually build anything full-featured. I was practical for the technological state of the time. Yet various people "took the trip," small step by small step, and the cell phones we have today are quite miraculous by walkie-talkie standards . . .

So I'll make a few comments about some of the more specific difficulties via the Notes scattered through this paper, but only a few, because the important goal to me is convey the interestingness of this idea. I hope this may trigger some further thinking on your part, dear reader, thinking that will expand and clarify these ideas. If you like the idea, a rereading of the paper with the Notes as they come up may be useful. And, even if you think these ideas are wrong and misleading, perhaps in the course of thinking about *why*, that may lead to some interesting thoughts too . . .

Toward a Science of Mind

As I thought about the implications of what I want to focus on, a better title than the original one for the Parapsychological Association convention⁴ would be "On the Potential Role of Psi in an Expanded Science of the Physical, Experiential, and Spiritual." The implications go far beyond specialized uses of parapsychology.

Like most ordinary people, I have always thought of psychology as the study of the mind. The progressive abandonment of mind, consciousness, awareness, experience as primary, in the belief that only physical matter was ultimately real and mind would eventually be explained (away) in physical terms, never made sense to me.⁵ Yes, there were physical components, brain and nervous system, but how could we develop a science of mind that included consciousness fully?

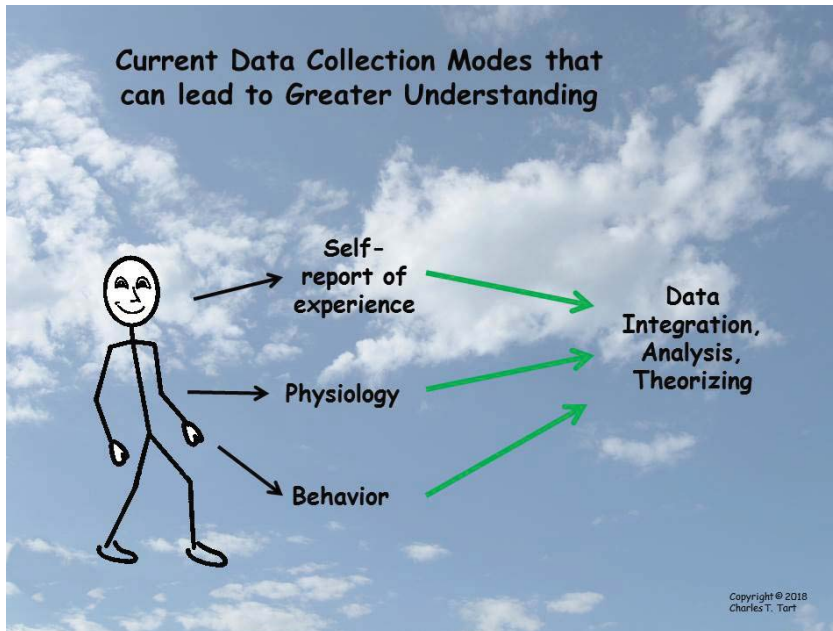


Figure 1. Current data collection modes that can lead to greater understanding.

If we think about how psychology functions now, we can conveniently talk of three major sources of data, sketched in Figure 1. One is the self-report. A person describes what he or she is experiencing, his or her intentions, etc. When psychology was originally conceived as a science of the mind in the 1800s, this was far and away the major source of data.

A second major source of data, increasingly important as our technology developed, was a person's physiology. What was his or her body and brain doing at various times? Showing signs of arousal or relaxation? Were there physiological signs that correlated with what they might be reporting as to their experience? Or, possibly even more interesting, contradicting what they were reporting? In what ways could you argue that a certain physiological change *caused* the experiential change? And indeed, as a philosophy of total materialism became more and more dominant in the culture of science, physiological measures began to be considered to be more valid and more useful than self-report. We see this very much now in the belief that once we fully understand how the brain functions, we will be able to fully explain *everything* that a person experiences, for *experience, consciousness, is nothing but a product of physiology*, especially brain physiology.

The third major source of data was a person's behavior. An insistence that this was far and away the primary source of data for psychology led to the development of Behaviorism, which was still dominant when I was trained in graduate school half a century ago. More open-minded psychologists today would probably say that the interactions of all three of these kinds of data help produce more adequate explanations, better theoretical ideas that explain what a person reports, what his or her physiology indicates, and what his or her behavior shows. That often includes a firm belief that consciousness, mind itself, will eventually be totally explained by brain action. Once we can really measure ongoing brain activity, self-report and observed behavior will become very minor kinds of data, as they can be better explained by brain activity.

Now we must drastically expand the conventional perspective.

One of the basic findings of more than a century of parapsychological research has been the repeated finding that sometimes a human mind can find out information or do things that a brain alone cannot do, given our current understanding of the physical nature of the brain and world and straightforward extrapolations of that knowledge. In hundreds of successful experiments over the decades, you ask a person to find out some information or modify some physical process when the situation has been set up such that there are no known physical channels to convey information or energetically affect a physical process at some distant, shielded target. You, as experimenter, a mind, understand what you are requesting, the person taking the role of percipient or agent understands what you want and intends for it to happen, and objective observations reveal significant information acquisition or process modification beyond chance expectations. Therefore, understanding the mind or minds involved is a fundamental issue for a science of psychology, not covering over our ignorance about the mind and its inherent nature with the faith (known as *promissory materialism*⁶ among philosophers) that someday brain functions will explain *all* mental functions and experience.

Thus I think that it is vitally important to get more reports of experience and more adequate reports into psychology, to put more *psyche* in psychology. By analogy, I think we should be putting a lot of emphasis into developing a psychology that is an analog of, say, the science of chemistry.

Physical chemistry progressed by discriminating basic elements, down to the atomic level, and eventually the subatomic level, and understanding the rules of reaction and interaction. Element A, for example, would react with element B under certain conditions to form compound X, element D wouldn't chemically react with any other elements, etc. In a science of mind, my long-term goal here is to recognize that *experiences are basic data*, not

things to automatically assume we will explain away as we understand the physiology better, and we go on to understand those basic experiential elements and the laws of interaction more and more precisely, refining and sharpening our theories to account for reported experiential data more and more adequately. At our present level of knowledge most experiences are probably like molecules of more fundamental mental chemistry. Deeper research may reveal the more basic atomic-like structures.⁷

Here I use “experiences” to include those directly known by an observer, one’s own experiences, as well as experiences reported by others. I emphasize that *I posit experience as one kind of fundamental data*. Physiology? It’s valuable to know that a certain region of the brain or body is more activated or inhibited sometimes or regularly when certain experiences are reported, but experiences should be treated as data just as fundamental as physiology or behavior. If a person reported feeling unusual warmth in their body, for example, or felt *out* of their body, as in an out-of-the-body experience (OBE), or reported that they were experiencing tremendous certainty about a specific idea, or feeling a rapid shifting of body area focus, these all are data that go toward creating a science of the psyche. We would then go on and try to find the more basic elements of experience and their rules of combination. These would include straightforward rules of combination as well as knowing about how things emerge in complex systems, systems emergents.

Historically, Wasn’t Mental Chemistry a Failure?

But wait, you might say: Wasn’t the mental chemistry model tried in early psychology—and failed? Yes. Rather than quickly arriving at a general consensus of what the basic “atoms” and “molecules” of experience were, and the “reaction laws” governing their interaction, different laboratories reported quite differing results under supposedly similar circumstances. This quickly killed the hope of producing a scientific mental chemistry.

I would argue that these attempts failed because of some major limiting and biasing methodological problems that were not adequately recognized. I believe that if we recognized and dealt with these problems constructively, we might make enormous progress. I’m not a historian of psychology, so I’m going to give overall impressions here, not details, about these methodological challenges,⁸ about why the mental chemistry attempt failed. Other reasons than those I highlight here undoubtedly also exist. I’m not so interested in trying to be absolutely accurate on the history as I am in inspiring people to learn to deal with these methodological problems.

I would stress that much of the power of these limiting and biasing issues, summarized in Figure 2, came from the fact that, by and large, they

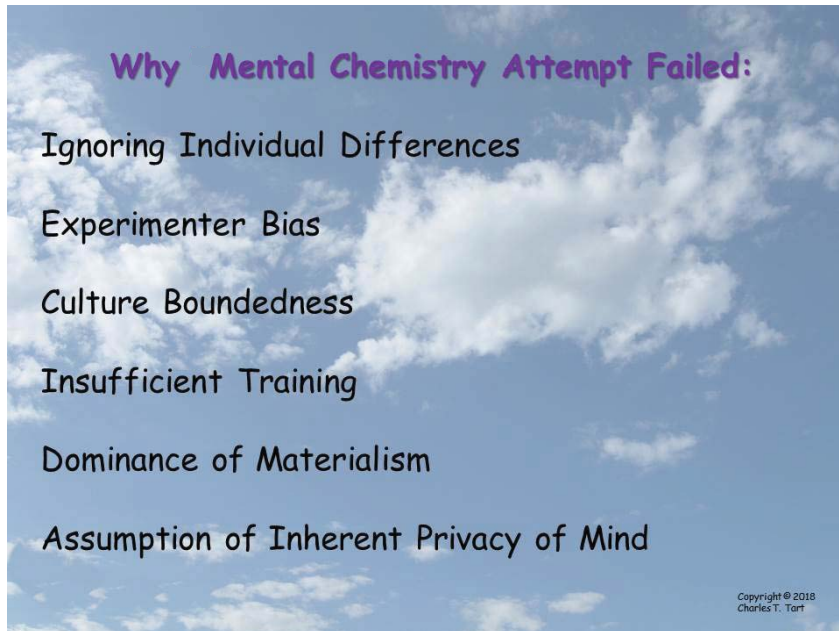


Figure 2. Why mental chemistry effects failed.

were not recognized. When you don't know that something is limiting and biasing you, there's little you can attempt to do about it . . .

Limiting and Biasing Issues

Individual Differences: A major problem that led to the failure of mental chemistry was ignoring individual differences. There was often an assumption, largely implicit, that any person⁹ you brought into the laboratory to ask about their experience had what I'd call a "Standard Mind," that our minds all basically worked the same way. The more I learn about how different "normal" people think, sense, and feel, the more misleading this assumption is! Those of us who've been in long-term relationships with loved ones, for example, have learned, often the hard way, that this is not true. The way this loved one, this otherwise clearly intelligent person, senses and thinks and reacts about some things can be drastically different from your own, even if your own seems like the *obvious* and *correct* way to understand and react! How odd . . .

Not only were individual differences largely ignored, but of course it's difficult to describe experience.¹⁰ The reality of individual differences

coupled with our social nature (we want to fit in and be “normal” for our tribe) was even more problematical as a common “laboratory language” evolved so observers and experiments could feel like a part of the particular laboratory grouping. So in addition to what may be basic differences in how different minds function, there are differences in ability to observe and describe that functioning, and social biases slanting descriptions and observations per se.

Experimenter Bias: A second basic reason why mental chemistry failed was the problem of experimenter (E) bias. I’ll deliberately begin using E for the Experimenter from this point on¹¹ to remind us of the usually implicit special laboratory languages, which can affect our thinking . . .

This is a problem that is still very much with us, and that scientists are socially and personally biased not to recognize—except, perhaps, in other scientists’ work. We are objective Authorities, we are Es, not biased people!

I became practically aware of the importance of E bias in a 1964 study of hypnotic suggestibility during my postdoctoral training at Stanford. My co-Es in the study couldn’t seriously consider that we might be doing (and effectively invalidating) the study in a biased way—although it turned out we were (Troffer & Tart 1964). I could argue that many, if not *most*, apparently factual findings in psychology may well be due to E bias, rather than the discovery of basic rules of behavior or physiology or self-report. Arguing this does not make me popular with most colleagues! The introspectors, those who observed their mental processes and reported on them, the basic data collectors in the historical attempt to establish a mental chemistry, were employees or students of Herr Doktor Professor X, who had theories to prove, was their employer, was an Authority way above them in social and scientific and academic status. Again, in spite of our commitment as scientists to “objectivity,” we humans are also social animals and much want to fit in with and be accepted by the culture around us, especially with the powerful people in it, so the assumption that we can easily get unbiased reports from standard minds ignored a lot of psychological and social reality.

Culture Boundedness: I would stress culture boundedness as a third major reason why the mental chemistry attempt failed. Some cultural differences are subtle and some are very obvious, such as the difference between authoritarian and egalitarian cultures. Our culture has shaped our mental processes, our perception, thinking, emoting, and behaving, so that, with our desire to be “normal,” we may indeed be “normal” for our culture, but not so for the culture background of the introspectors reporting in other researchers’ laboratories. Ordinarily we are not even consciously aware that we are members of a particular culture instead of just intelligent human beings, much less aware that we are biased by our culture.

Insufficient Training: Another major reason why I think the mental chemistry attempt failed was grossly insufficient training of the introspectors. My memory of the history is that a “trained” introspector was someone who had had perhaps 10 to 20 hours of training in how to report aspects of ongoing mental experience in a way that was, hopefully, accurate. While I’ve been very interested in observing my own mental experience as accurately as possible all of my life, as well as trying to be able to communicate it, I still find both tasks quite difficult. By contrast with “normal” culture, I have talked with a number of meditation teachers, especially those who teach variations of the basic Buddhist practice of vipassana (insight) meditation (Young 2016), where one tries to learn to observe ongoing mental experience without biasing, without trying to push it toward desirable forms or prevent it from going in undesirable directions. The several teachers I’ve spoken with about accurate observation and reporting think that in Buddhist tradition, derived from 2,500 years of meditation practices, generally it takes 5,000 to 10,000 hours of training in vipassana meditation for a person to get really skilled at it! This is in cultures that value meditation. I certainly know it took me many years of practicing various forms of vipassana to observe my mind with *less* interfering and biasing, and I’m not sure I’m actually able to observe anything in my own mind without some biasing to push things in desired directions. Sometimes my most productive meditation sessions are continual insights that in spite of my attempts to just observe mental events that happen on their own, as objectively as possible, I’m actually habitually pushing and pulling on my mental processes all the time. I don’t like to frequently find out how biased I am, but I believe knowing I’m biased makes me more likely to be able to reduce the effects of bias in my work.

Materialistic Substrating of Experience: A fifth reason why the mental chemistry attempt failed strikes me as the dominance, increasing through this historical period, of an absolute and *dismissive* materialism, the belief that *everything* will be explained by the physical processes of matter, particularly of the brain and nervous system. This includes all aspects of experience, of mind. Mind will be *explained away* by a reduction to a causal physical substrate. This also tends to produce a bias to try to describe mental experience in terms that sound like those of the physical sciences, giving the comforting illusion of scientific precision.¹² Also there’s an underlying bias that a mental chemistry must be inherently inferior in terms of precision and reliability to any physical science. Reporting, for example, that “This object in front of me produces nervousness” is far inferior to the standard (still a theory, not a fact) that a *real* explanation would be something like “When neurons numbers so-and-so are stimulated

by such-and-such neurochemicals in a certain temporal pattern, the person will (through fully explicable neural pathways) use the word nervousness.” This materialistic subrating of experience means it is inherently inferior data compared with material, physical data. It undercuts attempts to build a science if you believe, a priori, it’s going to be inherently inferior.

Assuming You Can’t Do Well, It’s Inherently Private: A sixth major reason I think the attempt at mental chemistry failed was the widespread assumption that what goes on in your own mind is *inherently private*. If you don’t report it, or if you can’t report it adequately, nobody but you will know what went on, and even you may not know it very well. This immediately implies that there are many aspects of consciousness it is basically impossible to adequately report, so your attempt to create a science of mental functioning is further bound to be of an inferior and incomplete sort at best. It would be analogous to want to study how an automobile functions, but you know you will never successfully look under the hood, although something basic seems to be happening there.

Again, as Henry Ford is reputed to have said, “Those who think they can and those who think they can’t are both right.”

Toward an Expanded Science of Mind: Developing and Using Ψ as a Reliable and Accurate Observational Method

Without being concerned at this point as to what the ultimate nature of the psi involved in the remote viewing procedure is, whether it’s what we conventionally call clairvoyance, telepathy, or precognition, or something else altogether, the fact remains that it has been one of the most successful parapsychological achievements in my lifetime. Developed originally at SRI and funded by various government intelligence agencies, as well as independently by Stephan Schwartz and his Mobius group for archaeological applications,¹³ it’s proved to be not only a statistically significant way of gathering information, but a highly *practical* one.

We will take the remote viewing procedure and expected improvements in it as a basis for the vision I am sharing.

These practical applications, especially the archaeological ones, often involve cross-validating teamwork, several remote viewers working independently, and then having their viewings selectively combined. This involves assuming (realistically) that no one remote viewer, no matter how good, is absolutely correct all of the time. Every viewing is a mixture of signal, information obtained by psi, and noise. Noise may be imaginative elaborations from the remote viewers’ minds and other irrelevant associations incorrectly inferred about the target, or even displaced psi perceptions. Learning to usefully combine the material from different viewers,

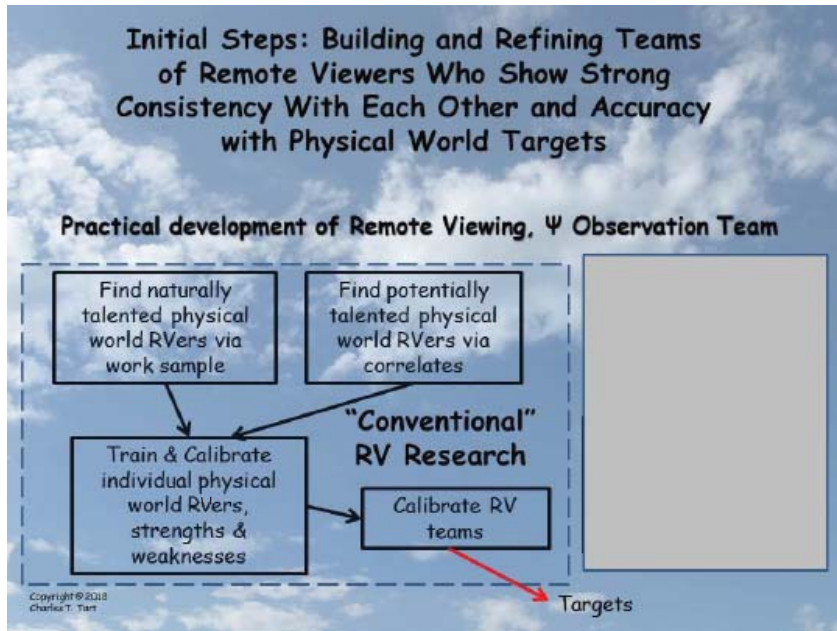


Figure 3. Initial steps: Building and refining teams of remote viewers who show strong consistency with each other and accuracy with physical world targets.

and knowing viewers’ individual strengths and weaknesses, can make the procedure more accurate.

Basic Remote Viewing Procedure

Here’s how we would go about using the basic remote viewing procedure to psychically acquire information on some distant physical target that the viewers and the interviewers of the viewers know nothing about. The basic steps are diagrammed in Figure 3.

To begin, you find some people who can, not always but often, successfully remote view. These may be people who have a reputation for being psychic, or previous specific remote viewing successes, or have certain correlated qualities that promise successful remote viewing.¹⁴ But whether such prior reputation or correlation exists or not, basically you test someone in a remote viewing procedure,¹⁵ and if you find good results they may become a long-term viewer. You then train them as well as we know how to at this point. This training would primarily include helping them to minimize *analytical overlay*, the non-psychic, ordinary type associations of images and ideas that would constitute noise that might drown out any psi signal.

Then if you can assess and “calibrate” their individual strengths and weaknesses (this viewer is good on colors, for example, or this one is usually wrong about building size), you can figure out how to combine several remote viewers’ activities for teamwork, to increase the signal-to-noise ratio.

It’s possible there may be some people who can do very well on remote viewing the content and processes of a subject’s mind, experiential content, but not do well on physical targets, but for beginning investigation, it seems likely those successful on physical world targets will be more likely to be successful on experiential targets.

I’ll illustrate possibilities with an example from Schwartz’s archaeological work. He once had a specific kind of archaeological target to find, in Egypt (Schwartz 1978, 1983). But an archaeological dig is very expensive,¹⁶ so you need to narrow down your possible areas enormously. He proceeded by sending to several talented remote viewers large-scale maps covering much of Egypt, along with specifications of the type of archaeological target he wanted found, and asked each viewer, working independently, to draw circles around areas on the map where they thought the target was located. The maps were then mailed back to Schwartz, he lay them on a light table, one on top of the other, to see where the different viewers’ circles overlapped. He then sent more detailed, smaller-scale maps of the overlapping areas back to the viewers, still working independently, and iterated this process until he had a consensus over a small enough area that it was worthwhile to invest in a serious archaeological dig.

I believe there was a selectivity factor of sorts in the early work. When I first began consulting on the SRI remote viewing project, for example, I believe for some, if not many years after that, the primary researchers were happy to ask viewers to try to find out information about the *physical world* for intelligence agencies, but did not want to try to psychically pick up information about the internal states of people, for both methodological and ethical reasons. That is, the targets were all external things. The request was on the order of “Tell me what the outbound experimenter¹⁷ at the target (or an observer who could be at the target, if the outbound experimenter method was not used) sees and senses.” This was also a needed methodological restriction to be sure the outbinder’s mental state, if it were part of the specification of the desired target, did not inadvertently act as a sensory cue for judges to correctly match targets and descriptions. But suppose remote viewing can be practically useful in revealing mental experiences and processes, and you use it with people who are suffering because of malfunctioning of their mind, would like to be cured, *and have given permission for their experiential content and processes to be remote viewed as part of the therapy?*

To begin at a practical level, we need to ascertain that selected remote

viewers¹⁸ can usefully detect and report internal events and processes in designated experimental subjects or therapy clients.¹⁹ I'm tempted to say we need to see if the RV paradigm is good at "telepathy," but most, if not all, past "telepathy" tests involved the procedure of a person, an "agent" or "sender" *actively* trying to "send" selected target information to a percipient or receiver. Whether "telepathy" is an actual mind-to-mind information transfer process (a currently unfashionable view for some parapsychologists), it is an accurate procedural descriptor for one person making efforts, trying to "send" information, while a sensorially shielded percipient tries to ascertain it by some kind of psi. For applying RV to psychotherapy to see if we can at least partially overcome the "inherently private" quality of experience, we can start by seeing how well the remote viewing procedure works with this kind of deliberate intention to "send" information via psi, but our deeper interest is how well a remote viewing team can accurately detect both emotionally neutral and psychodynamically potent information (thoughts, feeling, defense mechanism operations, etc.) that the client doesn't consciously understand or know of, and so doesn't or can't "deliberately" attempt to "send" or normally communicate to his or her therapist via actions or words.

We already have observational information from many case reports that allows us to infer that psi may operate between therapist and client in psychotherapy, reported by psychiatrists such as Ehrenwald and Eisenbud.²⁰ I say infer, for of course there is so much sensorially mediated information between therapist and client that it is often difficult to be certain that apparently psi-acquired information was not sensorially communicated or inferred from sensory contact, in contrast to laboratory studies where you can be almost²¹ certain there was no ordinary, sensory way of knowing the target information.

We could expand Figure 3 to Figure 4. Here we start from having already created a remote viewing team that's good at RVing external target objects or locations, so I've blanked that part of the diagram. Now we want to add in remote viewing of experiential contents and processes.

I'm suggesting here that we find talented *introspectionists*, people who can not only report on qualities of their ongoing experience in detail (hopefully accurately), but who naturally, or as a result of training, can create and stably hold particular kinds of mental experiences.²² People skilled in certain meditation techniques (pick the appropriate kind, there are many forms of "meditation," a word used very loosely by many), would probably be a good place to start. Then ask such an introspectionist, for example, to visualize target material that has been randomly selected, for example experiencing themselves being in a triangular room with a wild animal, a

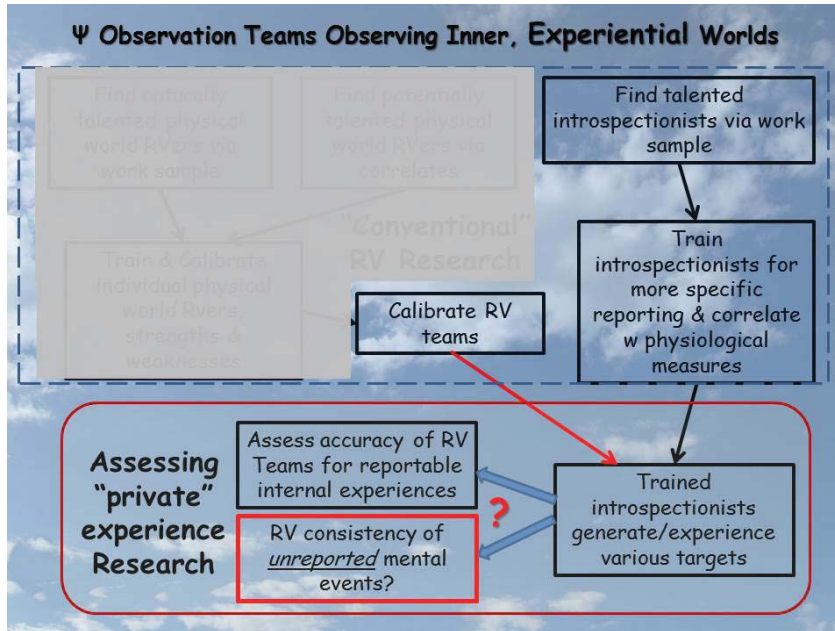


Figure 4. Ψ observation teams observing inner, experiential worlds.

lion for example pacing about, and hold that image stably for N minutes. At the end of that designated target period, have them report how well they accomplished this and what, if anything, interfered with or modified the target image, intrusions, etc. Although it is conceptually difficult to create a “purely mental, internal” event if a record of it is required for later analysis,²³ the introspectionist creating the target experience should not speak aloud about the target or draw a sketch of it.²⁴ If such image targets varied in emotional tone, you might measure physiological correlates during the imaging period: Agitated physiology, for example, would help verify an introspectionist’s success in holding a fearful image as opposed, for example, to a very relaxing one.

Meanwhile, your remote viewing team, who, of course, has had no sensory or other contact with the imager, tries to remote view the target experience at the imaging time, and degree of success is assessed the usual blind judging way.²⁵

What’s accomplished so far is seeing how well the classical remote viewing paradigm works in a task like a classical “telepathy” test. This is generally of little use yet for psychotherapeutic application,²⁶ as this is the sort of content a client can simply tell their therapist. Except that it would be

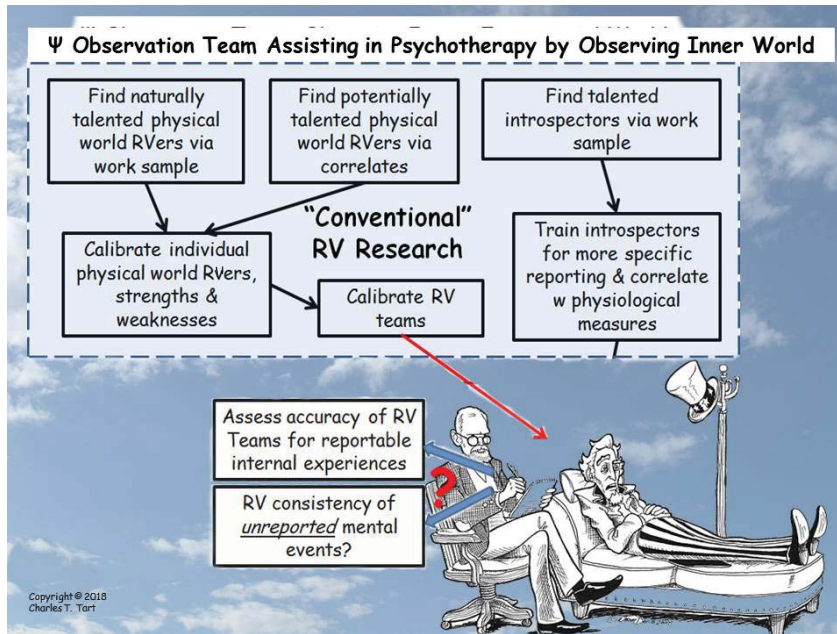


Figure 5. Ψ observation team assisting in psychotherapy by observing inner world.

very interesting if the client reported experiencing A, but a remote viewing team, already known to be generally accurate, reported that the client was mostly or exclusively experiencing B at that time. A hint that strong defense mechanisms were operating? Or non-reported experiential material might be reported by the viewing team which, skillfully combined by the therapist with what the client is consciously reporting, may give hints to psychodynamic processes. This general kind of potential therapeutic application is sketched in Figure 5.

What could be even more interesting in terms of insight therapies, though, is the possibility of the remote viewing team accurately reporting experiential content and process that the client does not or cannot report. One way to assess this is sketched in Figure 5. It does not produce “direct” validation of the remote viewing of unreported experiential content but rather proceeds by having the remote viewing reports given to the therapist, who then, in a manner skillful for the form of psychotherapy being used and the client’s dynamics, incorporates them into his or her therapeutic work with the client. Does the client then progress substantially quicker than the therapist would usually expect with this kind of client with these kinds of

problems? A slow approach, but potentially very useful in therapeutic situations that are not progressing well.

A very interesting question is whether the remote viewing team can pick up important psychodynamic content that is “repressed,” or otherwise unreportable because of psychological defense mechanisms. A rich approach here could involve the largely forgotten²⁷ work of Gerald Blum (Blum 1961), who modeled aspects of psychoanalytic theory using talented hypnotic subjects. Among other things, Blum would suggest to the hypnotized subject, for example, that they had gone through a somewhat traumatic experience while a child, such as having found a purse in a store while shopping with her mother, and rather than telling her mother, so it could be returned to the purse’s owner, had kept it or the money in the purse, but later felt so bad about it that they forgot, indeed repressed, all memory of this incident. Suggestions for amnesia for the hypnotic state were then given and the subject dehypnotized. Later psychological testing in the waking state, such as Rorschach or word association tests, showed reactions to shapes or words associated with something like a stolen purse even though the subject reported no conscious memory of this, in line with Freudian theories.

Ethics Note

A note on ethics is called for here. I want to stress that this proposal is about remote viewing of people *who have given permission* for this to happen. A person with psychological issues, for example a client or patient in the usual terminology, contracts with a professional to help herself or himself understand their psychological situation more clearly in order to relieve suffering or otherwise psychologically grow. The client understands that means the professional will use “normal” (but sometimes non-obvious) means to learn more about unclear psychological aspects of their self. This is done for the client’s benefit, of course, and with a doctor–patient restrictions commitment to confidentiality.

Using a remote viewer or a team of such viewers (who the client will never have any sensory contact with, and who are likewise bound by professional ethics of working to help the client and to maintain confidentiality) is similar to bringing in any ancillary personnel to help the therapist work more effectively with the client. The unusual nature of this ancillary help, though, should be explained to the patient/client and permission received. Indeed the unusualness may stimulate reactions that a skilled therapist could explore quite profitably with a patient/client. “You may have some people I don’t know read my mind at times and tell you about it? Whew, I don’t know . . .” I believe this would be an acceptably ethical way of working. Whether it would stimulate nastily intentioned persons to use a similar pro-

cedure to take advantage of others, and whether such attempts would be effective (I hope not!) is an issue beyond the scope of this current discussion.

“Onward” and “Outward,” “Backward” and “Inward?”

Okay, I’ve taken us, in our thinking, in an “interesting” direction, but one that’s really not too far from ordinary reality, and which might have practical therapeutic applications. Once you can pick up relatively inaccessible experiential and psychological data from others through some kind of remote viewing, of course, it reminds us that there may be many other kinds of data, including data of relevance to physics, chemistry, etc., accessible via remote viewing, but that one example has been enough to show possibilities. Now I’m going to ask you to take a trip that is really “out” there! Or perhaps really “in” there . . . I leave the words “out” and “in” poorly defined, as that takes us into deeper philosophical (and perhaps “spiritual”) waters than I will discuss here.

We might talk about this extension as a psychology beyond, as well as useful within, the ordinary physical world, extending into the “spiritual” world, both checking whether there is any independent reality to a spiritual world and, if so, what its nature is. And/or probably finding that some things considered “spiritual” are delusional.

In the first section of this paper, I’ve focused on the creation of a kind of mental chemistry, to be created by developing the remote viewing paradigm and advanced information synthesis and analysis methods, to look at deeper parts of the mind than what we can normally access. The practical example I proposed was overcoming, at least partially, the assumed inherent privacy of experience, especially when there might be psychodynamic defenses against it becoming conscious. There are major methodological problems, of course, and, as I said earlier, I have been briefly mentioning some of those in the various Notes. Meanwhile, I hope I can persuade you to remain open-minded, not get lost in the more proof, more proof, more proof endless loop of the very existence of psychic ability issues, but to treat psi as an interesting possibility, and see where we might go.

Now let’s move toward a realm of experience we vaguely call “spirit.” My goal is not to naïvely accept spiritual realities as powerful forces beyond our understanding nor to explain spirit²⁸ *away* as nothing but neurophysiological illusions, but to explore spirit more deeply with the essence of scientific method, namely (a) curiosity about what really happens in that vague but important area, (b) learning to observe it more closely, (c) creating theoretical explanations for what we see that (d) can be tested by further observation and experiment, thus hopefully moving toward a more effective and practically applicable understanding of the spiritual. I’ve discussed this

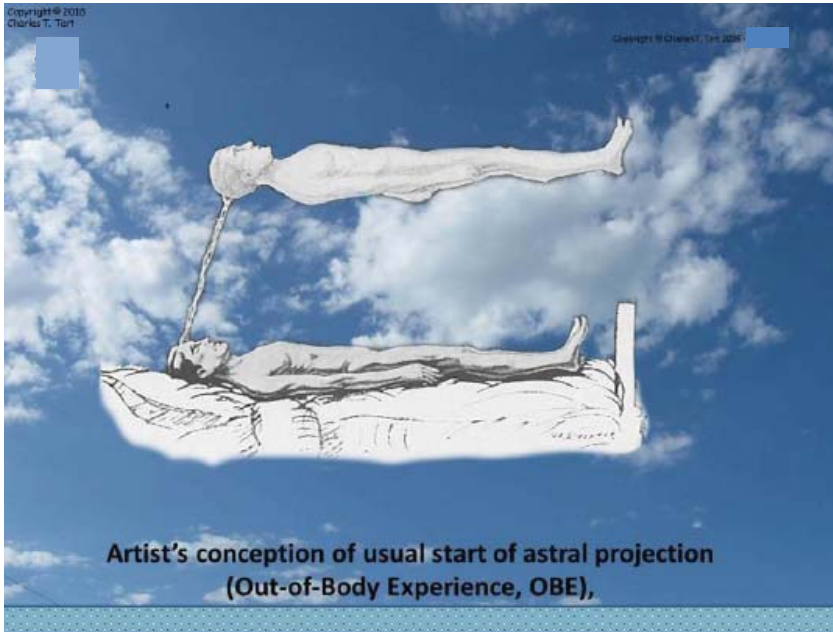


Figure 6. Artist's conception of usual start of astral projection (out-of-body experience, OBE).

methodology as *essential science* in various writings.²⁹ I make no claims, of course, that an essentially scientific approach is the only useful approach to study or validate the spiritual, the transpersonal.

To make this a little more concrete, although “concrete” is a strange word to use here, I’m going to focus on out-of-the-body experiences (OBEs).

My own interest in OBEs was stimulated by reading a book by Silvan Muldoon and Hereward Carrington, *The Projection of the Astral Body* (Muldoon & Carrington 1929). In 1915, Muldoon, then a young man of 12, living in the American Midwest, went to a spiritualist camp meeting with his mother, and spontaneously had an OBE. Figure 6 is an artist’s conception of the way he usually experienced leaving his body. Muldoon usually started an OBE with feelings of paralysis in his physical body, then a feeling of rising up and floating while still paralyzed, then in his “second body” moving away from his physical body until the paralysis disappeared. Then he could go places and do many things just by willing it. Muldoon referred to it as an *astral projection*, a term common in spiritualist literature of the time, but I’ll stick with OBE to make it sound more scientific, as well as OBE being basically descriptive, an *out of the body experience*.

Since I popularized the acronym OBE in parapsychological literature,³⁰ it has often been used rather vaguely and sloppily in much literature, so I'll define what I mean when I use it. You start recognizing an OBE by wondering where you are, and, by some combination of feeling and sensing what's around you, you find you are located outside where you know your physical body "really" is in the ordinary, physical world. To put it another way, the concept of location in the physical world makes sense to you but your perceptual input does not fit your memory of where you believe your physical body to be. It's quite common, for example, for an OBEr to begin with the OBEr suddenly finding themselves floating near the ceiling, looking down, and often seeing a body on the bed, which they then identify as their own physical body. Seeing your own physical body from outside is not invariant for OBEs: Some people may sometimes find themselves out of their body at some distance from their physical body. Usually there are no tactual sensations of the sort that ordinarily accompany sensing one's body.

A second, crucial part of my definition of OBE though, comes from the fact that our culture does not, to put it mildly, accept or welcome this kind of experience, judging it impossible and/or crazy. So one of the first things that typically happens in OBEs is that the experiencer wonders whether she is having a dream, or in some kind of insane state? This may involve reasoning about specific qualities of the experience, but usually it is a much more immediate judgment which you, dear reader, can make right now, and make instantly. Are you awake?

Right now you *could* be dreaming about reading a journal article, but I believe that you wouldn't want to bet money on that being the case. So are you dreaming?³¹ Almost everyone I've asked this kind of question says they instantly perceive that the gestalt feeling of their mind is that of waking.

Could this instant's experience be a lucid dream?³² People who have had repeated OBE experiences usually distinguish OBEs from ordinary or lucid dreams, although brief lucid dreams and brief OBEs may be so similar as to be indistinguishable. Robert Monroe, for example (personal communication 1970s), told me that in a lucid dream he could change the setting or action by an act of will, but in many OBE "realms" he could not, they had laws of their own.

It could be interesting to pause a minute or two and ask yourself how you answer the question, "Are you 'in' your physical body?"

If an OBE is some kind of "dream," then it's certainly not like an ordinary dream. You can reflect on the qualities of your mind and experience during the OBE, including reflecting on the fact that you think it must be impossible, but it's happening in spite of your reasoning, and the experience seems perfectly real to you. Indeed, a lot of times the OBE experience is

described as “more real than ordinary reality.” If you reason about particular aspects of the experience, your ability to reason seems as clear as in ordinary waking, especially when contrasted to the rather fuzzy and sloppy reasoning that occurs in many dreams.³³ The conviction that you were in pretty much your ordinary state of consciousness, even though not “in” your physical body, and that your OBE seemed real, almost always remains for the rest of your life, as contrasted with the fading sense of reality of ordinary dreams.

OBEs and the Soul

Why are OBEs so interesting? Most if not all religions have some kind of concept of a nonphysical part of the self, a “soul,”³⁴ which is separable from the physical body. To a person having an OBE, the *idea* of or *belief* in a “soul” moves from some kind of *belief* or intellectual *concept* to a direct experience. You are outside your physical body, you are quite conscious, your experience feels quite real, and sometimes “realer than ordinary real”;³⁵ you understand what soul is far better than those who merely believe in it.

If this is the first OBE for a person, then, as often happens the first time a person experiences a dramatic altered state of consciousness (ASC) with major changes in perception and thinking, it’s easy to think you are now seeing Truth for the first time, that your perceptions of reality are now far more accurate, perhaps fully accurate, a Revelation of Reality. With repeated experiences, either with OBEs or ASCs, there may be a broader perspective developed that you are experiencing things in a *different* way, and that way may include other aspects of truth, but you are still human, and your human qualities may be affecting the experience.

A common consequence of the realness of an OBE may be a desire to share the good news of the reality of the soul, explain the Truths you’ve seen, perhaps to start or revitalize a religion: I’m sure the founders of some religions had OBEs.³⁶ The NDE, and often a simpler OBE, also often results in a powerful conviction, expressed later in a phrase on the order of “I no longer *believe* that I will survive death: I *know* I’ll survive death! I’ve been alive and conscious outside my physical body, it’s a direct experience of reality for me.” As an outsider, hearing someone else’s account of an OBE, you might argue with this, but this is the impact an OBE or NDE usually has on concepts about death and survival.

How Do We Explain the OBE?

For those wedded to a totally materialist view, where consciousness is *totally reducible*, in principle (but certainly not yet in fact) to the physical

activity of the brain, the OBE is a purely subjective, internal brain construction, like a dream. A computer may be producing a visual image on a screen to a viewer, but that picture is totally and ultimately reducible to particular electron movements in silicon transistors. The image has no existence without the action of the computer.

In my developing MINDS (Mutually Interacting Neuro-Dualistic Systems) approach,³⁷ which includes material realities as well as possible mental or spiritual ones interacting with the material ones, an OBE can be pictured as one kind of world simulation process,³⁸ a biological psychological virtual reality (BPVR). In this approach ordinary consciousness is envisioned as a BPVR, but one constantly modified by incoming sensory information, so the virtual world you experience (and take for “reality”) adequately mirrors the physical world around you. Ordinary dreams can be seen as a BPVR where the world simulation process is almost totally “free running,” it has little or no sensory input to modify or limit its construction of an apparent reality. From a totally materialistic perspective (not the same as the MINDS approach), the OBE may be associated with more cortical arousal, so it feels more real, but it’s misidentified as being actually real by the experimenter. I believe any completely materialistic theory is incorrect when thought of as a complete explanation, but it’s useful to guide research on mind–body interaction. And my opinion is subject to eventual empirical testing, of course.

As noted above though, in the MINDS approach material reality is not necessarily considered as the only basis which experience can be reduced to or constructed from, there may be mental or spiritual realities (psi, for example) which are also part of the fundamentals from which the systems emergents, various states of consciousness, emerge. Consciousness at any time, in any state, can be usefully seen as the gestalt emergent of various interacting “basics.”

A second theoretical approach is that the OBE is pretty much what it seems to be; in ordinary physical space–time terms, some aspect of your consciousness is indeed temporarily “located” “outside” your body. I use “outside” here to describe important characteristics calling for investigation on their own terms, not necessarily to ultimately imply ordinary physical “spatial” separation. My study of the feasibility of studying OBEs under laboratory conditions with a Miss Z years ago³⁹ centered around this very question for her: Were her OBEs just special dream-like states, but basically imaginary, or was she, her mind, really floating above her physical body? Note that I only claimed this study demonstrated the feasibility of more precise laboratory studies of OBEs rather than a total dependence on reports of spontaneous experiences, and reported some interesting and suggestive

observations, but for Miss Z, correctly identifying a 5-digit number up on a shelf showed her that she was “really” up and out in some real spatial sense.⁴⁰

You can see then that the experience of OBE, of *being* something like a soul, will be far more supportive of religious beliefs than if it’s merely an abstract concept. I believe a phenomenon like OBEs (or NDEs) that seems to support many religious and spiritual beliefs deserves intensive investigation, not a prejudicial dismissal.

For many years my ideas about OBEs remained intellectual, derived from reading many spontaneous case reports as well as the classic Muldoon and Carrington book (Muldoon & Carrington 1929). I’ve never had an OBE myself, although I once had what I considered a vivid simulation of an OBE as a result of a psychedelic drug experience.

My early interest in OBEs and related phenomena led me to conduct an experiment in trying to use hypnosis to teach some other students to have an OBE in hypnosis, while I was still an engineering student at MIT.⁴¹ They reported interesting subjective experiences, but the psi test part of the study did not clearly indicate the appearance of psi. In retrospect, I saw that the experimental design wouldn’t have been sensitive to a qualitative analysis for psi anyway unless it had been massive in size.

Some years later, when I spent a year as a researcher at the University of Virginia in Charlottesville, I met and befriended Robert A. Monroe, who was to become, a year or so later, the author of the now classic book *Journeys Out of the Body* (Monroe 1971). For the benefit of younger researchers who may not be familiar with Monroe’s writings, let me briefly describe his experiences, and then introduce a specific type of his OBEs that could serve as a model for using remote viewing as a tool to explore the “spiritual” side of human nature in a more scientific (and possibly more effective in improving people’s lives) manner than simple belief or disbelief.

Robert Monroe

Monroe (1915–1995) was a son of a college professor father and a doctor mother, and lived as a normal person for most of his life. He became a successful producer of radio shows in New York City, and later a successful businessman, operating a cable television network when he moved to Charlottesville, where I met him in the fall of 1965. I was probably the first scientist who knew anything about OBEs who had befriended Monroe. I quickly recognized him as a sensible, honest person, and our friendship continued up through the time of his death. I found his reports of his OBEs especially valuable, as they just started happening to him for no reason that he understood, and he had no previous background knowledge of OBEs,

thus no strong expectations as to what an OBE was supposed to be like. Given our abilities as humans to shape our experienced world, constructed in the BPVR process, I give more credence to reports of unusual experiences where there is not an expectational background than from people who have been schooled or indoctrinated in what they are supposed to experience.

Late in his life Monroe founded an Institute (<https://www.monroeinstitute.org/>) that offers classes on how to have all sorts of ASCs and similar experiences, possibly including OBEs. I visited him several times when such classes were in session, when he spoke informally with students. It was interesting to see the students often trying to put him in the position of some kind of spiritual guru, a man full of occult wisdom, and I was impressed by Monroe's denial and defenses, usually carried out with pleasant humor, against allowing this kind of projection. He was describing as honestly and accurately as he could what had happened to him in his OBEs and sharing some techniques he thought might help others to have them so they could check for themselves whether there was anything to OBEs, but not dispensing transcendental wisdom.

Monroe was quite puzzled by his OBEs from their beginning, and very much wanted to understand them. I treated him as talented, not crazy, and our long-term friendship included many common interests. Monroe felt isolated in having OBEs, as, for most of his life, he didn't know anyone else who had experienced anything like that. He was socially normal, active in his community, running his business, playing poker regularly, and the like, but wondering about the meaning of his OBEs continually. What was happening, what did it mean? One version of his questioning was a "Why me?" Why had this strange stuff started happening to him?

For a long time Monroe was loath to publish any accounts of his OBEs or to talk to other people about them, as he suspected he would be ostracized in the conservative Virginia community he lived in, and, indeed, that happened at times, and may have been one of the reasons for the breakup of his first marriage, as his first wife was greatly bothered by his experiences.

As I noted above, Monroe attempted to describe what happened in his OBEs as accurately as possible. Some of it, though, was what I long ago named *state-specific knowledge*, it was something that made sense to him when he was in a particular OBE condition, but it simply didn't make sense later in his ordinary state of constant consciousness (Tart 1972b).

By 1964 Monroe had finally written a book-length description of his experiences, later published as *Journeys Out of the Body* (Monroe 1971). He'd placed the manuscript with a literary agent in New York, but when I met him 1965 he hadn't heard a single thing from that agent after a whole

year. We guessed that his agent was too disturbed by or unbelieving of the material in the manuscript to try to market it! I asked Monroe for a copy of the manuscript and sent it to the editor who had overseen the paperback edition of my *Altered States of Consciousness* book (Tart 1969), Bill Whitehead. Whitehead was not very interested in weird stuff, but he took my word that *Journeys* would be interesting to some people, so he took the manuscript home to read.

He later told me that he started reading it after dinner and, fascinated, couldn't put the manuscript down until after 3 o'clock in the morning, when he came to the "how to do it" chapter. Afraid he might have an OBE, he managed to put down the manuscript! I later wrote an introduction to the *Journeys* book. Monroe later wrote two other books on aspects of his OBEs (Monroe 1994, 1985), and hundreds of thousands of copies have been sold worldwide. There are translations in 25 countries, and still 7,000 to 10,000 annual sales almost 50 years after the original book was published.

From my primary perspective as a transpersonal psychologist, concerned with people's welfare as well as research, I think Monroe's books, especially the original *Journeys* book, were a great gift to thousands of people who have had their own OBEs but worried that they were crazy, and who couldn't talk to anyone about them. After reading Monroe's sensible accounts of his own experiences, they felt much better about themselves! They weren't crazy!

Monroe's First OBE

Monroe's first OBE occurred in the spring of 1958. He had fallen asleep normally, thinking about going gliding the next day, a long-time hobby of his. He woke up with a feeling that something was poking him in his back. As he was not a sleepwalker, he assumed that he must have sleepwalked, and he was now standing with his back pressing against something on the wall. He opened his eyes and looked around and found that he was looking at a light fixture—in the middle of a wall! "Who in the world would put a light fixture in the middle of a wall?" he thought, for a moment . . . until he realized he was looking at the light fixture on his bedroom ceiling, as he floated in the air! Turning around and looking down, he saw his wife sleeping in bed, and a strange man beside her! As he looked more closely, he realized the strange man was himself . . .

This kind of thing—he had no name like OBE for it—happened several more times in the following months, so he did the "normal" thing for our culture: He saw his doctor to see what was wrong with him. His doctor did a physical and told Monroe there was nothing physically wrong with him. Eventually a friend told him that he might be having something called

an “astral projection,” which yogis in India were supposed to have, and it might be interesting if he took notes on them if he was going to continue to have them.

As with most people who have an OBE, one of Monroe’s major concerns was whether they were *real*. Were they what they seemed to be, where he was actually out of his body? Or something else? Finally he had several OBEs over a course of months in which he went to distant places and correctly perceived what was happening there, things that he couldn’t have inferred, and he became convinced that he wasn’t crazy or just imagining things, he did indeed seem to be leaving his body. Several OBEs with verification are reported in *Journeys*.

Somewhat differently from Sylvan Muldoon’s classic account of his OBEs (Muldoon & Carrington 1929), which began with a feeling of paralysis, the initial experience that would lead to a full OBE for Monroe was a feeling of vibrations in his body. There was some variety in the way he experienced getting out of his body, sometimes floating up and out as Muldoon reported, sometimes rolling over, sometimes he wasn’t quite sure what the process was. He did not experience a “silver cord” that some people who have OBEs have reported, so it may be that the silver cord is a psychological construct as mentioned in the Bible.

Over the years Monroe had hundreds of OBEs. Where did he go? He roughly divided the descriptions of his OBE journeys into three locales.

Locale I: Locale I was in various places on Earth. Sometimes these were places he recognized, but seldom was something unusual enough happening that if he later checked on it he could convince himself there was a sense in which he had *really* been there. He once remarked to me that most of his experiences of visits to Locale I were basically finding himself on some unknown street corner in an identified city at four in the morning, with nobody around, and after half a minute the experience was over. No kind of verification was possible, but, as I mentioned above, there were enough cases over the years in which he did acquire information that was verifiable about distant events to convince him that at least some of his OBEs were real, he was really perceiving the world from a different physical place than where his physical body was located.

Locale II: This range of places was what Monroe would later call “heaven realms,” belief realms, or “spiritual realms,” where he might encounter the deceased, unusual and non-human beings, etc. Much of his experience there was state-specific and I’m going to skip Locale II experiences entirely to keep things simpler in this paper.

Locale III: He also visited a place several times that he called Locale III. It looked enough like our physical world to make him wonder whether

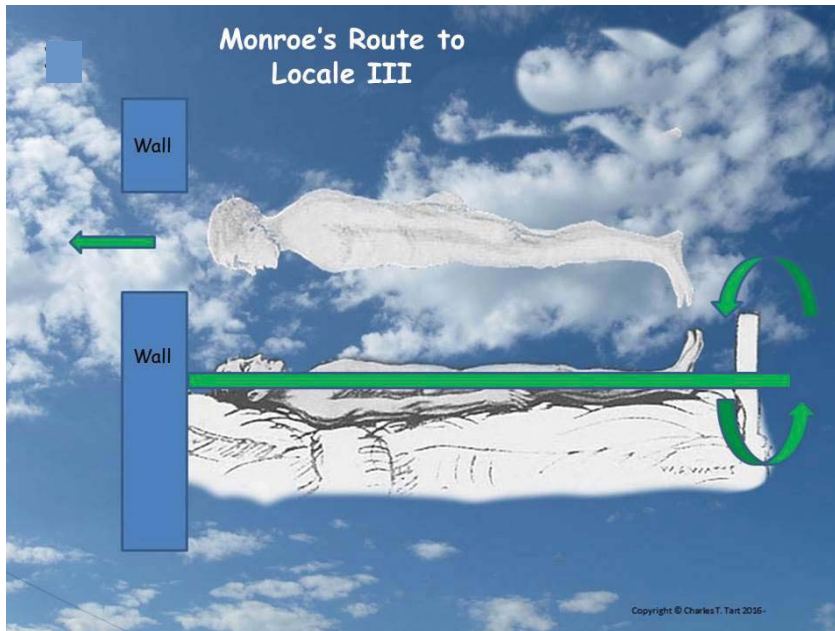


Figure 7. Monroe's route to Locale III.

it was a physical world somewhere in our universe. He described it in some detail before saying much about the Locale II areas in his *Journeys* book, in order to pique people's curiosity as something "far out," but not unimaginably different. If he was going to other places in the physical universe that were real, that could potentially be verified, we would have to develop, to use the common science fiction term, an FTL, Faster Than Light drive, for our spaceships.

His first visit to Locale III occurred one night when Monroe was having difficulty getting out of his body. The vibrations had come but gone and he didn't seem to be able to move out, so he tried something different. He did things that increased the vibration feeling, and then rotated 180° along the long axis of his physical body, so that his "second body" was in a prone position.⁴² This had never happened before. He then reached out with his hands over his head, just curious, and felt like there was a solid wall there, except there was a hole in the wall, just big enough for him to go through, so he pulled himself through. This is symbolically sketched in Figure 7.

What was Locale III like? Or, if it has an independent reality as Monroe was tempted to postulate, what *is* it like? First, it was not like an ordinary dream or like his occasional lucid dreams (van Eden 1913, reprinted

in Tart 1969:145–158). Monroe makes this distinction of OBEs from his lucid dreams because although his mind feels very much like it's functioning in ordinary consciousness in lucid dreams, as in most of his OBEs, he, like most lucid dreamers, can will dream objects or processes to change and generally have success in a lucid dream. But in Locale III he could not make any changes in Locale III by simple willing it. He had to discover what the inherent rules for action were there, and abide by them.

Locale III was like our ordinary physical world in most ways. There were trees, houses, cities, artifacts, machinery. People looked like ordinary people, seemed to live in homes and have families, went to work, and ran businesses. There were roads and vehicles to travel on them, and it was so similar that Monroe was at first tempted to think he was having an OBE to some relatively unknown part of our earth. But, Locale III was not like our physical world in quite significant ways.

There were no electrical devices at all, for example, no lights, telephones, radios, TV, or any other examples of electrical power. There were devices using mechanical power, but they didn't look like internal combustion engines. Locale III had railroads, and once he got to inspect a train locomotive that was being refueled. It looked like it was powered by some kind of steam engine, and it was sitting on a track with a width that look significantly smaller than even our narrow-gauge railways. The refueling processing seemed to be a matter of taking vat-like containers from under the locomotive's boiler, putting them on hand trucks and taking them into a heavily walled building, then taking similar containers, presumably fresh "fuel," out from the building and putting them back under the locomotive's boiler. The people doing this seemed to be taking considerable precautions in the way they handled these containers, as if either they were thermally very hot or as if they were radioactive. From what little I know of physics, I think it's actually technically feasible to have relatively portable heat sources made from radioactive materials that would work to power a boiler, although it would take some advances in shielding technology to make them practical and safe.

They had roads in Locale III, and the roads were twice as wide as ours are, because the automobile-like vehicles were much wider. Even the smallest one had a bench seat that could seat five people abreast. Only the driver's seat was fixed, and the other seats were movable within a roughly rectangular space. The vehicles seemed to move at a relatively slow speed, Monroe estimated at 15 to 20 mph (Monroe had raced cars when he was younger, so was knowledgeable on mechanical things). There were no traffic lights, no inflated tires, and steering seemed to be done with a horizontal bar rather than a steering wheel.

As I said, Monroe had been a racecar driver and an airplane pilot, and he knew that this extra wide design was a stupid thing to do in engineering terms. You've got more air resistance, you needed roads that were much wider, such vehicles would have problems in differential gearing allowing the outside wheel to turn faster than the inside wheels, etc.

The "people" Monroe encountered in Locale III did not seem aware of Monroe in his initial OBEs there until he started, as he tries to describe it, "merging" with a particular person there. After several merged visits, though, he realized that while he could sense things through that person, he was creating trouble for that person, his friends wondering at his occasional ignorant and stupid behavior and inappropriate responses, so he stopped OBEing to Locale III.

It's hard to seriously consider that what Monroe was experiencing was literally true, that he was visiting in an independent reality. Hard because of the success of our dominant physical belief systems and extrapolations from them: We haven't explored any planet much but Earth. But, it's *interesting* to wonder, suppose Monroe was "there?" Perhaps there is an independently real Locale III?

I'm going to now describe a proposed hypothetical study to check the "reality" of Locale III and to pilot extending our use of the remote viewing procedure considerably further toward investigating the "non-physical," or perhaps the "spiritual."

Is Locale III "Real?"

Over the years in my studies of ordinary, "normal," as well as obviously altered states of consciousness, ASCs, the model that has gradually become most generally useful to me is that we live in a Biological Psychological Virtual Reality (BPVR), analogous to Computer Generated Virtual Realities (CGVRs).⁴³ We may implicitly or explicitly believe we are perceiving the world around us and ourselves as directly as possible, but we now believe our apparently "direct" perception is of a semi-arbitrary, neural/mental construction.⁴⁴ The clearest example of the BPVR process constructing an experienced reality, known to almost everyone, is nocturnal dreaming, when various aspects of our minds and brains (you can collapse that to a monistic basis for reality if you're not happy with any kind of dualism) create a world model with space and time in it, and add scenery, characters, and storylines as well as a sense of self. During the time we are "in" a dream, we almost never (with the exception of lucid dreams) question the reality of what's happening to us at the moment, we don't know we're dreaming. When we shift our state and return to our ordinary waking state, we may talk about the strangeness of the dream story or find detailed differences from ordinary

consciousness—I've noticed, for example, that the experienced "visual" detail in my dreams is generally not as great as it is in waking—and then, as is normal for our culture, we tend to subrate and dismiss our experience as *just a dream*, and make no effort to remember it, so it's gone.

I model ordinary, "normal" waking consciousness as basically the same sort of BPVR as a nocturnal dream, probably with many specific creation mechanisms and processes in common with the two states of consciousness, but while awake there is a massive amount of sensory information coming in. Much of it is about our bodies, what they feel like, position and movement of our limbs, etc., and often even more as to what is sensed by our exteroceptors that tell us about the physical reality around us at any time. Thus the BPVR process in waking has to constantly and rapidly adapt its creations to be consistent with sensory, interoceptive, and exteroceptive information coming in, otherwise we will walk into walls or off the edges of cliffs, etc., and not survive.

We Live in Virtual Reality

From my MINDS theoretical perspective, *we exist in a virtual reality all of the time*.⁴⁵ The difference between different virtual realities, different states of consciousness, then, has a lot to do with what kind of "external" reality we are in and how well information about that environment is integrated into the ongoing BPVR world simulation—the virtual world that we are existing in and taking for real at the time it's happening, as well as culturally and personally learned and conditioned (and almost always implicit) rules for reality construction, and the condition of our body.

Insofar as you believe in a total materialism, then only our BPVR of the physical world is a simulation of a *real* world, and while other simulations may *feel* real as we experience them, they're basically all fantasies, subrated like dreams. This leads us to an immediate question with regard to Monroe's Locale III: Although it *feels* like an external "physical" reality to him, and passes his own test for whether a reality is independently real or more a matter of thoughts and beliefs, is it one? Or is it just a fantasy that is more stable than typical dreams? Or, to avoid an implicit assumption that Locale III can only be real if it duplicates everything in our physical reality, does Locale III have an independent reality in that it exists "somewhere/sometime" regardless of whether Monroe or anyone else is experiencing it, and it has a lawful structure?

Perhaps another way of thinking about the degree of reality of Locale III is that it might be experientially real for Monroe (and perhaps some other people) in the sense that it's a set of genetically coded instructions in the human brain, and, under appropriate conditions, an experience of being in

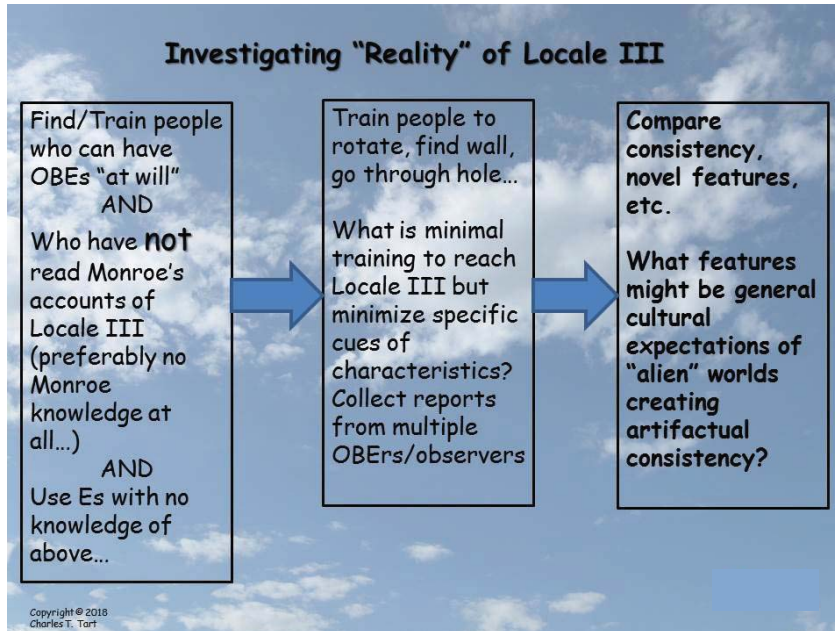


Figure 8. Investigating the reality of Locale III.

Locale III, with its inherent characteristics, will be created. But if all human beings who have that particular inborn genetic program have died before passing on their appropriate genes, Locale III will never exist again. I will not pursue this particular line further here, but it's an interesting one.⁴⁶

A further puzzling question is why genes to create a Locale III should evolve from evolutionary processes at all. What kind of survival advantage would they produce? In my early career doing hypnosis research, for example, two highly talented hypnotic subjects, who were experiencing what I called *mutual hypnosis*, jointly created a very real and psychologically potent reality as they verbally suggested things to each other,⁴⁷ but my bias was certainly to call it a transitory fantasy . . . but perhaps somewhere there is a "non-physical" realm like what they experienced?⁴⁸

Hypothetical Study on the "Reality" of Locale III

As shown in Figure 8, to begin investigating the degree of "independent reality" of a place/experience like Locale III, you could start by finding people who are naturally talented at having OBEs, or people who have characteristics such that they can be trained to have OBEs pretty much at will.

I say “pretty much,” as experimental time is costly, and someone who can have an OBE more than once a month is not likely to have it at the time and in the manner you desire. But, even more importantly, the selection process must work to assure that these are people who have *not* read Monroe’s books, and otherwise do not know anything about Monroe’s accounts of his visits to Locale III. Ideally they should have no knowledge of Monroe’s experiences at all, thus allowing a wider variety of OBE experience qualities to be checked than just the details of Locale III. Sadly for fans of Robert Monroe’s books, or for people who have taken courses at his Monroe Institute, this pretty much excludes anyone who is trained in Monroe’s methods. Further, since I’m very sensitive to the problem of experimenter bias, you should use experimenters and laboratory staff who also have no knowledge of the qualities of Locale III. The basic investigation parameter is sketched out in Figure 8.

Having found some people who can have OBEs reasonably at will, you then train them to have an OBE like Monroe reported, starting by feeling “vibrations” in the body. Specifically, for Locale III, once an individual’s vibrations have started, they should learn how to mentally rotate along the long axis of their body, find a wall just above their head with a hole in it, and go through the hole. This raises interesting questions: If Locale III is independently real, what’s the minimal training required to have someone experience Locale III, but a training that minimizes and ideally eliminates any specific suggestions of the expected characteristics of the locale once the OBEr is there?

Some aspects of such a non-biasing training are straightforward. I can’t imagine, for example, why feeling out of your body and going through a hole in a wall would suggest a world in which the cars were twice as wide as cars in our world! So that part of the “travel instructions” doesn’t seem biasing. But after self-initiated reports of OBEs in this attempt are gotten, investigators would have to be very careful about the kinds of followup questions they asked, as the wording of the questions might suggest the features whose independent reality we are trying to ascertain. And with experimenters who are also blind to Monroe’s experiences, how would you train them not to suggest things that produced artifactual similarities?

It’s relevant to note the finding among experimenters and taskers who’ve used remote viewing in intelligence missions that, in general, the less leading information given about the target in a given session, the more likely you are to get relevant, psi-acquired information. Specifying a target by arbitrary spatial coordinates, for example,⁴⁹ is unlikely to lead to artifactual specificity with almost anyone, other than gross features; like with a certain range of latitude and longitude coordinates, that coordinate must

be in an ocean, so water or an island is likely. In many successful intelligence applications of remote viewing, a quite non-specific request, such as "Describe today's target please" works quite well. How the remote viewer knows what target is desired is an interesting question.

You then go on to collect reports from multiple OBE years/observers reporting that they go through a hole in a wall, and see how much they parallel what Monroe has reported about Locale III.

Possible Outcomes of Locale III Studies

Suppose we had had a reasonable number of trained OBEs or remote viewers who reported they had experienced going through the "hole in the wall," and who gave us detailed reports of ensuing experiences. What might some of our findings be?

Perhaps the most interesting and startling outcome would be to find that independent OBEs/remote viewers/explorers showed strong consistency with what Monroe reported for Locale III, and we could not reasonably attribute it to common cultural expectations. What kind of interesting research would be stimulated with a working hypothesis that Locale III seemed to be independently real? Is it a physically real place somewhere in the physical galaxy? Or "independently real," but not a part of any practically knowable physical reality? Or perhaps showing apparently physical characteristics, but of values that our general picture of the physical world deemed impossible? What would "independently real" mean in that case? Perhaps a part of a real "spiritual" or "psychic" or "non-physical reality?" How would you tell the differences among these three categories? Admittedly those three terms are vague and associated with much noise, emotional as well as intellectual, at this stage of our knowledge, but interesting to think about . . .

A relatively contrary outcome to the above might be that none of our explorers describe anything that shows particular consistency with what Monroe reported, especially the notably unique features he attributed to Locale III. We then would probably conclude, at least within the limits of the number and talents of the explorers we used and the adequacy of our induction procedure for getting to Locale III, that it existed in some form, perhaps only in imagination, only as a BPVR, only for Robert Monroe. Why his mind or brain created Locale III would be a fascinating psychological problem, but one not likely to yield to investigation, since Monroe is long deceased. But other people might have similar creations, and understanding them would advance general psychological knowledge.

We might also find significant relative consistency among our explorers, in spite of our minimizing expectational cues in the OBE induction process, and would want to ask how much of this might be due to gen-

eral cultural expectations about what alien worlds would be like. If reports of people having NDEs in American culture were very much in line with Biblical ideas (they aren't), for example, we might be inclined to attribute this to the widespread knowledge of Jewish and Christian ideas in our culture, even in people who were not formally religious. For NDEs, the fact that NDErs with highly different beliefs and cultural backgrounds reported striking similarities is what made NDEs much more interesting to study and in their implications than if we could easily dismiss them as just fantasies of a dying brain.

Another noise and biasing problem, common to many psychological studies, is that simply putting people through the training and collecting of their OBE or remote viewing reports constitutes a suggestion that we at least expect them to find *something* interesting, and probably more interesting and surprisingly different from just descriptions that would apply to the ordinary world.⁵⁰ To investigate this further, we have to carry out extensive studies into what cultural expectations our explorers have and the degree to which they could shape the particular state(s?) of consciousness necessary for experiencing something like Locale III.

Then there might also be interesting investigations of what the evolutionary significance of phenomena like non-physical locales were. If, for example, you theorized that OBEs and NDEs were merely wish-fulfillment fantasies of some sort, a kind of psychological reassurance that even though we're going to physically die, we might go to some kind of heaven, why has OBE and NDE content evolved that way? I've always been puzzled, for example, by the fact that most NDEs, which usually have an OBE component, are *extremely* positive to experiencers! Insofar as evolution "advances" by having survival-enhancing traits passed on, from an evolution theory perspective you would expect NDEs to have evolved to be horrible in order to make near-death survivors much more careful about not risking their lives and so being able to pass on their genes, not a wonderful experience that can make dying a wonderful idea, encouraging more risky behaviors . . .

And, coming back to Locale III specifically, why odd features like cars that are twice as wide as known cars, or no electrical devices?

Or we might find enough inconsistency to doubt that Locale III has any kind of independent reality, but the results are interesting enough, and a useful pilot on how to investigate other "non-physical" realities. These would especially include ones people have talked about that have religious and spiritual significance for people, such as heaven realms and hell realms, bardos, and afterlife states. Monroe, for example, reports visiting a variety of sort-of-seemingly-solid-and-real *belief realms*, which he called Locale IIs. He thought these were created and maintained because the surviving

souls of believers in some particular religion gather together and reinforce each others' and their own faith in the correctness of their religious beliefs. There's a kind of semi-independent reality to them, yet they are also strongly shaped by the entities there. Given the motivating power of belief in such spiritual realities, I don't think it's sufficient to simply be totally materialistic and say it's all nonsense and let's ignore it or call it pathology, and try to make people more rational. These kinds of experiences keep happening to people, and we need to investigate their qualities to find out more about what their nature is.

Conclusions/Summary

Obstacles

I've sketched a number of ideas for creating a relatively scientific psychology of the mind, which could also put the *psyche*, the mind, back into a central position in psychology (and perhaps other fields of science). I've touched lightly on methodological challenges, mainly mentioning them in Notes, not wanting to overly interrupt a flow that I hope will inspire others to push our knowledge forward.

Basically, I think most of the problems that undermined an earlier attempt to create a science of *mind*, a mental chemistry in the past,⁵¹ can be significantly *reduced* in power. I'm tempted to say overcome, but I accept the fact that we are human and our nature means we undoubtedly have certain kinds of biases built in.⁵² I do think we can understand those biases more and reduce their distorting effects as we try to get a clearer view of the ultimate nature of reality, but it may only be a more probabilistically true view of reality, rather than a certainty. And, of course, we need all the help we can get from various other kinds of disciplines to help us in this quest!

Notes

¹ I use Notes to insert references to more detail, methodological points, and suggestions for deeper exploration, but, as I explain later in the text, the important overall message of this essay can best be grasped, with, I hope, enthusiasm, by saving these Notes for more detailed reading later. At this specific point, I simply refer to my most recent and comprehensive book on psi and consciousness (*The End of Materialism: How Evidence of the Paranormal Is Bringing Science and Spirit Together*) (Tart 2009) where I have indicated a number of reasons psi is important for any intelligent person's worldview. The hardcover version is now out of print, but it's available in paperback as *The Secret Science of the Soul: How Evidence of the Paranormal Is Bringing Science and Spirit Together* (Tart 2017). I

- expect the High Priests of the Church of Absolute Materialism to show up any day to take my white lab coat back for daring to use a word like soul . . . ;-)
- ² Compulsive scholars like me will find it hard not to read these references, but remember, it's just an attempt at humor here . . .
 - ³ (Bierman & Rayberon 2013).
 - ⁴ The title of the Parapsychological Association talk was Parapsychology as an Essential Component of an Expanded Science of Mind: Promises and Challenges.
https://www.parapsych.org/media/player.ashx?id=xY_g8UBiu0E and
<https://www.parapsych.org/media/player.ashx?id=6e1lddiFT4E>
 - ⁵ I've discussed the shortcomings of this position, and, indeed, of any absolutist "Everything will completely reduce to X," in many articles over the years (Tart 1972a, 1975a,b, 1976, 1977a, 1978, 1979, 1981, 1987, 1990, 1992, 1993, 1996, 1997, 1998a,c, 2002, 2004).
 - ⁶ Promissory materialism is not science, for it does not meet a most basic criterion of what constitutes science, namely that theoretical explanations can be falsified by data. There is no way you can ever disprove the premise that "Someday" your theory of the efficacy of (fill in your favorite explanatory mechanism here) can be tested if it does not make observable predictions about data.
 - ⁷ I am not a very skilled meditator or introspector, but even my basic level of practice often demonstrates that as one learns to observe ongoing mental experience with gentle concentration, non-interfering, and equanimity, an experience such as an emotional feeling often reveals itself as a compound of two or more emotions. My own writings (Tart 1986, 1994, 2001) give useful instructions for a basic level of vipassana (insight meditation), and more advanced but highly practical instructions can be found from sources like Shinzen Young's writings (Young 2005, 2016).
 - ⁸ I've written about these problems with the mental chemistry attempt elsewhere (primarily Tart 2005).
 - ⁹ At least the kind of person who was associated with an institution of higher learning, as they have class and status differences, too.
 - ¹⁰ Sometimes I've tried to describe my own experience when something interesting happens and I see the many difficulties. To begin with, it's flowing and changing rapidly, I can't possibly talk or write fast enough to accurately and comprehensively describe it, and many aspects don't have words that are accurate in the first place. How about sampling, I wonder, suppose I tried to be more thorough with every fifth or tenth experience? Get enough of those and you would get a reasonable sample of what the flow of experience is like? Develop a special language to describe experi-

ence? Some of the meditative traditions have tried. But language is tricky, I notice at times that in my desire to adequately voice an experience I'm letting the meaning of the words I'm using predominate and that's not quite what the experience is, etc. But many fields of science have become quite good at data description, and what would happen if we gave a lot of attention to it instead of blocking ourselves in advance by believing that experience is inherently difficult to describe? One of my favorite sayings, attributed to Henry Ford, is that those who think they can and those who think they can't are both right . . . I suspect there are actually a lot of specialized vocabularies to describe aspects of experience if we began looking for them . . .

- ¹¹ Even though I've argued that the apparent objectification of laboratory procedure is often misleading, the human relationships between Experimenter and "subjects" can be vitally important (Tart 1964, 1977b, 1980, 1984, 2010, Troffer & Tart 1964, Hilgard & Tart 1966).
- ¹² As an example, one of the reasons that, by parapsychological standards, an enormous amount of money was spent on *remote viewing* over twenty some years (May & Marwaha 2018) was that *remote sensing* was a widely used and fashionable engineering research area during that time, making what was done as remote viewing much more scientific and acceptable sounding than if it had been called *telepathy* or *clairvoyance*.
- ¹³ One of the chief investigators of remote viewing, Edwin May (May, personal communication, 2018), pointed out that from 1973 (the Stanford Research Institute program's beginning) through 1995 (22 years) there were 504 intelligence gathering missions requested by 19 different USA intelligence and military organizations. Of these 19 agencies, 17 were satisfied enough with initial results that they returned with new missions. One joint task force came back 172 times alone. There would have been much more use of remote viewing for intelligence operations except for political factors, powerful government people mindlessly dismissing it or calling it the work of the devil and trying to stop it. As to archaeological applications, I highly recommend Schwartz's reports (Schwartz 1978, 1983, Schwartz & De Mattei 1988, 1989). Having hiked various deserts that, to me, were featureless, I've always wondered how in the world archaeologists knew where to dig when there are no surface ruins or other signs. Fascinating as they are, it would take up too much space here to give examples of how remote viewers in Schwartz's studies found buried ruins that had been lost for hundreds, if not thousands, of years.
- ¹⁴ Various factors that may correlate with remote viewing skill have been tested—for extensive documentation see May and Marwaha (2018)—but my impression is that previous success at remote viewing is still the best

screening procedure. Correlations that may be statistically significant in testing groups of people may not be practically useful in smaller studies. A parallel was in the research on the nature and uses of hypnosis that I was involved in early in my career. The best predictor of hypnotizability was a work sample: Try a hypnosis induction and suggestibility test procedures on a person and see how well they did. Hypnotizability turned out to be a relatively stable personality characteristic, although it could be increased under some conditions (Tart 1970). I don't know if that is true for remote viewing ability. Certainly it would be useful to find correlates with a strong enough relationship for practical screening if projects involving training large numbers of remote viewers were undertaken.

- ¹⁵ The procedures developed in the SRI work and Schwartz's work are state-of-the-art. Many variations developed by others as remote viewing became fashionable in popular culture are untested or questionable.
- ¹⁶ There can be a kind of dark humor here, as Schwartz reports that sometimes the bureaucratic hurdles needed to be able to dig seemed far worse to overcome than the actual cost of digging.
- ¹⁷ An experimenter, the "outbound experimenter" or "outbounder," went to the target site in the early experiments, as it seemed common sense that having someone the viewer knew at a target site would make it easier to access psychically. Sometimes the outbounder was referred to as the *beacon* person. Later studies without outbounders generally seemed just as successful though, although I'm not sure there's been a formal comparison of this.
- ¹⁸ Here and throughout the paper I remind the reader that when I write "remote viewer" I usually mean a person using the procedures developed at SRI and by Schwartz, containing such essential elements as (a) all of the research team accepting that remote viewing can work well and hoping it does so, (b) the viewer being completely blind to the target, (c) a skilled interviewer to help the viewer elaborate and clarify his or her impressions, and (d) blind judging by otherwise skilled judges to detect matches. Whether other procedures to elicit psi information will be useful is a question for other empirical research.
- ¹⁹ We could talk about the *target* person's mind, but I don't like the associations of a person being a target, especially in a therapy context.
- ²⁰ Some examples of the interaction of psi and psychotherapy can be found in the writings of two of the pioneers, Jan Ehrenwald and Jule Eisenbud (Ehrenwald 1971, 1977, 1986, Eisenbud 1970, 1983).
- ²¹ Given the usual rigor of formal parapsychological experiments, why would I preface "almost" to "certain?" It's my temperament—I distrust absolute ideas like "certain." As my colleague David Hufford observed,

“What I have always told my graduate students is that *certainty* is a great direction in which to head, but it is a disastrous place to believe you have arrived” (2014 personal communication),

²² Classical “telepathy” procedure experiments instructed a person, designated the “sender” or “agent,” to try to send the target information, but usually we have no idea of what the ostensible sender actually did, whether they did it in a strong and/or stable fashion or with great variability, etc. Knowledge of the possibilities of various meditation trainings creates the possibility of deliberately creating strong, stable experiential content, while the success of clairvoyance experiments suggests that a sender may not usually be of much value. I make that last statement from knowledge that the bulk of older card-guessing experiments showed no significant differences in level of psi, even though the initial expectations of experimenters was that someone “sending” would enhance results (Rhine 1947, 1953, Rhine & Pratt 1957, Wolman, Dale, Schmeidler, & Ullman 1977). I’m not sure if general unimportance of a “sender” would apply to remote viewing procedures.

²³ See comments on “pure telepathy” tests elsewhere in this article.

²⁴ This procedure will not meet the criteria used in the old card-guessing era for a test of “pure” telepathy, i.e. where a percipient successfully describes the content of a sender’s mind, but this success cannot be ascribed to clairvoyance or precognition because no physical representation of the target exists at the time of the test or is created in the future. The difficulties of “objectively” checking the correctness of scoring when there is no objective record produced some ingenious but laborious experimental procedures, the best of which was McMahan’s study (McMahan 1946).

²⁵ Other methods than blind human judging for assessing the amount of psi-acquired information in a remote viewing trial have been tried, but there are inherent problems. If the system gets too fine-grained, for example, useful gestalts of perception may get lost. At the other extreme, using mainly the overall match of the gestalt of the attempted remote viewing may lose details. Thus I just mention the original style of blind judging here for simplicity.

²⁶ Success in this might be useful in some therapy cases, as informing the patient about it might reassure them that a new source of potential assistance is being developed to use with them.

²⁷ Interest in and investigation of psychoanalytically based ideas began waning in the 1960s and is still largely neglected in psychology and psychotherapy, although interest is growing again.

²⁸ The variations and inconsistencies as to what people mean when they use the term *spirit* call for putting it in quotes every time I use it, but it looks

awkward, so I'll skip using quotation marks unless there's an especially vague usage being considered.

- ²⁹ An early discussion of what constitutes the essence of science, "pure science" as I usually call it, was in 1972 (Tart 1972b), while my most recent elaborations are in my book *The End of Materialism: How Evidence of the Paranormal Is Bringing Science and Spirit Together* (Tart 2009), now retitled and available in paperback (Tart 2017).
- ³⁰ Note that in my first article on OBEs (Tart 1967b), I used the acronym OOBE. John Beloff, then editor of the *Journal of the Society for Psychical Research*, reacted by reminding me that the "o" in "of" was not to be capitalized in acronyms (I think anything goes now), so I shortened it to OBE in future work. How could I not respect the linguistic opinion of a distinguished British professor? I was also motivated to drop OOBE for OBE by an unexpected consequence. People who had read my first article started to come up to me after lectures to tell me about their personal ooh bees; I had never thought anyone would pronounce it. ;-)
- ³¹ In the altered states of consciousness class I taught at UC Davis for two decades, when I lectured about dreaming I often asked the hundreds of students in the course whether anyone wanted to argue that they *could* be dreaming, *right then*, about being in a lecture rather than awake and really here. No one ever argued that such a dream couldn't occur. Someone always had some clever intellectual argument that they could be dreaming this then, but when I asked if anyone wanted to bet me fifty dollars that they could wake up and find themselves in bed at home in a minute, nobody ever took up the bet.
- ³² Lucid dreams, where you know you are dreaming but the quality of your consciousness during the lucid dream seems as clear as your ordinary waking consciousness, were something that had happened to me as a child, but were brought to my scholarly attention by a now classic article by Frederick van Eeden (van Eeden 1913), which I reprinted in my *Altered States of Consciousness* anthology (Tart 1969). This had the salubrious effect of alerting many who had occasionally experienced lucid dreaming, but worried that it was abnormal or a sign of mental illness, to accept lucid dreams as an interesting experience instead of something to worry about. Subsequently, researchers such as Stephen LaBerge greatly added to our knowledge of lucid dreams (LaBerge 1985).
- ³³ Of course if you think about whether you're dreaming while in an ordinary dream, you may reason that you're not dreaming, but on waking recall that the state of your consciousness was much less clear and logical than in waking. But memories of the quality of consciousness in OBEs are that it was clear, perhaps sometimes clearer, than in ordinary consciousness.

- ³⁴ I know the term *soul* is objectionable to some people, but attempts by various writers over the years to replace it with a more emotionally neutral term that would facilitate scientific study have not found general acceptance. I keep the term in this paper, without awkward quotes from this point on, in two ways, usually clear from the context. The first is as a shorthand way to characterize certain kinds of experiential *data*, such as the feeling of having been out of body but still conscious and existing, and usually with an emotional feeling that the existence of such a soul is very important. The second is as a theoretical term about the absolute reality of such an “entity” or “process” quite aside from its psychological consequences. Absolute reality would here mean that if all human beings ceased to exist, souls would continue to be real. Strong emotional beliefs and reactions would also be associated with this second meaning of the term *soul*. As scientists we are interested in both the psychological aspects of soul, irregardless of its absolute reality, and the degree to which we can study its absolute reality or lack of it within the framework of essential science.
- ³⁵ I’ve been a student of Buddhism, but not a “Buddhist,” for some years, and although Buddhism claims there is not some permanent, immortal soul, I suspect this insistence is a teaching device to help students lessen habits of over-attachment to concepts. As an example of why I don’t take the no-soul idea literally in Buddhism, it is hard for me to picture how a person’s personal karma could follow them from life to life without a “something” or “some-process” to carry the information. And Buddhists report OBEs.
- ³⁶ The Apostle Paul being taken up to heaven sounds like an NDE, as an example.
- ³⁷ Elements of this MINDS approach are scattered through many of my writings, but I have not yet consolidated this approach in a formal way. Some introduction to major elements can be found in Tart (1993, 2008).
- ³⁸ I speak of kinds of world simulation processes here, in spite of the fact that experiencers often feel as if, analogously, social, psychological, and biological blinders have fallen from their eyes, and they now perceive the ultimate level of reality directly. How much is this correct and how much of it is mainly a contrast effect? Since there is so much processing and creation of apparent, perceived “reality” in ordinary waking, it seems cautiously conservative to assume some similar processes are going on in ASCs, OBEs, and NDEs. Some aspects of reality may be being perceived more directly, some perhaps with more semi-arbitrary processing. I’ll assume that research will someday be able to specify the degrees of “true” perception versus semi-arbitrary construction.

³⁹ (Tart 1968).

⁴⁰ I emphasize the caution I showed in drawing conclusions from that study, as half a century later I am still being erroneously accused, clearly by people who have not actually read the original report, of claiming to have *proven* that OBEs mean a soul is really “out.”

⁴¹ (Tart 1998b).

⁴² I don’t recall for sure whether he was still “inside” his physical body after rotation or had floated a little above it. The latter is shown in my figure, but I think he told me he was still “inside” the boundaries of his physical body, which was lying on its back.

⁴³ (Tart 1991, 1993).

⁴⁴ The degree to which the BPVR we experience is constructed in the brain and nervous system, as well as in a different “mind” reality, is a question to be researched one day, rather than holding firm a priori beliefs about it with no reference to observable data such as psi.

⁴⁵ One experiential exception is the feeling, sometimes occurring in various ASCs, that we are now really perceiving reality *directly*. This *feeling* is data, to be studied. Like any theory in science, though, we must test its actual applicability to observable realities. The possibility that such experiences may actually be truer understandings and perceptions of reality is exciting. I think it happens that way sometimes and other times is illusory and calls for much investigation.

⁴⁶ The possibility of unconscious mind-to-mind communication, implicit telepathy, creates another interesting possibility that even if all people having the genetic heritage to create a close version of Locale III in their BPVR processing die, so it ceases to “independently” exist, it could spring back into existence the next time it is genetically possible even though not specifically created by certain genes in a new person. Then unconscious telepathic processes could fine-tune details for more agreement across persons. And, to point in the direction of really difficult methodological problems that could hamper study, suppose the appropriate information for creating Locale III or the like is stored “somewhere” even if not in living human memory? I’m not suggesting that essential science cannot cast any light on this, but it does get complicated!

⁴⁷ (Tart 1967a, reprinted in Tart 1969:291–308).

⁴⁸ I have deliberately made this statement in a strong form to make the reader wonder if I have abandoned my scientific objectivity. I haven’t abandoned my *attempts* at using scientific objectivity—my studies of my own mind over the years have shown me I’m very biased on many things, but knowing what these biases are gives me more of a chance to be more

objective—but we’ve been discussing a lot of far-out stuff, and I don’t want the reader to get too accepting . . .

⁴⁹ At SRI we did one remote viewing experiment where we built a square, open-front display cabinet with 16 discrete cubbyholes in it, and an independent experimenter randomly arranged 16 different objects in them. The display cabinet was then kept in a secure vault. Then instruction to a remote viewer in a distant room would ask her or him for a description of one of the items by specifying the coordinates of that object, such as B3, (second shelf down from the top, third compartment from the left). The results were significant for psi (Puthoff, Targ, & Tart 1980).

⁵⁰ When the first reports of demonstrations of Experimenter bias appeared back in the 1960s (Orne 1962, Rosenthal 1963, 1966), I envisaged a major shakeup in psychology and related fields as we realized how biased many of our studies could be, as well as expecting strong resistance to accepting the reality of E bias. My own and Suzanne Troffer’s study of E bias in sophisticated Es, *who knew they were going to be tested for bias, but showed it anyway* (Troffer & Tart 1964) demonstrated how strong such bias could be, as well as the resistance to accepting it. We were *scientists*, highly educated, superior beings dedicated to discovering truth, we couldn’t be biased!

To my amazement, studies of E bias seemed to rapidly fade away, as if the problem had been solved by straightforward means such as having research assistants act as the Es who actually ran Ss, rather than the principal investigator, who we would expect to be the most invested in particular study outcomes. A simple matter of eliminating possible bias by using intermediaries?

The problem of E bias is made even stronger once the existence of psi is recognized: How do you stop bias being transmitted over an information “channel” of almost entirely unknown characteristics, such that we do not know how to lessen or eliminate it? I’ve long suspected that the intense and irrational resistance to accepting research involving the reality of psi is strongly motivated, at some level, by the realization that psi may make it very difficult if not impossible to control bias.

⁵¹ Ignoring individual differences; experimenter bias; culture boundedness; insufficient training and lack of method; the dominance of materialism; and the stubborn assumption that the mind is inherently private, a priori we believe we can’t really do well in studying mind, so we don’t try that hard, and, sure enough, we don’t do very well.

⁵² It’s very important not to get carried away with the idea that we have built-in biases as humans and so don’t try hard enough to overcome them!

My colleague David Hufford observed (personal communication, 2014):

*What I have always told my graduate students
is that certainty is a great direction in which to head,
but it is a disastrous place to believe you have arrived.*

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RESEARCH ARTICLE

**How Smokers Change Their World and How the World Responds:
Testing the Oscillatory Nature of Micro-Psychokinetic
Observer Effects on Addiction-Related Stimuli**

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Abstract—According to standard quantum theory, the occurrence of a specific outcome during a quantum measurement is completely random (see Bell 1964). However, some authors refer to revised versions of quantum mechanics (e.g., Walker 2000, Penrose & Hameroff 2011, Mensky 2013, Stapp 2017), and propose that the human mind can actually influence the probability of such outcomes. Empirical support for this idea has been provided by micro-psychokinesis (micro-PK) research, which shows a small but significant overall effect (see Bösch, Steinkamp, & Boller 2006). However, attempts to replicate specific findings have often failed (e.g., Jahn et al. 2000), a critique that is not exclusive to psi paradigms. In an attempt to explain these failures, von Lucadou, Römer, and Walach (2007) established a theoretical model predicting unsystematic variations of such an influencing effect across replications, resulting in a decline of a predictable effect in micro-PK data over time. Maier, Dechamps, and Pflitsch (2018) slightly expanded this theory by proposing that the temporal variation of such an effect follows a systematic pattern, which can be tested and used for prediction making. In this research, we generated such a prediction using data from two previous studies that initially demonstrated a strong micro-PK followed by a subsequent decline in the effect over the course of 297 participants (Maier & Dechamps 2018); we then put it to the test with a preregistered additional set of newly collected data from 203 subjects. We compared these results with 10,000 simulated datasets (each set with an $N = 203$) each comprising random data. Three tests were applied to the experimental data: an area-under-the-curve analysis, a local maximum fit test, and an endpoint fit test. These tests revealed no significant fit of the real

data regarding the predicted data pattern. Further analyses explored additional techniques, including an analysis of the highest-reached Bayes Factor (*BF*) over the course of the experiments, the overall orientation of the *BF* curve, and its transformation into oscillatory components via a Fourier analysis. All these methods allowed for statistically significant differentiations between experimental data on the one side, and the control group and simulation data on the other. We conclude that the analyses of the temporal development of an effect along these lines constitute a fruitful approach toward testing non-random and volatile time trends within micro-PK data.

Keywords: micro-psychokinesis—observer effect—mind–matter—quantum measurement

Introduction

The relationship between the mental and the physical worlds, between mind and matter, remains a fascinating enigma to this date. Recently, advanced theories have tried to shed light on the mutual interactions of these worlds by incorporating ideas pertaining to an inherent connection of quantum theory and consciousness (see Atmanspacher 2004). A quantum state is characterized by peculiar properties, such as the superposition of multiple states or the possibility of entanglement. The transition from a quantum to a classical state is associated with the measurement of the quantum system. Whereas orthodox quantum mechanics mostly denies that mental aspects play a crucial role in the measurement process (see Greenstein & Zajonc 2006), some researchers argue that consciousness, or other mental concepts, contribute a much more relevant part in this transition (e.g., Penrose & Hameroff 2011, Mensky 2013, Stapp 2017, see also von Neumann 1932, Wigner 1963, Walker 2000).

According to some of these models, the quantum state can be interpreted as a pre-real possibility space, which interacts with both classical physical states and their corresponding representations within the conscious mental world (Filk & Römer 2011, Atmanspacher & Filk 2012, Stapp 2015). Quantum measurement, the moment when one of many superposed possibilities becomes real according to classical physics, is equally considered as a transference of knowledge from unknown into consciously known knowledge. If a measurement is characterized by the conscious processing of a classical outcome, quantum-specific effects must therefore operate within a pre-conscious realm (Penrose & Hameroff 2011, see also Penrose 1989, 1994, Hameroff & Penrose 1996, Hameroff 2012). Accepting this possibility, Jung and Pauli (see Atmanspacher 2012) even argued that conscious observation during quantum measurements itself does not cause the physical transition from quantum to classical states; rather, conscious observation of a measurement result co-occurs with the manifestation of

a classical state in the physical world. Standard orthodox quantum theory predicts the occurrence of specific measurement outcomes with a probability function that is ontic in nature. Comparatively, some authors argue that the measurement interaction is not unidirectional, but that information can flow from observer to the observed system, consequently biasing the probability distribution (e.g., Walker 2000, Stapp 2015).

Although consciousness is crucial to these considerations, the origin of such a bias must be located in the transition from a subconscious to a conscious state of mind. Studies showing directional psi effects of subjects not consciously intending them support this claim (Stanford 1976, Stanford, Zenhausern, & Dwyer 1975). Maier and Dechamps (2018) extended the idea that this reality-shaping effect is non-intentional (Stanford 1990) and goal-oriented (Schmidt 1974) by characterizing the underlying unconscious motivational states.

Accordingly, these states, which translate their drives via emotions (e.g., fear or hope) into conscious realizations, provide the object of the influencing mechanism. For instance, if an individual has an unconsciously grounded motive to smoke, their corresponding desire is emotionally laden with the fear of not having enough nicotine in their body. This deficit-oriented fear translates into an expectation of never receiving enough of the desired substance. When such an addicted person interacts with a quantum system that is connected with two equally likely realities—one that corresponds to their motive, and the other being neutral regarding their motive—their desire will influence the measurement of that quantum system. Accordingly, this will lead to an increased likelihood of experiences corresponding to the implicit belief; a consequence that standard quantum probability would not allow. In other words, under such circumstances the unconscious mental constitution of an observer would create classical realities that include their conscious observations that accord with the individual observer's inner emotional desires. This would represent a truly self-fulfilling prophecy.

This idea is congruous with results yielded by micro-psychokinetic (micro-PK) research. Micro-PK can be described as mental influences on inanimate and probabilistic systems that produce effects that are detectable only through statistical means (Varvoglis & Bancel 2015). Typically, this involves studies measuring the influence of observers on random target systems, such as random number generators. Several meta-analyses have aggregated data from hundreds of micro-PK studies and found substantial overall effects resulting from observer influences (Radin & Nelson 1989, Bösch, Steinkamp, & Boller, 2006). The high heterogeneity of the effect sizes raises concerns for some critics; however, Varvoglis and Bancel (2015) conclude that despite some publication bias quite possibly being present, an

unrealistically large file drawer would be needed to annul the results (see also Radin et al. 2006).

Nevertheless, micro-PK research has to grapple with recurrent difficulties, arguably the most important of which is the lack of successful direct replications (e.g., the replication of the PEAR landmark study; Jahn et al. 2000). This evasive nature of psi effects has not gone unnoticed within the psi research community (e.g., Bierman 2001, Kennedy 2003), and has furthermore confronted us with challenges during an attempt to replicate a promising first study on micro-PK (Maier & Dechamps 2018).

In this set of two consecutive studies, we originally aimed to investigate a possible mind–matter interaction by experimentally linking the outcome of a random event to a psychologically meaningful experience. Studies focusing on a relaxed mental state of effortless intention usually generate better results than those focusing on an intentional and deliberate influence on randomness (e.g., Braud & Braud 1979, Debes & Morris 1982). Accordingly, we designed our independent variable using a primarily unconsciously driven state: the desire for cigarettes within regular smokers. Through these means we ensured that we integrated the subject’s implicit mental state into the design.

In the first study, we compared the effects of participants with a pronounced drive—regular smokers—on the output of true random number generators (tRNGs) with the effects of non-smoking, and therefore unmotivated, participants. Depending on the tRNG’s outcome, pictures pertaining to the drive and need (e.g., people smoking) or pictures not pertaining to this need (e.g., a chair) were displayed to the passively observing participants. We then tested whether the distinctive mindset of regular smokers led to a bias in the quantum measurement process and, subsequently, to a non-random distribution of addiction-relevant stimuli.

In Study 1, 122 smokers and 132 non-smokers were presented with 400 trials. We hypothesized that the specific mental attitude toward cigarette-related content within smokers would lead to a deviation in the number of smoking pictures from that of random chance; accordingly, no deviation from chance was expected concerning the non-smokers.

Data were analyzed using a Bayesian approach (see Wagenmakers et al. 2011), which updates the support and evidence for either hypothesis with each new data point by pitting their likelihoods against one another. This subsequently allows for sequential testing, which involves adding and continuously analyzing data until a pre-specified stopping criterion has been met. We decided upon a Bayes Factor (*BF*) of 10 as a stopping rule, which corresponds to strong evidence toward a given hypothesis. Concerning a prior probability, we decided to use a Cauchy distribution centered on

TABLE 1
Mean Number of Cigarette Pictures and *BF*
for Both Subsamples in Studies 1 and 2

	SMOKERS			NON-SMOKERS		
	N	M (SD)	<i>BF</i> ₁₀	N	M (SD)	<i>BF</i> ₁₀
Study 1	122	196.7 (9.87)	66.06	132	200.5 (9.86)	0.16
Study 2	175	200.3 (10.38)	0.09	220	199.0 (10.34)	0.30
Studies 1 & 2	297	198.8 (10.31)	0.61	352	199.6 (10.11)	0.12

zero with an r of 0.5, i.e. $\delta \sim \text{Cauchy}(0, .5)$, as proposed by Bem, Utts, and Johnson (2011). At the beginning of this research there was no clear prediction regarding direction of the effect, and so a two-sided approach was chosen.

The final Bayesian t -test analysis with 122 smokers yielded a BF of 66.06 toward H_1 ; this means that it was 66 times more likely to obtain the data when the alternative hypothesis was true than when the null-hypothesis was correct. Smoking participants saw an average of 49.18% addiction-related stimuli ('cigarette pictures'), a percentage substantially below the expected chance value of 50%, thereby representing a very strong effect. Concerning the non-smoking subsample of 132 participants, the Bayesian t -test yielded a BF of 6.13 ($=1/0.16$) toward H_0 ; this indicates moderate evidence for a null effect (see Table 1). These remarkable results point to a need-specific micro-PK effect within those participants who experienced a subconsciously active but deficit-oriented desire toward the relevant stimuli. The results are in line with the reasoning of the emotional transgression model, as outlined above (see also Maier, Dechamps, & Pflitsch 2018). This model emphasizes the importance of implicit beliefs—in this case an unconscious experience of a lack of nicotine—when influencing the actualization of a corresponding outcome as part of a micro-PK experiment. This deficit translates into craving and addictive actions on one side, and into a subconsciously active mental pattern centering on 'not having enough' (of the addictive substance) on the other. Consequently, this unconscious fear leads to a self-fulfilling prophecy: The lack experienced by the smoker 'establishes' a confirming reality that misses smoking-related content. Accordingly, the consciously experienced classical reality corresponds to the unconscious inner beliefs of the observer.

After these initially promising results, we decided to perform an exact replication of the experiment to further substantiate the observed effect. The pre-registered replication¹ used the same procedural details as those of the first study. Surprisingly, our expectations were not met, and Study 2 did not replicate the findings of Study 1. For smokers, Study 2 revealed strong evidence toward the null hypothesis ($BF_{01} = 11.07$); comparatively, for non-smokers Study 2 revealed moderate evidence in the same direction, replicating the findings of Study 1 (see Table 1).

We faced a dilemma at this point, as the data yielded by Study 2 suggested there was no replicable effect of the smoker subsample on the number of cigarette pictures. Still, the effect development of the smoker and non-smoker subsamples seemed to differ immensely over the course of both experiments. This was particularly obvious when concatenating the data of both studies, as is allowed with Bayesian sequential testing. The two-sided one-sample t -tests showed a remarkable development within the smoker subsample. The BF begins to rise after about 80 participants, quickly surpassing the threshold of (very) strong evidence, reaching a climax of $BF_{10} = 421.2$ at participant number 134, shortly after commencing the second study. Soon a dramatic decline began to appear, countering the effect and eventually leading to an overall BF lower than 1, indicating evidence toward the null hypothesis. This extraordinary change in effect across times was found only within the smoker subsample. Contrarily, the non-smoker subsample showed a steady and consistent decline toward H_0 from the beginning (see Figure 1).

Given the apparent differences of the temporal changes in the sequential BF s of the two subsamples, we speculated as to the presence of a systematic time-dependent micro-PK effect, despite the lack of replication. As mentioned earlier, decline effects are somewhat prominent, especially in psi research (e.g., Bem, Palmer, & Broughton 2001, Radin 2006). Accordingly, it seems as if for these—and potentially other effects—different methodological approaches might be used other than the replication of original results. As the temporal development of the effect is so clearly identifiable among the data, we decided to focus on this temporal change. Of particular interest was the onset and development of the effect decline and whether a systematic pattern could be described here (see also Maier, Dechamps, & Pflitsch 2018).

This decline of initially strong effects—referred to as the statistical equilibrium effect, the balancing effect, the differential effect, or the first-timer effect, among other names; we refer to this pattern as the “Nemesis effect” (in reference to the ancient Greek goddess of retributive justice)—seems to pervade many psi studies, within both our lab and others.

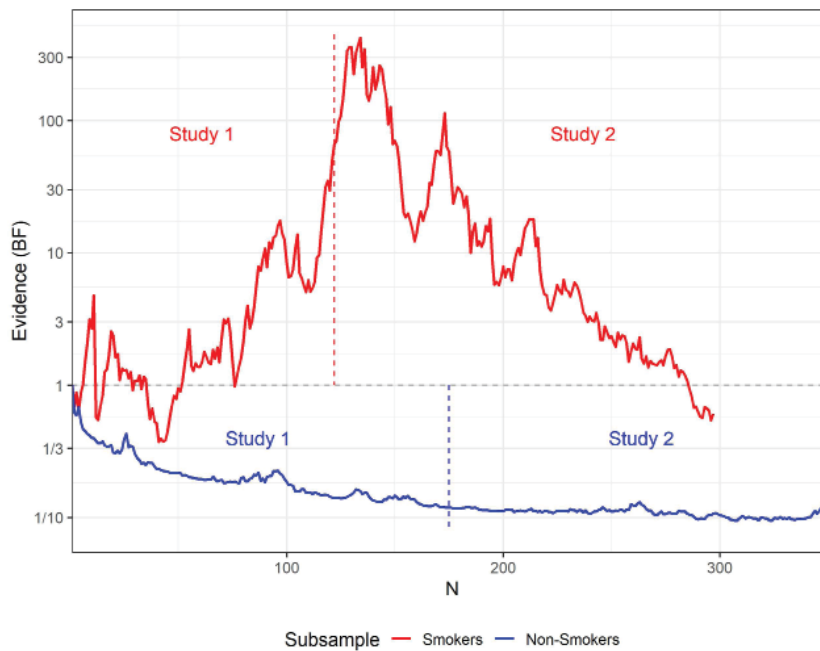


Figure 1. Sequential Bayesian analyses of mean number of cigarette pictures for smoker (top curve, red line) and non-smoker (bottom curve, blue line) subsamples. Dotted lines indicate the transition from Study 1 to Study 2 within both subsamples.

Explanations for this effect, along with selective publication and increasing study quality (both do not apply here), include several psychological variables, for example subjects becoming tired or bored with the task. However, between-experiment declines of effects, as is often the case, cannot be explained through these mechanisms (for an overview, see Bierman 2001). Another psychological explanation revolves around unconscious belief systems toward supernatural phenomena. Since implicit beliefs seem to play a crucial role in the characteristic or direction of a reality-shaping influence, Eisenbud (1992) reasoned that an unconscious motivation to live in an orderly world might be responsible for a decline of paranormal effects (see Braude 2016).

Von Lucadou, Römer, and Walach (2007) addressed the phenomenon of declining effects theoretically and provided an elaborated explanation in their Model of Pragmatic Information (see also von Lucadou 2006, 2015). Pragmatic information is meaningful for a closed system and provides an opportunity for entropy-reducing change (see Wiezäcker 1974). The authors

considered this kind of pragmatic information as a prerequisite for the emergence of nonlocal correlations within closed psychophysical systems, which underlie most psi effects (von Lucadou 1995, Walach, von Lucadou, & Römer 2014). Psi effects can be understood as entanglement correlations that are mediated not through causal signals, but rather as a consequence of a specific configuration of a system's mental and material components (von Lucadou, Römer, & Walach 2007). Any artificial setups allowing the use of such pragmatic information within entanglement correlations for signal transmission—something that would be feasible if psi effects were robust and replicable—would lead to a reduction of pragmatic information and, therefore, to an unsystematic disappearance of psi effects in later replication attempts. This would satisfy the important Non-Transmission Axiom (NT Axiom) of quantum mechanics (Atmanspacher, Römer, & Walach 2002, von Lucadou, Römer, & Walach 2007). The NT Axiom is supposed to prevent physical paradoxes such as faster-than-light signal transfer (see also Einstein, Podolsky, & Rosen 1935) and ensure that natural order is abided (Walach, von Lucadou, & Römer 2014). Von Lucadou, Römer, and Walach (2007) condensed the factors responsible for the decline in the following formula:

$$I = R * A = B * E$$

Here, the amount of pragmatic Information (I) increases with higher proportions of Novelty (E) and Autonomy (A), and decreases with higher proportions of Confirmation (B) and Reliability (R). Replication studies involve low levels of novelty and autonomy as they often involve the specification of a clear prediction and are, therefore, unlikely to contain a sizable amount of pragmatic information; i.e. they are without unambiguous results (Etzold 2004, Walach, von Lucadou, & Römer 2014). In sum, the Model of Pragmatic Information (MPI) predicts that exact replication attempts of psi effects based on non-locally correlated systems need to decline in the original dependent variable and possibly reappear unpredictably in other variables of the system (so called 'displacement'). This would also be true for micro-PK effects and may provide an explanation for the elusiveness of such phenomena (von Lucadou, Römer, & Walach 2007).

Maier, Dechamps, and Pflitsch (2018) (see also Maier & Dechamps 2018) also refer to the MPI to explain their results, though they revised its predictions regarding the decline of psi effects. Instead of adopting the assumption regarding the unsystematic disappearance of the effect per the original MPI, these researchers proposed a systematic temporal change of micro-PK effects (and psi effects in general). This revision is based

on the idea that evidence for micro-PK constitutes a severe violation of the Second Law of Thermodynamics because a chaotic system, such as a random quantum event, becomes more orderly under micro-PK without the investment of energy. Such a system therefore decreases in entropy over time. Consequently, with increasing evidence for micro-PK, the onset of an entropic countereffect is proposed. We assumed that this entropic countereffect manifests as soon as the information resulting from mentally induced deviations from quantum randomness reaches a certain threshold of experimental evidence. Once this original effect—and the evidence for such an effect—has weakened, the countereffect also loses power, allowing the micro-PK effect to reappear again. This interplay between effect and countereffect should lead to a distinctive pattern when analyzing the effect's temporal development.

In a recent publication reporting the results of two micro-PK studies using smokers and non-smokers—and the time course of these effects across the studies (Maier & Dechamps 2018)—we proposed a way to study a systematic variation of the effects by analyzing their temporal change. Within the aforementioned publication we argued that psi effects will display a systematic interplay of effect and countereffect in the form of a dampened harmonic oscillation. We then applied this idea to the smokers' data from Studies 1 and 2. To do this, we plotted the effect as a cumulative z-score and used a curve-fitting algorithm, with the software Wolfram Mathematica Version 11.1 (<https://www.wolfram.com/mathematica>), estimating a dampened harmonic oscillation as an approximation of the raw data. Furthermore, an extrapolation of this function was then used to predict the future time course of a third set of data with that of prospectively collected micro-PK data (the study reported herein). For this purpose, the extrapolated continuation trend of our calculated curve should constitute a rough estimate for the development of the effect within this future dataset (see Figure 2).

From this curve extrapolation, a prediction for the next 200 participants can be derived: After a temporary local maximum (technically a local minimum, as the effect is negative), the effect will reappear, though with a lower effect size and then decline once more to a value similar or slightly more negative than the initial end-point value. The local maximum should appear around subject number 410–450 and should lie between a z-score of -2.5 and -3 . We decided to look at several test statistics to test for the quality of our prediction among the experimental data compared with 10,000 simulated datasets:

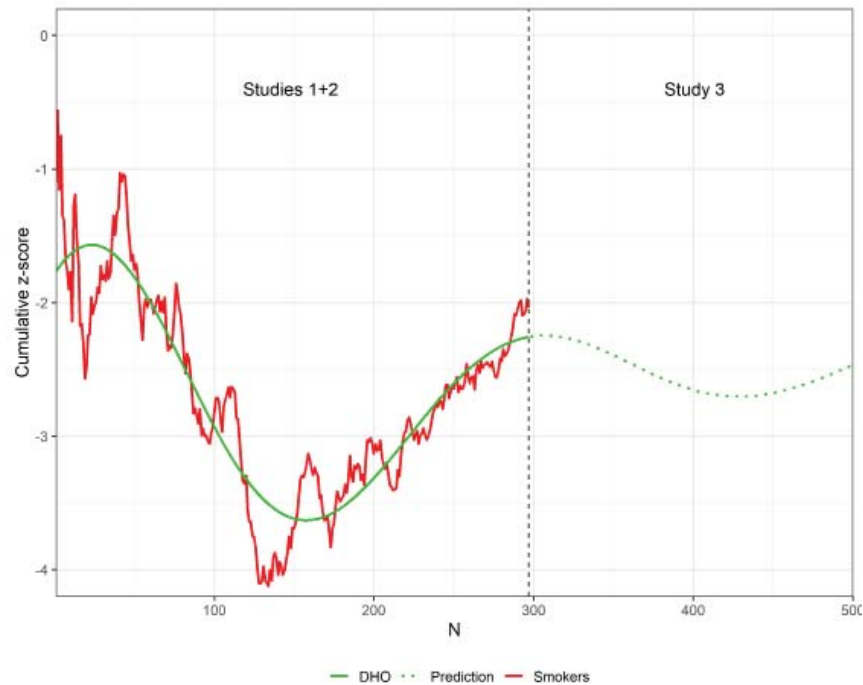


Figure 2. Cumulative z-score of the smoker subsample (jagged curve, red line), fitted damped harmonic oscillation (DHO) (smooth curve, green line), and prediction (dotted curve) for the following 203 participants.

- 1) The area between the experimental data curve and the prediction.
- 2) The Euclidian distance of the local maximum compared with that of the prediction.
- 3) The Euclidian distance of the end point compared with that of the end point of the prediction.

Methods

This study was pre-registered at the Open Science Framework (OSF) (<https://osf.io/ac839>). It was conducted in accordance with the ethical requirements of the American Psychological Association (APA). All materials and procedural decisions were identical to those of Studies 1 and 2 (Maier & Dechamps 2018).

Consent

All participation in this study was voluntary, and written consent was obtained from all participants prior to their involvement. Interested participants were given an individual explanation about the purpose of this study once they had completed their tasks. The procedure used in this study was approved by the ethical board of the Department of Psychology.

Participants

Participants were added until a total of 500 smokers had been reached, including all participants from Studies 1 and 2. This resulted in a total of 236 tested participants for this study; of these participants, 203 were smokers and were included in this analysis accordingly (102 female, 101 male; mean age = 33.1 years, $SD = 14.44$). Participants were recruited via several means, including through the Department's announcement board, handouts in psychology classes, Facebook university groups, and direct contact with the experimenters. Participants who were enrolled in the university's psychology bachelor's degree classes were able to acquire credits as part of their program. For this study, we explicitly recruited 'smoking participants'.

Participants were asked to provide information about their smoking behavior. Only those participants who reported that they were a 'regular cigarette smoker' (at least 1 cigarette per day) were considered for inclusion in this study, mirroring those inclusion criteria applied for Studies 1 and 2. Of the 33 excluded subjects, 3 claimed to be strict non-smokers, 23 stated they were casual smokers, and 7 described themselves as smokers of other tobacco products (e.g., pipe). Additional questionnaires were the same as those used in Studies 1 and 2, namely the German version of the Fagerström Test for Nicotine Dependencies (FTND-G) (Schumann et al. 2003), and a questionnaire assessing the subject's attitude toward smoking. Here, participants were asked to respond to ten statements about smoking, indicating their level of agreement toward each one positively (e.g., "smoking is fun") or negatively (e.g., "smokers smell bad"). The questionnaires were not part of our hypotheses and therefore were not considered further.

Materials

Software and Computers. The study was conducted using three mobile testing stations comprising a notebook, a tRNG, and the experimental software. All notebooks used for this study were prepared in an identical fashion. The experimental software was programmed in C#, and translated the tRNG output into a display of either a smoking-related ('cigarette')

picture, or a neutral picture (e.g., objects of daily use). The stimuli were presented in the center of the screen on a black background with a size of 500×400 pixels, corresponding to roughly one-quarter of the overall screen size.

Stimuli. All smoking-related pictures were taken out of the Geneva Smoking Photographs (GSP) set (Khazaal, Zullino, & Billieux 2012), a normative database providing 60 addiction-relevant photographs for nicotine and tobacco research. A set of 10 pictures was chosen from this database, providing variation in terms of product, smoking behavior, and tobacco-related cues (e.g., cigarette packs, ashtrays, smoking individuals, etc.).

All neutral pictures were taken from the International Affective Picture System (IAPS) (Lang, Bradley, & Cuthbert 1997), a dataset providing an experimental set of 1,169 digitized photographs rated on arousal and valence on a 9-point scale. A set of ten pictures displaying everyday objects and geometric forms was chosen from this dataset. These pictures were rated neutral (mean valence = 4.90, $SD = 1.09$), and unexciting (mean arousal = 2.61; $SD = 1.86$).

Generation of quantum randomness. Randomness in the form of superposed quantum states was provided by a quantum random number generator (Quantis-v10.10.08) as developed by the company ID Quantique (<http://www.idquantique.com/random-numbergeneration/quantis-random-number-generator>). This tRNG generates quantum states by emitting photons aimed at a semi-conductive, mirror-like prism, similar to the well-known double-slit experiment. Photons have an equal chance of being deflected or passing through the prism to be registered by one of two sensors. Upon measurement, the photon's superposition vanishes, and it is found on either route with a 50% probability. Depending on which sensor registers the photon, a "0" or "1" bit is created and passed to the computer via a USB interface. The Quantis tRNG has passed all serious tests of randomness (e.g., NIST SP800-22 Compliance) and is considered one of the most effective sources of randomness (Turiel 2007).

Experimenters

Informally trained research assistants were used as the experimenters for this study; their task was to find 203 participants who were regular smokers. The experimenters had only rudimentary knowledge about the aim of the experiment and were blind toward the specific hypothesis at the point of data collection.

Procedure

Participants were tested in different environments without distractions or other persons present. At the beginning of the experiment, written instructions were read to the participants:

Thank you for participating in this experiment! In the first part of the study you will sit in front of the computer and look at pictures. I know that this can be very tiring, nevertheless I ask you not to get distracted and that you focus your attention on the computer for the whole period of this part of the study. It is *absolutely necessary* for this experiment that you look at the pictures! This will take approximately 10 minutes. Of course, you can quit the experiment at any time, should you feel uncomfortable.

As soon as you have finished, a message will appear on the screen. Please let me know, so I can prepare the computer for the second part of the experiment, which will be a questionnaire. Filling it out will take about five more minutes. All data will be collected anonymously.

Do you have any questions?

After participants' potential questions were answered, the experimenter opened the software and instructed participants to start with the presentation of the pictures when they felt ready. Subsequently, the experimenters sat aside and distracted themselves mentally to exclude possible interference with the experiment itself, only occasionally checking on the participants.

Participants attentively watched a series of 400 picture trials displaying either an addiction-related or a neutral stimulus on each trial, depending on the output of the tRNG. Stimuli were chosen by sampling without replacement during a ten-trial block. After every 10th trial, all pictures had the same probability of being chosen again. This process ensured that, over the course of the experiment, each stimulus had the same probability of being displayed and affecting a participant. Participants first looked at a centered cue (700 msec), then at the addiction-related or neutral stimuli (400 msec), and finally at a black screen (400 msec). This process was repeated 400 times and took approximately 10 minutes (see Figure 3).

After the completion of the stimulus presentation, participants were asked to fill out the questionnaire using the computer's web browser.

Derivation of the Prediction

Data from Studies 1 and 2 were transformed to cumulative z-scores in order to derive a prediction. First, standard deviation was calculated for each of the 298 data points:

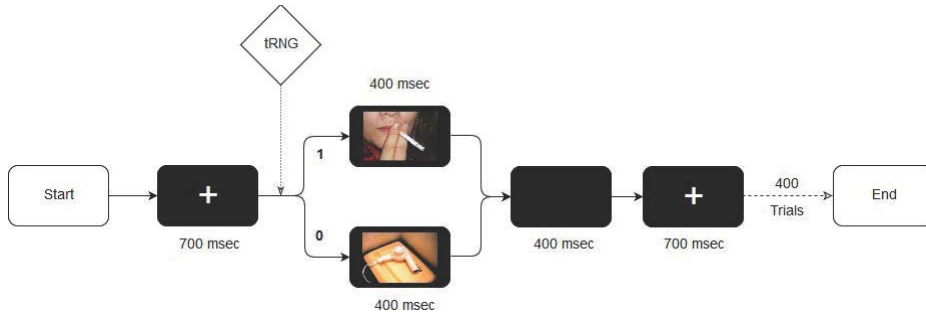


Figure 3. Experimental procedure: Schematic flow chart for a single trial.

$$SD_n = \sqrt{t \times n \times p \times (1 - p)}$$

where t is the number of trials ($t = 400$), n is the number of subjects from 1 to 298, and p is the probability of a hit for a single trial ($p = 0.5$). Subsequently, the difference between the cumulated number of hits (number of smoking-related pictures) from the expected value for each data point was transformed in units of standard deviations:

$$z_n = \frac{\sum_1^n M_{obs} - \sum_1^n M_{exp}}{SD_n}$$

The resulting dataset was then plotted and a nonlinear model fit applied using the function of a general dampened harmonic oscillation:

$$y(n) = ae^{-\beta n} \cos(\omega n + \varphi) + mn + h$$

The software used for the curve fit (Wolfram Mathematica 11.1) required boundary values for the model fit; hence, the following boundaries were given:

$$\begin{aligned} -5 < a < 5 \\ 0 < b < 0.01 \\ 0 < \omega < 0.1 \\ 0 < \varphi < 2\pi \\ -5 < m < 5 \\ -5 < h < 5 \end{aligned}$$

After it had been ensured that none of the fitted parameters were near a constraint boundary, the goodness of the fit was evaluated by a variance estimation using the mean squared error. It was ensured that no other relevant combination of constraint boundaries would produce a lower variance estimation than the attained variance of $Var = 0.065$, and, therefore, a better fit to the experimental data. The final model produced the following estimates:

	Estimate	Standard Error	Confidence Interval
α	1.53484	0.0472682	{1.44197,1.62771}
β	0.0031827	0.000182361	{0.0028244,0.00354099}
ω	0.0191355	0.000296296	{0.0185534,0.0197177}
φ	6.00159	0.0599918	{5.88372,6.11946}
m	0.00193325	0.00014269	{0.0016529,0.00221361}
h	-3.02286	0.0423804	{-3.10613,-2.93959}

Therefore, we used a damped harmonic oscillation of the following kind to produce the prediction for this study, from $N = 298$ to $N = 500$ (see also Figure 2).

$$y(n) = 1.53484 e^{-0.0031827 n} \cos(0.0191355 n + 6.00159) + 0.00193225 n - 3.02286$$

This predictive function was described in the pre-registration stage, as well as in the original Maier and Dechamps (2018:288) article.

Data Analysis

To determine whether the experimental data of this study fits the prediction, we decided to look at three key characteristics. Firstly, if the courses of the empirical and the prediction curves are similar, the area between them should be minimal. We therefore decided to calculate the area under the curve (AUC) of both the experimental data and the prediction before using their difference as the first test statistic.

Secondly, harmonic oscillations are generally characterized by a systematic upward and downward movement with local minimum and maximum points. Our prediction shows such a local maximum (a maximum for negative effect, so technically a minimum) at $N = 429$ and predicts a z-score of $z = -2.70$. We then used the Euclidean distance of the local maximums of the experimental data and the prediction as the second test statistic.

Thirdly, we decided to look at the last cumulated data point, since it contains the most precise information about the effect. To test how well our harmonic oscillation predicts the final z-score, we decided to use the Euclidean distance of the end points of the experimental data and the prediction as a third test statistic.

Since no theoretically derived criteria were available that indicated at which respective scores these statistics could be defined as being statistically significant, we decided to compare our results with those obtained from 10,000 simulations. Simulation data were generated using the same tRNG device employed in the original study. For each simulation, 203×400 bits were generated and summed up in the same fashion as in the experimental dataset; this produced 10,000 instances of a purely random development of our data that we could compare with our actual experimental data.

Main Results

Of the 236 subjects, 203 self-identified as regular smokers and were added to the overall analysis, which also included smokers' data from Studies 1 and 2. The total mean for cigarette pictures was $M = 199.1$ ($SD = 10.11$). This is slightly below chance value and, therefore, in the same hypothesized direction as in the first two studies. A Bayesian analysis revealed a $BF_{10} = 0.40$, which corresponds to anecdotal evidence toward the null hypothesis. Accordingly, in looking at this average score no substantial deviation from chance was found. It must be remembered that we did not hypothesize a consistent effect for the data in this study but rather a specific development of the effect curve.

The z-score plot of the experimental data started slightly above the prediction value, before rising toward zero until it reached a z-score of -1.58 at $N = 380$. Subsequently, the effect can be seen to increase once more, reaching a local maximum at $N = 475$ with a z-score of -2.60 . Finally, the last data point at $N = 500$ equals $z = -2.35$, which is slightly more negative than at the beginning of Study 3 ($z = -2.06$; see Figure 4).

Area under curve. The area between the prediction and the experimental data curve was found to be 85.22. Of all 10,000 simulations, 18.71% revealed a smaller area between them and the prediction (Mean $AUC = 157.48$, $SD = 74.03$). Figure 5 shows the distribution of AUCs.

Local maximum (turning point). The local maximum, or turning point, of the experimental data is equivalent to the highest absolute z-score; this was identified at $N = 475$ whereby $z_{\max} = -2.60$. To measure the distance to the predicted turning points, x- and y-coordinates were standardized to fit in a 1×1 square, i.e. the lowest value was transformed so that it was equal to 0, and the highest so that it was equal to 1. This ensured that both

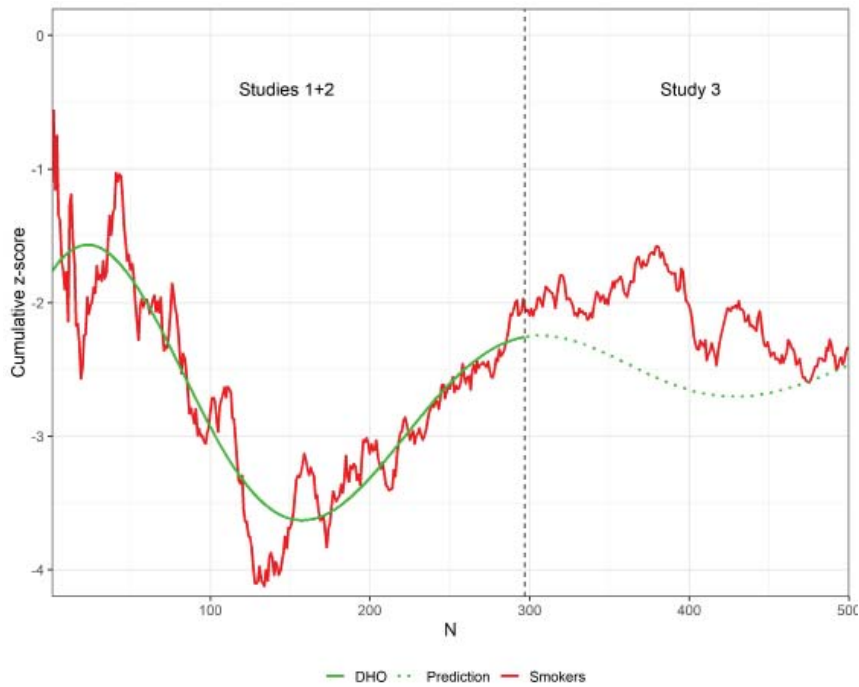


Figure 4. Results of Study 3 were transformed into cumulative z-scores and compared with the prediction, the extrapolation of the fitted dampened harmonic oscillation (DHO).

distances—value of the z-score and the subject number—were accounted for in equal parts. The transformed distance between the turning points of the experimental data and the prediction was $d_{\text{trans}} = 0.23$.

In sum, 19.42% of simulations showed a smaller distance for their turning points to the prediction (mean distance_{trans} = 0.44, $SD = 0.19$) (see Figure 6).

End point. The final z-score of the experimental data lies at $z = -2.35$; this is 0.11 points above prediction. Of all simulations, only 5.28% showed a smaller distance to the prediction (mean distance = 0.90, $SD = 0.62$).

Discussion of the Main Analyses

The results of our confirmatory analyses of Study 3—which combined data from Maier and Dechamps (2018) with the newly collected data described herein—revealed a mixed picture. For two of the three analyses applied, roughly 1/5 of all simulations showed a closer match to our prediction

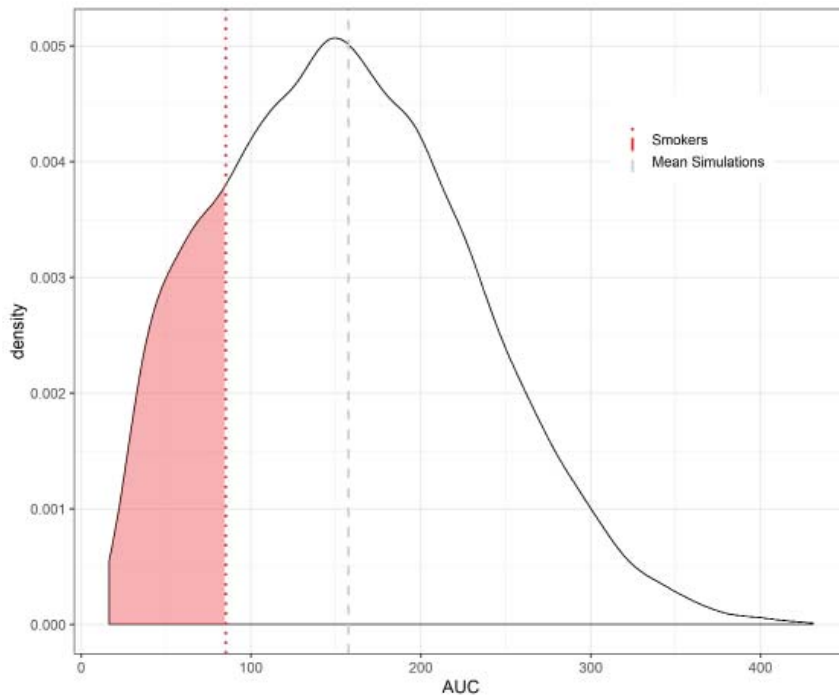


Figure 5. Distribution of the area under the curve between the simulations and the prediction. The shaded red area (gray in the print journal) indicates the proportion of simulations with a smaller area, i.e. a better fit than the experimental data.

function than the actual experimental data. This includes the area between curves, as well as the occurrence of the local maximum or turning point.

Descriptively, it seems that the general trend—a temporary increase of the effect that declines toward the end of the study—is present in the data. As can be seen in the median simulation in Figure 7, a truly random dataset should move toward $z = 0$. In contrast, while the experimental data do indeed temporarily increase in effect, the highest effect is found 46 subjects later than predicted. The final cumulative z-score, the end point of the curve, is met pretty well by the experimental data. Only 5.28% of all simulations lie closer to the prediction, which correctly predicted a more extreme value than the initial z-score.

Even though the experimental data are closer to the prediction than most simulations, they do not stand out in a statistically significant way. A closer look at the characteristic of the prediction increases the plausibility

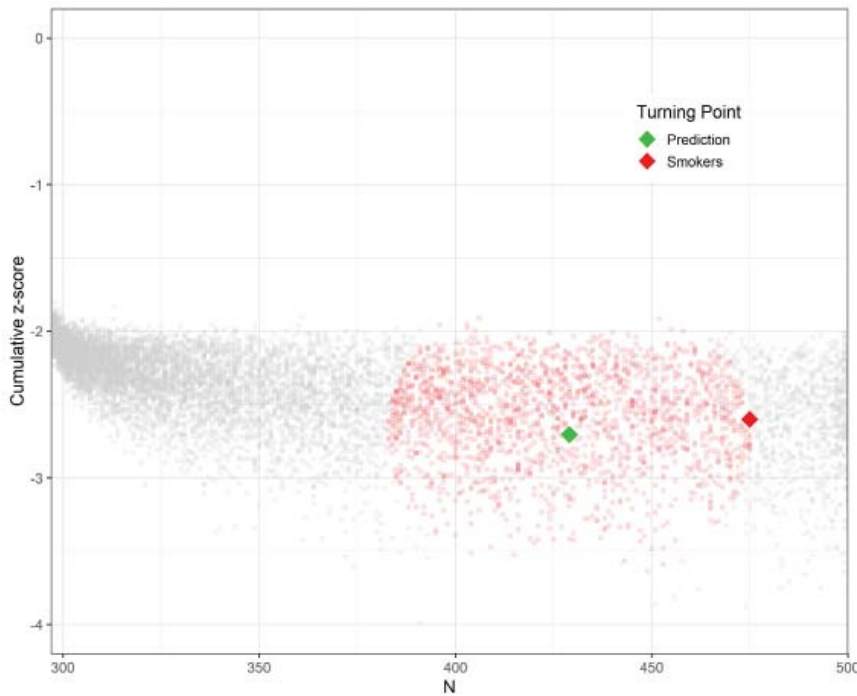


Figure 6. Overview of the turning points (local maxima) of all simulations (top rhomb is green and rhomb to the left is green; bottom rhomb is red and rhomb to the right is red). Red dots (brighter dots between 375 and 475) indicate simulations with a turning point closer to the prediction than to the experimental data.

of this finding. Although the prediction shows a subtle oscillating pattern, the development is not notably protuberant, meaning that it may not be sufficiently distinctive to be able to reliably distinguish between random and non-random data. Therefore, it remains inconclusive as to whether the temporal development of a micro-PK effect follows the pattern of a dampened harmonic oscillation closely enough to derive a prediction about its future progress.

Exploratory Results

Since the pre-registered confirmatory analyses of Study set 3 yielded no definite results, we decided to further explore the unambiguously extraordinary effect development of the smoker subsample over the course

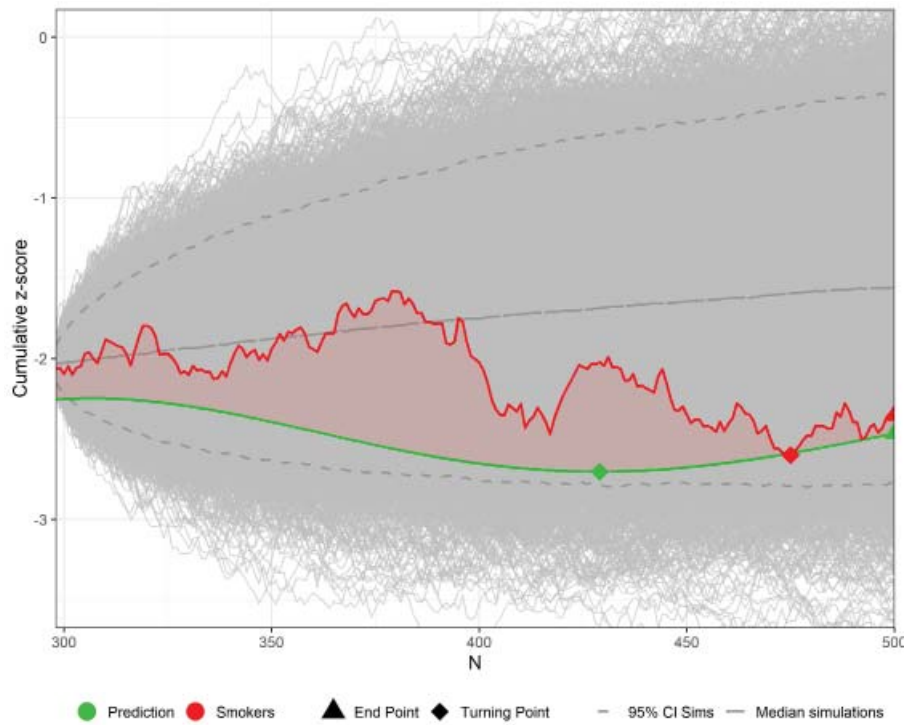


Figure 7. Overview of all confirmatory analyses conducted for Study 3. Here, the gray lines represent random simulations; their median is the center, gray, long-dashed line, while the outer, gray, short-dashed lines represent the borders in which 95% of all simulations lie. The red line (top curve) represents smokers' experimental data, and the green line (bottom smoother curve) the prediction. The red shaded area (darker gray shading) visualizes the area under the curve between the experimental data and the prediction. The rhomb represents the turning point and the triangle the end point for both the experimental data and the predictions.

of all 500 participants. Arguably, the best indicator of evidence of an effect at any moment is the *BF*. This test statistic can be used sequentially and gives a precise estimate of the probabilities of two competing hypotheses at every data point. The *BF* is consistent, meaning that it will give a more precise answer the more data it considers, even if the null hypothesis is true (Rouder et al. 2009).

For this reason, we decided to take a closer look at the temporal change of the *BF*s of both subsamples (smokers and non-smokers) before comparing

TABLE 2
Results of Confirmatory Analyses of Study 3
in Comparison with 10,000 Random Simulations

	Result	% of Simulations with a Better Result
Area under curve	85.22	18.71
Turning Point	0.23	19.42
End Point	0.11	5.28

them to another set of simulations. These simulations were generated in the same way as before, though on this occasion containing 200,000 bits for a total of 500 data points (400 trials \times 500 subjects per simulation). Again, we generated 10,000 simulations using the tRNG device, summed up the bits, and calculated two-sided sequential Bayesian t -tests with a prior of $\delta \sim$ Cauchy (0, 0.5) (see Figure 8) (Table 2).

Highest BF reached. As can be seen in Figure 8, the smoker subsample ($BF_{10} = 421.22$ at $N = 134$) peak is remarkable. Only 0.21% of all simulations showed a higher BF at any point. This is in stark contrast to the non-smoker subsample, which resembles a nearly perfect null curve and at no point rises above a value of 1 in the H_1 direction (as do 40.56% of all simulations).

BF Energy. The overall orientation of the curve is also of interest, much like the highest peak. The overall area between the baseline at $BF = 1$ and the empirical sequential BF curve above and below that line, where deviations above $BF = 1$ receive a positive sign and below a negative sign, can be described as its *energy* (see Figure 9). For the smoker subsample, this area was: $AUC_{\text{Smokers}} = 7981.73$, this is surpassed by only 0.15% of all simulations (mean $AUC = -291.96$, $SD = 1485.19$). Non-smokers show an Energy of $AUC_{\text{Nonsmokers}} = -323.35$, which is surpassed by 95.95% of simulations cut after the non-smoker N of 385 (mean AUC for these simulations is -208.28 , $SD = 1375.05$).

Frequency Spectrum analysis. The Energy of the curve does not account for periodicity as a characteristic, however. As this is the core idea of the Maier, Dechamps, and Pflitsch (2018) revision of the MPI, which postulates systematic variations of micro-PK effects over time, we then applied an analysis of periodicity to our dataset. One such way to gain a deeper understanding of the oscillative nature of our empirical BF curve

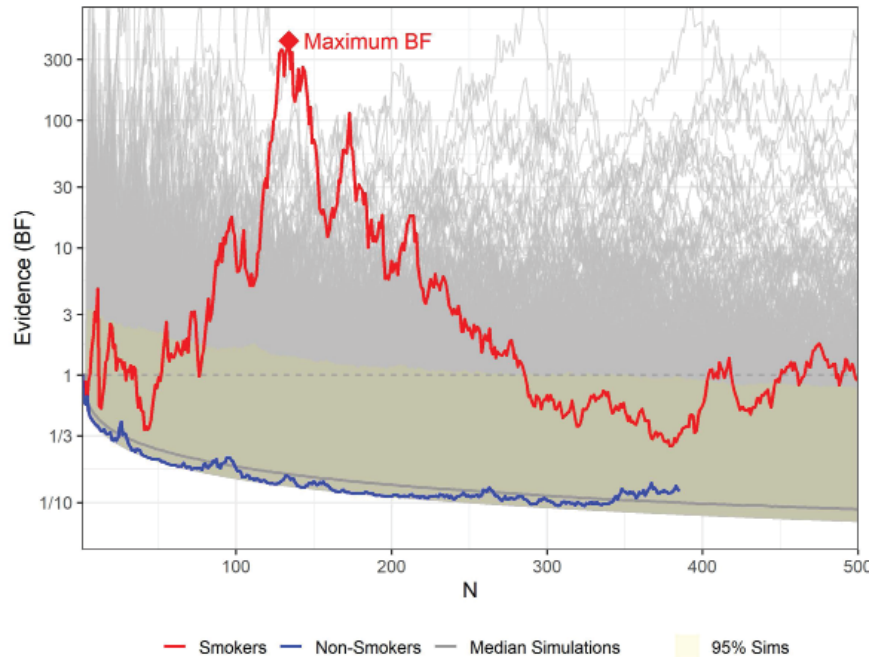


Figure 8. Sequential Bayesian analyses of both subsamples (Studies 1–3) in comparison with 10,000 simulations comprising random data (muted gray lines). Overall, 95% of simulations are located in the yellow area (the slighter lighter gray shading roughly under 1); the median of the simulations is represented by the single bottom gray curve line (near the bottom).

is by applying a Fourier transformation. This analysis converts a time-dependent function to a representation of its composited frequencies and can be understood as a harmonic analysis of the input. The resulting curve—the Fourier transform—shows the amplitude of all frequencies comprising the original input sequence (Penrose 2017). Accordingly, high amplitudes indicate the presence of pronounced periodic elements within the original curve.

A fast Fourier transformation (FFT) was conducted on the sequential Bayesian analysis of both subsamples, as well as on all 10,000 simulations. Simulations that were used for the comparison of the non-smoker subsample to the smoker subsample were cut after $N = 385$ data points. Sampling rate was $1/N$ in each case. The transform shows the amplitude at every point of the sampling rate. As it is symmetric, only the first half of the transform is considered. Remarkably, the spectrum analyses of the subsamples differ

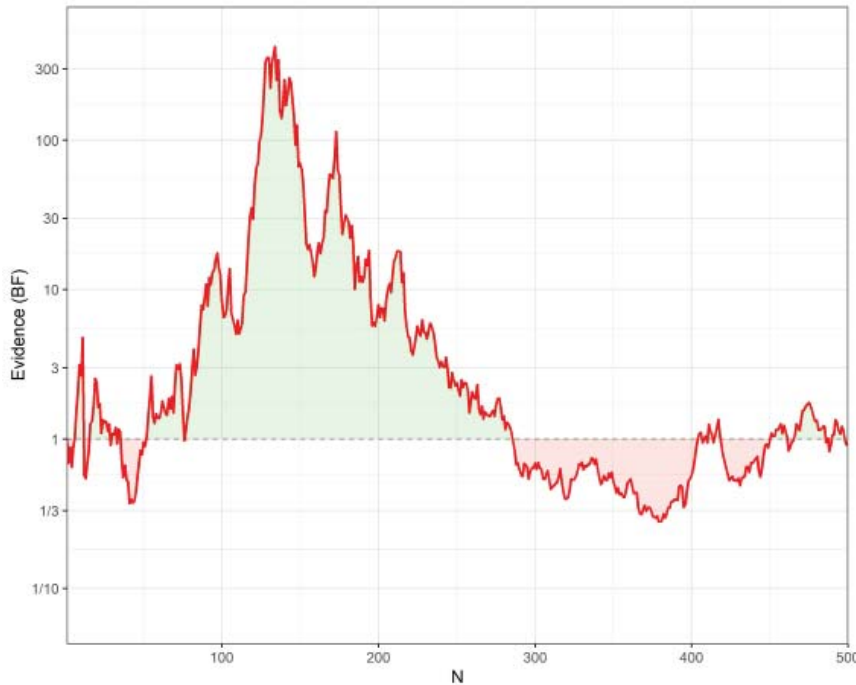


Figure 9. Representation of the curve's Energy. Parts below $BF = 1$ (red [lighter gray shading]; direction toward H_0) are subtracted from parts above 1 (green [darker gray shading]; direction toward H_1).

tremendously: The smoker subsample shows a very high amplitude at almost every frequency (see Figure 10a for the first 50 frequencies); comparatively, the non-smoker subsample shows very small amplitudes (Figure 10b). Compared to the simulation data, 245 of 250 frequencies (98%) of the transform of the smokers' data have a higher amplitude than 99% of all simulations, while all frequencies (100%) have higher amplitudes than 95% of simulations. Comparatively, none of the frequencies of the non-smoker subsample show an amplitude in the top 5% of simulations.

Discussion of Exploratory Results

All exploratory analyses revealed very promising results in their ability to distinguish between the smoker and non-smoker subsamples and simulations. The latter illustrated that the occurrence of a BF_{10} of 421.22 over the course of the data collection stage is quite exceptional; indeed, it can be expected with a probability of only about 0.21% using truly random

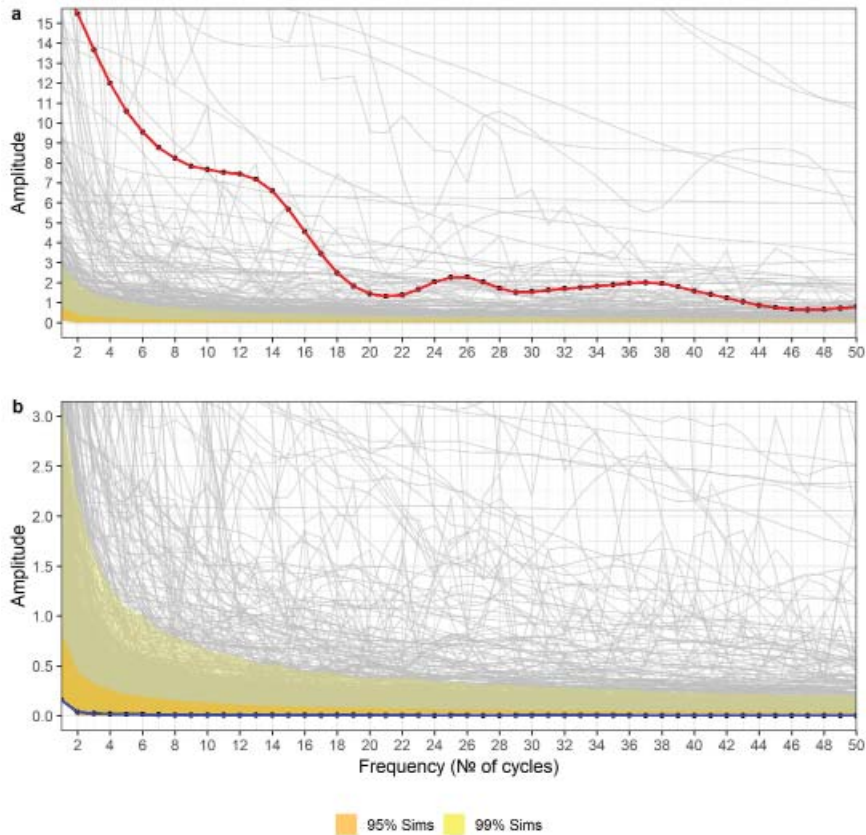


Figure 10. FFT of Bayesian sequential analyses of the smoker subsample (red [dark curve on the top graph a]) and 10,000 simulations, and the non-smoker subsample (blue [dark line on the bottom graph b]) and 10,000 simulations. The orange (95%) and yellow (99%) areas (near the axes) represent the simulations' empirical confidence bands (darker and lighter shadings near the bottom of the graphs).

data. The *BF* is designed so that it yields more precise information as more data are added. This strategy reaches a limit, though, when the tested effect is not consistent in its strength but volatile, as might be the case with ψ effects.

Considering this, it might be fruitful not only to consider a single *BF*, but also to closely examine its development. The difference of the sums of all *BFs* going in the direction of either hypothesis may also provide valuable information; this is calculated by subtracting the correspondent areas from one another, and can be described as the curve's energy. The energy of the

sequential Bayesian analysis of the smokers' data is even more unusual than its highest BF and is surpassed by only 0.15% of all simulations. Conversely, non-smokers' energy resides in the bottom 5% of simulations. This BF curve is, therefore, oriented remarkably strongly toward the direction of the null hypothesis. The data seem to be 'more random' than most random datasets.

Finally, from our perspective, it is important to account for the possibility of an oscillating nature regarding ψ effects. Effects like micro-PK might be possible in general; however, they are likely being counteracted by an opposing force at some point. This interplay should present itself as a periodic pattern of the effect (Maier, Dechamps, & Pflitsch 2018). As a Fourier transformation is able to extract the harmonic structure of an input signal, it might therefore be suitable as an instrument to reveal such non-random oscillating patterns. A remarkable difference can be noticed when this is applied to the sequential Bayesian analyses of both subsamples and their corresponding simulations; the amplitudes of the frequencies—meaning their significance in comprising the input signal—show a much higher value in the smoker subsample. When compared with 10,000 simulations, this transform features amplitudes in the top 1% in nearly every frequency. This means that it is highly unlikely to be obtained from a truly random BF development.² Comparatively, the non-smoker transform on the other hand shows a pattern similar to a typical null-effect simulation, and does not stand out in any way.

General Discussion

A certain degree of experimental evidence for ψ effects is unquestionable (Cardeña 2018). Nevertheless, successful replications of ψ effects, such as micro-PK effects, are scarce, which is demonstrated exemplarily in the first two studies of this experiment (Maier & Dechamps 2018). It seems that these effects evade classical detection through replication. Different ideas about the elusive nature of ψ have emerged, one example of which is the No-Transmission Axiom of the Model of Pragmatic Information (von Lucadou, Römer, & Walach 2007). The authors of this model conclude that ψ effects in general cannot be tested with specific predictable outputs and are, therefore, not accessible to reproducibility-oriented research (von Lucadou 2006).

A more tangible equivalent to these considerations is grounded in the quantum nature with which ψ effects are usually brought into connection. A systematic deviation from quantum randomness violates the No-Communication theorem and the Second Law of Thermodynamics because information is created from nothing and could be used to transmit signals. This temporary decrease in entropy could lead to an onset of a counter-

mechanism whose task it is to restore randomness, dwindle the information away, and increase entropy. There is one critical difference from the MPI, however: We predict that the interplay between effect and countereffect leads to a systematic pattern in the development of the effect itself; in principle, this is something that can be predicted and tested (Maier, Dechamps, & Pflitsch 2018, Maier & Dechamps 2018).

Our first instinct was to assess the z-score as this embodies the standardized deviation from a given value: in this case, from chance. We hypothesized that effect and countereffect should balance in a form comparable to a dampened harmonic oscillation and applied a correspondent curve-fitting algorithm to experimental data showing a rise and decline of micro-PK. In doing this we derived a prediction about the behavior of the cumulative z-score of future data. At this point, no definitive conclusion can be drawn regarding the goodness of the prediction. Experimental data of 203 further participants in the micro-PK study fitted the prediction better than most simulations according to three different test statistics; however, they did not stand out in a significant way. This is potentially attributable to the lack of distinctiveness, a characteristic that makes it easy for random data to match the prediction.

Subsequently, we aimed to find other ways of assessing the development of the effect on a post hoc basis. We decided to favor the sequential Bayesian analysis over the cumulative z-score because the former represents more sophisticated information regarding the state of effect at every point of the experiment. We felt that the evidence toward one of two hypotheses better matches the knowledgeable information of the presence of an effect, which should comprise the basis for the interplay with the entropic countereffect. This development went hand in hand with new methodological possibilities that arose during the course of the experiment, specifically the extraction of individual *BFs* and the execution of FFTs.

Analyzing the development of the *BF* has proven to be a very promising approach using the data from this series of experiments. The maximum *BF* expresses the climax of the initially present micro-PK effect, whereas the curve's energy gives an indication of the overall distribution of the *BFs* with regard to the competing hypotheses. Both these methods could reliably distinguish between an experimental sample and a control group or simulated data at a 1% level. In particular, we would like to emphasize the method of a Fourier transformation of the sequential Bayesian analysis. This approach assesses the existence of a periodicity in the data, which is the fundamental idea behind an interplay of effect and countereffect. The transform of the experimental sample showed a behavior that fully supports this claim. The remarkably high amplitudes seen for nearly every frequency

suggest the presence of a strong oscillative element only in the dataset where micro-PK is expected. The transform stands out from both the control group and random simulations. Meanwhile, all three methods have been applied to a different high-power and within-subject, micro-PK study; once again, they show very promising results (Dechamps, Maier, & Pflitsch 2019).

Psi effects such as micro-PK repeatedly present us with scientific challenges. This circumstance has led some people to conclude that the classical epistemological standpoint—which demands the replication of results to establish a proof—is not applicable within such a context. While we agree that current standard methods might not be suitable when securing comprehensive evidence for volatile effects, such as psi, we propose a way out without giving up testability and predictability. The goal, therefore, must be to move away from considering only end results and toward a closer investigation of temporary change and the development of psi effects themselves.

Notes

- ¹ Study preregistration is available at <https://osf.io/wn5b7>
- ² A Bayesian binomial test revealed an ‘infinitely high’ BF toward the hypothesis, indicating that the proportion of top 1% frequencies is far higher than the 1% chance base rate.

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RESEARCH ARTICLE

A Multi-Frequency Replication of the MegaREG Experiments

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Abstract—The MegaREG set of experiments run by the Princeton Engineering Anomalies Research (PEAR) group prior to 2004 found that the response to human intention was positive when the Random Event Generator (REG) bit-rate was 200 bits per trial, but increased in magnitude and was negative in direction when the bit-rate was 2 million bits per trial. This reversal of the influence could not be explained within the existing theories of psi influence, and for several reasons this avenue of investigation was terminated. Given that this effect might represent a clue to the underlying structure of mind–matter influence, the current study set about replicating and extending the MegaREG experiments by examining the influence of human intention over a range of 10 different frequencies from 200 bits per second to 16 million bits per second, on a new, purpose-built REG machine. The study used commercially available REGs, covered 127 series of 1,000 trials each, and was undertaken mainly by 5 operators over a period of 18 months, following protocols similar to those of the PEAR study. The results are ambiguous with respect to the reversal of influence at a high bit-rate but appear to support the MegaREG findings of an increase in effect size with bit-rate, though below statistical significance. It is concluded that further work should address this apparent effect amplification, as any increase in effect size with bit-rate would be of undoubted value to mind–matter investigations.

Keywords: consciousness—anomalies—human–machine interaction—random event generators—replicability

Introduction

In the PEAR (Princeton Engineering Anomalies Research lab) mind–matter experiments, operators tried to “influence” random event generators (REGs). Random event generators can be set up to produce binary bits, 0s

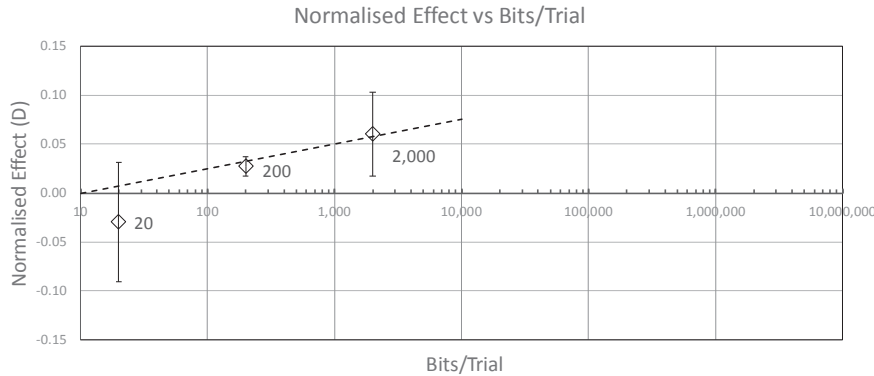


Figure 1. Pilot tests giving the suggestion of a bit-wise amplification of effect.

and 1s. The expectation from chance is that half the output will be 0s and half will be 1s. In each experimental session, the volunteer-operator first chooses and records what they want: more 0s or more 1s. They initiate the process and try to influence the REG in the chosen direction. While there are many variants, a typical session may last 15 minutes, and comprise 1,000 separate trials, each lasting about 1 second. Each 1-second trial may involve the generation of 200 bits, about one hundred 1s and one hundred zeros. The actual number is recorded and shown in a display that is updated each second, so the volunteer can see how they are doing. PEAR results show that over many thousands of trials there is a small but consistent residual effect that is in line with human intention and is not expected by chance (Dunne & Jahn 1992, Jahn et al. 1997, Jahn & Dunne 2005).

Pear's Motivation for the MegaREG Study

The postulate of what was physically happening, was that each bit was being “forced” by the mind in the direction of the operator’s intention. If the force was the same for every bit, then having more bits in the process would result in a bigger effect. This idea was called the “bit-wise” effect which was anticipated by at least one author to scale as the square root of the number of bits. To test this idea, PEAR ran some pilot sessions with 20 and 2,000 bits/trial. The results are shown in Figure 1 and suggest that there indeed could be a bit-wise increase in effect.

PEAR's MegaREG Experiments

The original MegaREG experiment by a PEAR visiting scholar (Ibison 1998) attempted (among other things) a more thorough test of the bit-wise–

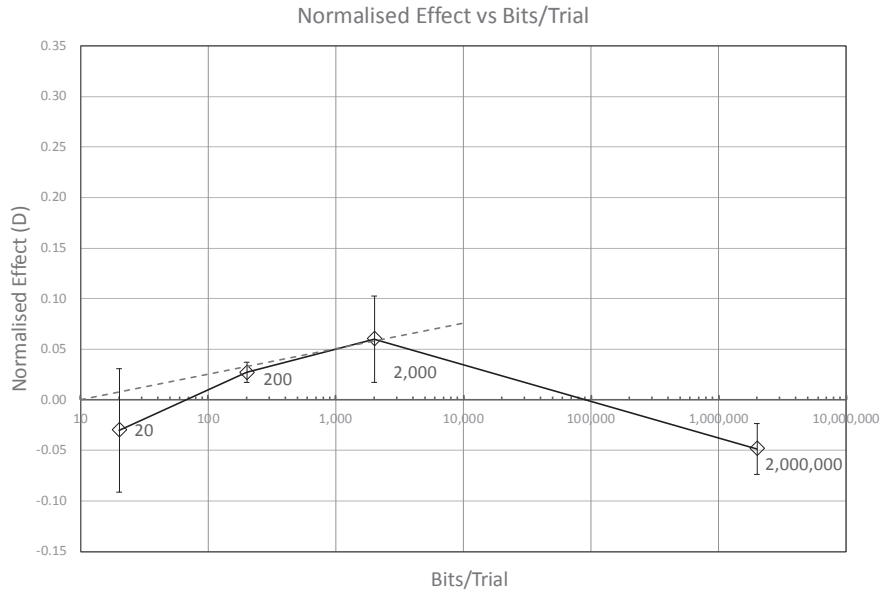


Figure 2. PEAR results for the MegaREG study.

effect hypothesis. This had the REG running at 10 MHz with the capability to source robustly random samples at 2 million bits per second. It was designed to display the results the same way as in the traditional PEAR tests at 200 bits per second. The MegaREG experiment also had the capability to source trial bits at 2 million and 200 bits per second in the same run. That is, in a given 15-minute session there might be half the trials at 2 million bits per second and half at 200 bits per second. Which of the two was presented to the operator was itself decided by a random process that was invisible to the operator. The subsequent analysis could separate any difference between the two frequencies, for the same intention. Because all bits were sourced from the 10 MHz REG, they each took the same interval of time for their production, and in the forcing model would each gather the same force (Dobyns et al. 2002, 2004).

The challenging results are shown in Figure 2 and listed below:

1. The high bit-rate (2MHz) gave results **opposite** to intention. This was unexpected.
2. The interleaved lower bit-rate (200Hz) gathered in the same runs gave results **in line** with intention (and in line with previous findings).

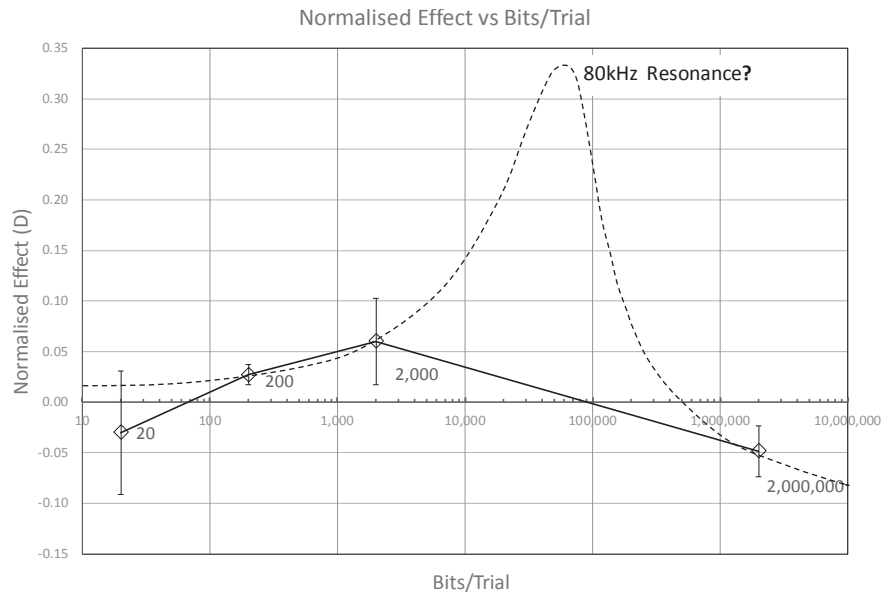


Figure 3. A possible interpretation of the MegaREG results.

3. Assuming the “forcing” model, the bit-wise effect size at the high bit-rate would be expected to be at least ten times that of the lower bit-rate. Instead the bit-wise effect decreased by 33 times.
4. On the other hand the effect size per trial was **larger** by 2.77 times than that of the 200 Hz bit-rate.
5. “The cause of the increase in the effect size [per trial] and inversion with respect to intention is unknown” (Dobyns et al. 2004).

Motivation for this Replication Study

The original motivation for the MegaREG experiment was to find out if a statistically significant mental influence on a random process could be achieved more quickly by gathering data at much higher rates than had been done in the past. Both Ibison and Dobyns et al. (2004) had this as a stated objective for the MegaREG experiments. The author of this replication, with the same motivation, was taken by the MegaREG results, because:

1. It was clear from the PEAR conclusion that the mechanism was still unexplained.
2. MegaREG appeared to provide evidence of some (if small) **signal amplification** per trial that was **bit-rate-dependent**.

3. The inversion anomaly suggested there might be an **optimum** bit-rate frequency somewhere.
4. In particular there could be a beneficial **resonant bit-rate** frequency with a large amplification of the effect, potentially as illustrated in Figure 3.

After visiting PEAR and discussing the MegaREG results with them, I decided to proceed with an independent replication of the MegaREG study.

Objectives of This Replication Study

1. Replicate the MegaREG experiments.
2. Test the hypothesis that the bit-wise effect is present at yet higher frequencies than 2 MHz.
3. Test the hypothesis that there is some optimum operating bit-rate.
4. Test the hypothesis that the reversal of the effect at high frequencies is a replicable result.

Quantifying Effect Size

The normal way to measure the effect in mind–matter experiments involving binary strings, is to measure the displacement δ of the trial distribution mean from the mean expected by chance. For the “Hi” and “Lo” intentions, the effect size would be represented respectively by:

$$\delta_h = \mu_h - \mu$$

$$\delta_l = \mu - \mu_l$$

The MegaREG work used the tripolar protocol which meant that each trial involved one set of “Hi” intention, one of “Lo”, and one of “BL” or baseline. This tripolar protocol was developed to guard against irregularities in the REGs of the time. But the fact that there were the two influence effects being gathered at the same time, Hi and Lo, allowed a “Figure of Merit” to be developed as follows:

$$D = \frac{\mu_h - \mu_l}{\sqrt{2}}$$

Here the difference between the Hi and Lo means $\mu_h - \mu_l$ is equivalent to the sum of the magnitudes of the two displacements of the means δ_h and δ_l , that is:

$$D = \frac{\delta_h + \delta_l}{\sqrt{2}}$$

where: D = the Figure of Merit, a measure of the size of the mental influence

μ = Mean expected by chance

μ_h = Mean of all trial results with Hi intention

μ_l = Mean of all trial results with Lo intention

δ_h = Mean shift from chance expectation, in the “Hi” direction

δ_l = Mean shift from chance expectation, in the “Lo” direction

Assuming the displacement of the mean was equal in size and opposite in direction for the Hi and Lo cases, then D is twice the effect size in one direction only.

Figure of Merit for This Replication Study

In this replication study, the tripolar protocol was not used. Operators could choose their direction of intention and some operators adhered to a single direction throughout all their trials. The output in this study was only recorded as positive in the direction of intention and negative in the direction opposite to intention. The result is a mean shift that is half the size of the MegaREG “Figure of Merit.” To be on the same footing as the MegaREG study, the replication study mean shift, d_R , was doubled to get an equivalent figure of merit size for comparison.

Taking into account the square root of 2 in the MegaREG figure of merit, the equivalent figure of merit in the current replication study is:

$$D_R = \sqrt{2} \delta_R$$

where: D_R = Normalized Figure of Merit for the Replication Study

δ_R = Mean of a set of trial distributions in the direction of intention

Normalizing the Effect at Different Bit-Rates

Since different bit-rate frequencies will result in different mean sizes, the MegaREG results were “normalized” so that the mean trial output at any bit-rate would be represented in the same way as the output from the 200 bits/second standard tests used by PEAR. The normalization extended to adjusting all standard deviations and measurement uncertainties to a common size for direct comparison. This allowed the effect size per trial to be readily compared across all bit-rate frequencies. The normalization equation for trial means is:

Normalized Mean



Figure 4. The REG source for the replication machine.

$$\mu_N = \left(\sqrt{\frac{BPT_{ref}}{BPT}} \right) \times (\mu_{BPT} - 0.5 \times BPT) + 0.5 \times BPT_{ref}$$

where:

μ_{BPT} = Mean of trial score distribution at a second bit-rate

μ_N = Normalized mean trial score distribution for the second bit-rate

BPT = Bits per Trial at the second bit-rate

BPT_{ref} = Bits per Trial for the reference bit-rate (in this case $BPT_{ref} = 200$ bits/trial)

REG and Interface Specifications for the Replication Study

The REG for the replication machine comprises $4 \times$ Quantis, 4-MHz, certified, quantum random number generator units combined on a single board as in Figure 4, interfaced with a specially made board in a dedicated computer. For the tests described here, the 4 Quantis units ran in parallel (on “Sample Rate” setting 16 MHz in Figure 5), generating essentially a 16-million-bit array every second (and every trial), which was then sampled at regular intervals to deliver the chosen bit-rate. So for example if the chosen bit-rate was 200 bits/sec, only every 80,000th bit would be selected for the sum.

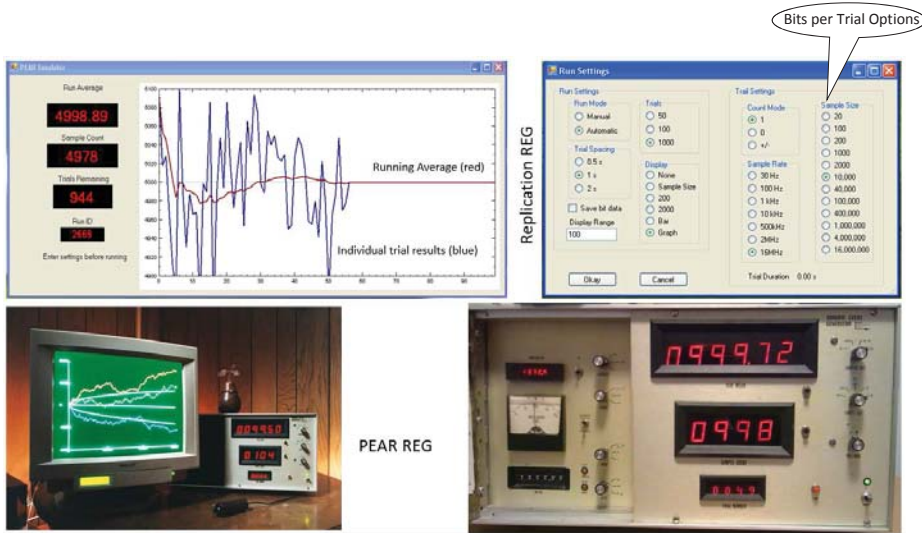


Figure 5. The replication machine interface above and the original REG machine at PEAR.

Efforts were made to reproduce the main functions of the Princeton machine with parallel functions on the replication machine. Table 1 compares the functions of both machines and their setting options. The difference of note is that while the PEAR machine has 4 bit-rate options from 20 Hz to 2 MHz, the replication machine has 12 bit-rate options (or “Sample Size” options in Figure 5) from 20 Hz to 16 MHz. This facilitates the search for any optimum bit-rate frequency between 20 Hz and 16 MHz. Figure 5 shows what the machines look like and how their interfaces compare. As can be seen, the replication machine displays both a two-part graphical representation (cumulative deviation and by-trial deviation), as well as a digital score to the left of the graphical screen.

Experimental Protocols

While PEAR used a tripolar protocol, this was largely to deal with the vulnerability of their machines to a systematic bias. Although that was appropriate at that time, the quality of available REG machines has improved since then, and the Quantis machines in particular have proven reliable in both commercial quantum cryptography (id Quantique 2018), the full battery of Die-Hard tests, and in other mind–matter experiments (Radin 2006). For these reasons it was decided in this replication to put a level of trust in the machine, and to reduce the amount of operator effort to

TABLE 1
Functions of the Princeton REG Machines Compared with the Replication Machine

FUNCTIONS	PRINCETON REG MACHINE(S)					MEGAREG					CANTERBURY REPLICATION MACHINE	
	Signals & Controls	Settings Options	Man	Auto	Other	Signals & Controls	Settings Options	Man	Auto	Other		
1 Trial Mode (User starts every trial or automatic start)	Knob		Man	Auto								
2 Sampling Rate (Hz)	Knob		10	100	1k	10k						(Red is default)
3 Sample Size (bits)	Knob		20	100	200	1k	2k					30 100 1k 10k 500k
4 Calibrate	Knob		1	0	+/-							20 100 200 1k 2k
5 No of Trials per Run	Knob			50	100	1000						1 0 +/-
6 Count what?	Knob			REG	Ext	Test						Click in Setup Window (Same as Trial)
7 Source	Knob			Press								Click in Setup Window N/A. Only one source
8 Start Run or Trial (depending on Man/Auto)	Button (Start, Red)											Click in Run Window
9 Trial Counter (Reset? Hide?)	Button (black)											Automatic On
10 Reset Run Mean (Reset? Hide?)	Button (black)											Automatic On
11 Trial Number	Display											Run Window
12 Present Trial "Sample Count" of +/- or +/-	Display											Run Window
13 Run Mean	Display											Run Window
14 Temperature (for REG noise source stability)	Display											
15 Bar Graph Option	Display											
16 Real time Graphical trial scores & cumulative Run Score	Display											
17 Trial duration (s)	Display											
18 Power on/off	Switch		On	Off								
19 Farenheit/Centigrade	Switch		F	C								
20 Meter Switch	Switch		Volt	Amp								
21 Power on/off	Light (Green)		On	Off								
22 Error Voltage	Light (Orange)		On	Off								
23 Error Count	Light (Orange)		On	Off								
24 Set at expected Mean for this Run (Called "Initial")	Manual set Counter		On	Off								
25 Voltage or Current	Meter											
26 External Source input	Thermal noise source											
27 REG	Automatically recorded											
28 Memory of Trial Summary												

Click in Setup Window
N/A. Only one source

Click in Run Window
Automatic On
Automatic On
Run Window
Run Window
Run Window
Run Window
Run Window
Run Window

Trial aborts on Error

Custom Card
4 off certified 4MHz ORNG units ex Quantico; in 16MHz configuration
Always recorded automatically

get results compared with the tri-polar protocol. For the same reason, there was no modification, or “XORing” of the certified bit-stream to deal with potential systematic bias. The possibility of systematic bias was checked at the end of the series by making use of the calibration runs data collected at the end of each volunteer session.

In another departure from the PEAR protocols, operators in this replication could opt for a Hi or Lo target for any run, and were not constrained to have balanced portions.

Other processes for the operators were largely similar to those of PEAR. Operators recorded their intention before each run (“Hi” or “Lo”), and recorded which of the 12 available bit-rate frequencies they wanted to use. They worked for about 1 hour, doing three or four 1,000-trial runs. Typically they would do no more than one session per week.

Regular calibration runs were initiated by the operators each time they finished a session and left the room where the REG was situated.

Anatomy of the Replication Dataset

The replication study comprised 127,000 trials over 18 months, with runs primarily of 1,000 trials each. They covered 10 different bit-rate frequencies from 200 bits/trial to 16 million bits/trial. There were 13 operators in all, with 5 people with more than 10,000 trials each. There were 7 females, 4 males, including 2 paired sets of volunteers among the group.

Experimental Results

Results have been presented in Figures 6 and 7 as normalized effect size D_R on the vertical axis, so that they are directly comparable to the MegaREG results in Figures 1 to 3. It quickly became apparent that the only appropriate horizontal axis was a log scale, and this has been used on all figures including Figures 1 to 3.

In line with the MegaREG study, the error bars in the figures represent a 1 SD, standard error. To represent statistical significance at $p = 0.05$, these error bars would need to be 1.96 times larger. With this in mind it is easy to read off the figures which data points are statistically significant at better than the $p = 0.05$ level. For example in Figure 6, only two data points are significantly different from no effect at all, and these are at bit-rates of 1,000,000 bits/trial ($p = 0.04$) and 16,000,000 bits/trial ($p = 0.004$).

Combined results of all operators are shown in Figure 6, and individual operator results are shown in Figure 7.

Systematic bias in the random number source was investigated by analyzing the calibration runs collected when no one was near the machine

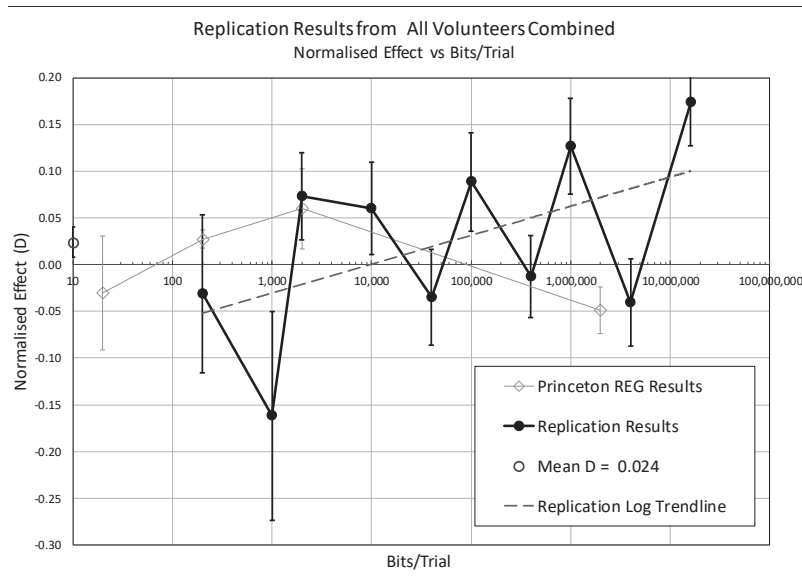


Figure 6. Results from all volunteers combined, overlaid on the MegaREG results.

or trying to influence it. The results in Table 2 show all bit-rate calibration runs, as well as the combined calibration runs.

Discussion

This study covered 10 different bit-rates rather than the two in the MegaREG study. As a result it was not realistic to run a program of tests long enough to get statistical significance at each bit-rate or for each operator. Consequently, the results, while still indicative, mostly do not have the authority of full statistical significance. This study therefore should be seen more in the nature of a pilot study that needs a follow-up study for confirmation.

Bit-Wise Effect

One of the objectives of the MegaREG experiments was to determine whether or not there was an increase in the effect of intention with bit-rate frequency, or a “bit-wise effect.” The most prominent feature in Figures 6 and 7 of this replication study is an overall increase in size of the intention effect with bit-rate up to 16 MHz. While this is in line with the MegaREG experimental findings, the increase with bit-rate per se of this replication study is not confirmed at a significant level:

Figure 7. Results from individual volunteers who contributed 4,000 trials or more.

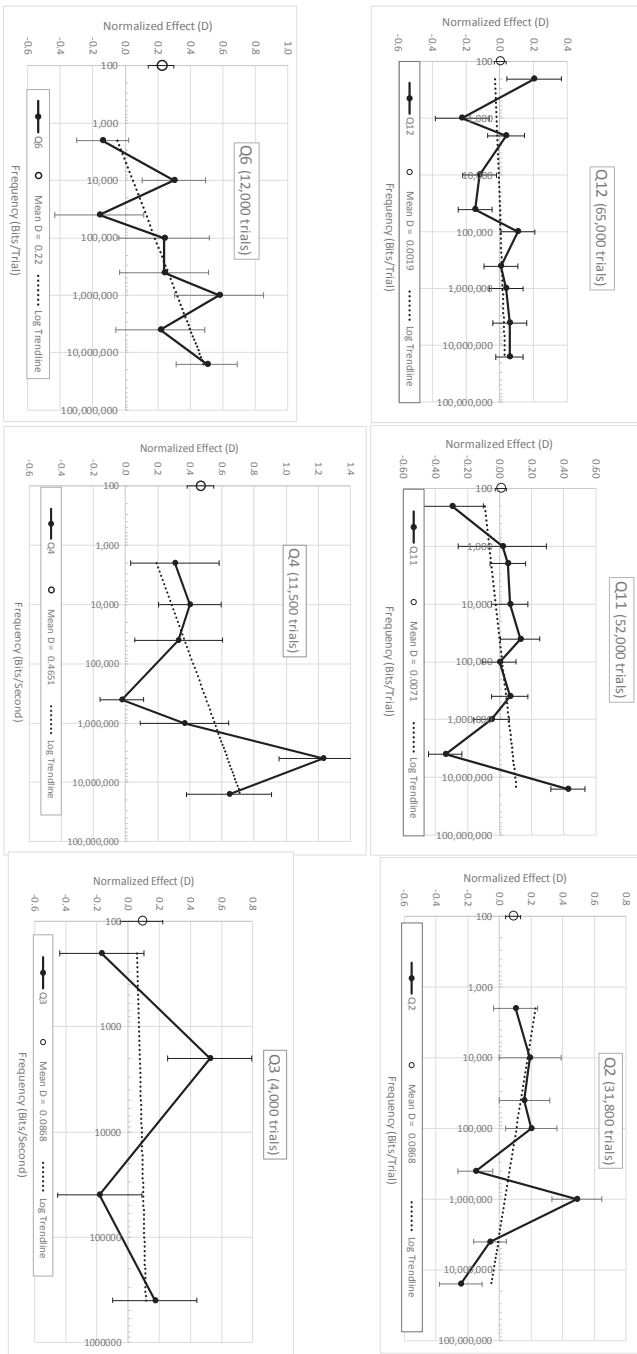


TABLE 2
Analysis of Calibration Runs

Bit-Rate (per sec)	200	1,000	2,000	10,000	40,000	100,000	400,000	1,000,000	4,000,000	16,000,000	Combined
No of Calibration Trials	16000	12000	9000	10000	10000	13000	81000	11000	23000	26000	211,000
Z	1.19	-0.90	-0.71	-0.01	-0.59	0.32	1.11	-0.41	0.95	-0.84	-0.39
p	0.116	0.817	0.762	0.503	0.722	0.374	0.134	0.658	0.172	0.799	0.651

- In Figure 6 the probability that the slope is greater than zero is $p = 0.3$.
- In Figure 7 the probability that the slope is greater than zero is $p = 0.09$, based on 5 out of 6 trendlines showing the increase.

Taking this as a pilot study, it is enough to inspire further work, given that it suggests a way to amplify effect size.

Signal Strength of Bit-Wise Effect

The MegaREG study, as reported in Dobyns et al. (2004), calculated that the effect size at 2 million bits per trial was 2.77 times the effect at 200 bits per trial. This amounted to an effect size of about -0.05 . For the replication study the magnitude of the effect at 2 MHz shown by the trendline in Figure 6 is about $+0.06$. Nevertheless, until statistically significant results can be achieved this is largely speculative.

Intention-Effect Reversal with Bit-Rate

An issue that was confounding for the MegaREG experimenters, as reported in Dobyns et al. (2004), was the reversal of the effect at the 2 MHz bit-rate, the highest bit-rate of their tests. It would have been ideal if this replication study had shown that in all probability this reversal was a software error, or, on the other hand, that it was part of a replicable trend. Unfortunately, it did neither. Instead it gave two opposing indications, neither with adequate statistical significance:

1. The overall trendline in Figure 6 suggests a steady increase in effect size right up to 16 MHz, well beyond the 2 million, maximum bit-rate of the MegaREG tests. This would seem to challenge the reversal in effect in the MegaREG experiments.

2. The combined operator bit-rate results in Figure 6 show that the nearest point to the MegaREG 2 MHz point, (being at 4 MHz), is a similarly negative result, albeit without statistical significance. This could mean that there is a local minimum around the 2 to 4 MHz region that cannot be confirmed with the relatively low precision of this replication study.

These conflicting points leave unanswered the question of whether the reversal of the effect at 2 MHz in the MegaREG experiments has been replicated or not. Again, taking the replication as a pilot study, this reversal issue is worth exploring further with a more focused study.

Presence of an Optimum Bit-Rate?

In Figure 6, of the combined results of all operators there is subjectively no obvious indication of an “Optimum” or “Resonant Frequency bit-rate” peak that looks like the dotted addition in Figure 3.

Similarly in Figure 7, of the individual operator results there is no clear indication of an “Optimum” or “Resonant Frequency bit-rate.” If there had been, it would appear as an increased effect across all participants at one particular bit-rate, but this is not the case.

These results give no indication that there is any value in putting more effort into looking for an optimum within this bit-rate range.

Experimenter Effect

The potential for experimenter influence needs to be addressed. It is known that the attitudes and beliefs of the experimenters themselves as well as of the operators may affect the magnitude and/or direction of the results (Kennedy & Taddonio 1976, and others). Ideally, experimenter expectations, like conflicts of interest, should be declared before starting the experiment. Although it was not done for this replication, it is still worth noting after the fact as follows.

The main hope of the author, who was the lead experimenter but not an operator, was to discover that there is an increase in effect-size with bit-rate. To a lesser degree, the author’s hope was that the negative effect at high bit-rate in the MegaREG experiments would turn out to be an artefact such as a software error. The possibility of an optimum was of lesser interest.

While the author’s hope for an increase in effect size with bit-rate has occurred, the reversal effect at high bit-rate in the MegaREG experiments is a bit more complicated. At this point it has not been convincingly replicated, nor convincingly denounced, by the replication study. But in

the author's mind this effect reversal is nonetheless satisfactorily explained as a direction-reversing, experimenter effect, arising from the conflicting motivations of the MegaREG experimenter team that became apparent to the author, only after talking with people at PEAR. The author from the outset believed that these conflicting motivations may have been the cause of the reverse-direction results in MegaREG, through an experimenter effect generated by a passionate team in conflict.

While it remains to be seen from future work whether the effect reversal can be replicated, at this point it is notable that the replication study results align fairly well with the expectations of the lead experimenter.

Calibration and Systematic Bias

Table 2 shows that the calibration runs vary to non-significant degrees. This supports the view taken in this study that the Quantis random number generator units are not introducing a systematic bias into the results.

Recommendations and Conclusions

This study was wide but not particularly deep and should be viewed as a pilot study. It addressed three of the questions raised by the MegaREG experiments:

1. Does the intention effect increase with bit-rate at yet higher frequencies than 2 MHz?
2. Is there some optimum operating bit-rate?
3. Is the MegaREG reversal at high frequencies a replicable result?

While not having the statistical significance to be definite, the following observations may be made:

1. The indications are that the intention effect does increase with frequency, but this needs to be confirmed with a more focused study.
2. There is no evidence of an optimum bit-rate in the range investigated. This suggests there is no value in spreading future operator effort over a number of intermediate frequencies in the hope of finding an optimum as was done for this replication study.
3. This replication raises a modicum of doubt about the MegaREG reversal, but this question, too, needs a more focused study before conclusions can be drawn.

Further work should be designed to provide the statistical significance necessary to answer the single question: Does the effect of intention increase with trial bit-rate? Answering this one question with sufficient confidence will address the two remaining questions 1 and 3 above.

In conclusion, this replication has explored several issues raised by the MegaREG experiments, and has focused attention on what can be done next to settle the remaining questions. Of particular importance is the indication of a monotonic increase of effect size with bit-rate. Should future work confirm this to be the case, it will be of undoubted value to mind–matter investigations.

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RESEARCH ARTICLE

The Location and Reconstruction of a Byzantine Structure in Marea, Egypt, Including a Comparison of Electronic Remote Sensing and Remote Viewing

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A video of these events can be seen at: <https://www.youtube.com/watch?v=kCzHhYYQjQ&t=47s>

Conference Presentations: *The Marea Probe: An Experiment in Applied Parapsychology Involving the Location, Reconstruction, and Excavation of a Byzantine Structure—Marea, Egypt*, Invited Paper, Annual Meeting of The American Research Center in Egypt, M. H. de Young Memorial Museum, San Francisco, April 14, 1980; Annual Meeting of the Parapsychology Association 1980, *Proceedings of the Parapsychology Association 1980*.

Abstract—This paper reports the location and reconstruction of a Byzantine structure in the now-buried city of Marea along the shores of Lake Maryut, some 44 km southwest of Alexandria, Egypt. A Pharaonic trade center that was occupied until the 16th Century, the city has been long abandoned and lies buried around what formerly was the lakeshore. This paper reports on an applied remote viewing experiment in which two remote viewers were asked to first locate Marea, and then a buried building within the city, and, finally, to describe what would be found within the building site selected, with a particularly emphasis on tile and other decorative material. It also includes a comparison of remote viewing data with electronic remote sensing, and geographical data for the same area done independently three years earlier. The comparison is striking because while the remote viewers were successfully able to locate a building, including staking out its door, and corners, as well as providing a wealth of reconstructive and descriptive material about what would be found at the site, the electronic remote sensing and geographical analysis produced no suggestion whatever that there was a site at this location. For this reason, prior to discovery, much of the remote viewing data seemed extremely improbable, and notably contradicted the informed judgment of an archaeologist deemed by the University of Alexandria to be the leading authority on Marea.

Introduction

When Alexander founded Alexandria in 331 BCE, Marea was a regional capitol and it remained a trade center active as late as the 16th Century. But all that lies in the past. Today Marea is hidden beneath a desert of low semi-arid hills, some 44 km southwest of Alexandria, Egypt.¹ Archaeology prior to this study had revealed little besides a cluster of foundations as a hint of its past.

The name Marea may be derived from the Pharaonic word *Per-Mert*, meaning: the country by the lake (Sadek 1978). If so, it was an apt name for what contemporaneous accounts describe as a lovely city with marble public buildings, situated to catch the breezes along the reed-lined, bird-filled shore of Lake Maryut, which served as a source of food, a means of transportation, and a place of pleasure, and the town was ideally positioned as a way-stop for travelers going up and down the country. The gentle airs, warm weather, and beauty both natural and constructed, also recommended Marea as a tourist attraction. The Greek geographer and historian Strabo (64 BCE–21CE) describes canals that emptied into the lake (Strabo 1989:69). The Shediya River, which originated from the Nile at Memphis also emptied into the lake, and there was access to the Mediterranean via a canal cut through to what became the Western Harbor of Alexandria.

Strabo says the commercial wines of the area were “so good that the Mareotic wine is racked off with a view towards aging it” (Strabo 1989:59). The vintages really must have been outstanding because almost 300 years later the physician Athenaeus of Attalia (1st century CE) would echo Strabo’s words saying, “The Mariotian wine . . . is excellent, it is white and pleasant, fragrant, easily assimilated . . .” (Athenaeus 1927).

Wine-making, though, was just one of the area’s activities. Authorities of the past also extol the virtues of the lake region’s olive oil, fish, papyrus, and fruit. Perhaps most famous of all, though—next to the wines—was the hand-blown glass produced in the city. It was so delicate that few examples have survived intact, and Mareotian glass is among the most prized possessions of Alexandria’s Greco–Roman Museum.

Although commerce and pleasure were its major contributions, Marea also played a political and strategic role in early Egyptian history. Herodotus (BCE 484–425), known as the father of history, and the first writer to record a remote viewing in the 46th chapter of his *Histories*, as well as his contemporary the Greek historian Thucydides (460–404 BCE), discuss it at some length. Herodotus speaks of Marea as being a garrison town during the reign of Pharaoh Psamtich I (664–610 BCE), and indicates that it still served a strategic function under the Persians in his time (450 BCE) (Herodotus 1939 II:18,30).

At least two royal claims were settled at Marea. According to Diodorus, Amasis (575–526 BCE) defeated Pharaoh Apries and assumed his mantle at Marea (Diodorus Siculus 1933). Soon thereafter, in 525 BCE, Amasis' grandson, Inaros, proclaimed himself king at Marea, and used the city as his headquarters while he fought the Persians under Cambyses (Herodotus 1939 III:12,15).

But all that is gone, both lake and city: the one now vastly diminished, strongly alkaline, and no deeper than four feet at any point, and the other abandoned and buried.

Archaeology

The previous archaeological work relating to Marea is very limited. It begins with Mahmoud-Bey, known casually as “El Faliki” (“the Engineer”), an astronomer in the Khedival government. Although a very problematic figure, he carried out one of the first systematic archaeological explorations of Alexandria at a time when much that was ancient still remained relatively intact. His mid-19th Century excavation report describes far more than is visible today (Mahmoud-Bey 1872). Half a century later Hannibal Evaristo Breccia, generally considered the first modern archaeologist working in Alexandria, examined the area, and his description compared with Mahmoud-Bey's suggests how much had already disappeared (Breccia 1922).

Peter Fraser, a Fellow of All Souls College, and generally regarded as the best historian of pre-Islamic Alexandria, writing a century after Mahmoud-Bey, touches on Marea, both in his text and in his remarkably bounteous notes. From them it is clear to see that by the time he published his classic *Ptolemaic Alexandria* in 1972 there was little left to see (Fraser 1972).² It is the work of Professor Mahmoud Sadek of the Department of Fine Art at the University of Guelph, however, that most concerns us here, because his research is the reason we undertook the Marea experiment.

From November 16 to mid-December 1976—just three years prior to the work presented in this report—Professor Sadek and his team searched “the shoreline, promontory, and south to the end of the visible remains,” seeking evidence of previously undiscovered buried structures. It was a methodical job, using the latest electronic remote sensing technologies, and search methodologies, including aerial photography, topographical survey, and, most importantly, proton precession magnetometer (Sadek 1978:72). Measurements were recorded on a four-meter grid, 800 meters long by 100 meters wide (Sadek 1978:72). Transverses “were made along lines parallel to the lakeshore from west to east at four-meter intervals, and readings were taken every four meters along the transverses” (Sadek 1978:72). A plan was

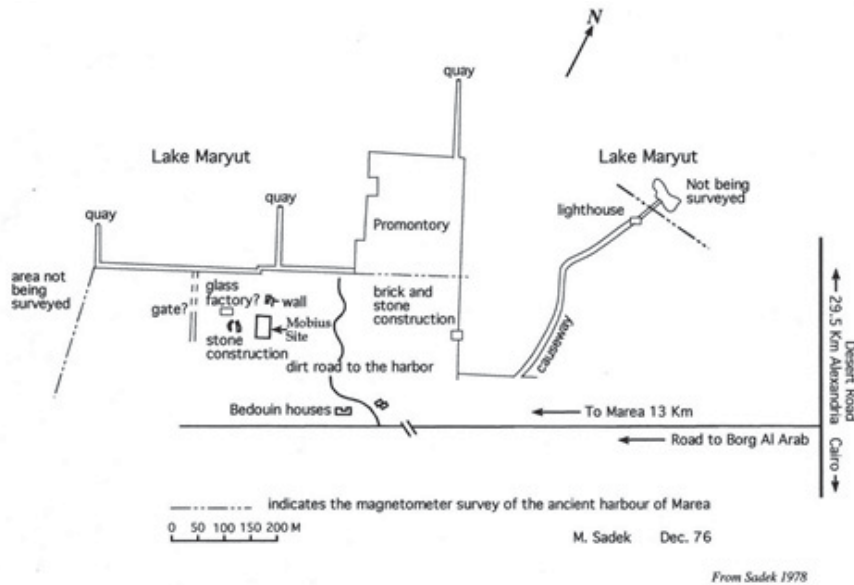


Figure 1. M. Sadek surveyed the area three years before the Mobius work. Using a variety of electronic remote sensing technologies, his team found buried constructions, as can be seen on the map. Note nothing was found at the site selected through remote viewing.

produced that indicated that there should be found at the promontory and near the quays “a high concentration of sub-surface structures, probably indicating that this was the town center and pointing to the probable existence . . . of warehouses and factories” (Sadek 1978:72–73) (Figure 1).

What was most interesting to Sadek was “the grid system pattern of streets running north and south intersected by others running west to east” (Sadek 1978:73). Such a grid design is unusual in Pharaonic times (Sesostris II and Tell El Camarna, as Sadek notes, notwithstanding) and was apparently constant through all subsequent inhabitations. Sadek reports that his magnetometer exploration produced no layout that did not conform to the grid, and “there is a strong likelihood that this latest level of occupation was based upon earlier settlements” (Sadek 1978:73). The report concludes saying, “It is unlikely that anything remains of the structures except foundations” (Sadek 1978:7).

While we were interested in what Sadek found, we were even more interested in what he did not find in his careful search.

Comparison of Electronic Remote Sensing and Remote Viewing Data

One of the research objectives of Mobius' Egyptian fieldwork was to arrange for a comparison between remote viewing and electronic remote sensing. Sadek's 1976 work offered exactly the comparative dataset we needed to effect such a comparison. When we were challenged by skeptics on the University of Alexandria faculty to prove remote viewing actually worked, by locating and describing an unknown archaeological site of their choosing in an archaeological area the University controlled, Marea, we enthusiastically accepted (Schwartz 1983/2001).

Sadek's report provided a completely independent source against which to measure the remote viewing data and thus made Marea an ideal location for our experiment. Here was a site of sufficient importance that a fairly detailed history of its past had been recorded, a history which could be compared with the reconstructive material produced by the remote viewers. Yet one still obscure enough to be *unknown* to all but a few archaeological professionals.

Additionally, because Marea was an intermittently active archaeological area under the supervision of the University of Alexandria, a regularly updated historical and archaeological description of the city was available. It established what was known about Marea, as well as a clear definition of what was *not* known. Thus, we could begin with a defined problem, and be sure that true triple-blind conditions existed—no one knew the correct answers, only excavation could reveal accuracy—upon which all could agree. A clear delineation between the results obtained through utilization of remote viewing input and electronic remote sensing could be established, and it would be relatively easy to isolate verifiable reconstructive data obtained through remote viewing that was previously unknown (Figure 1).

Remote Viewing

Remote viewing is the demonstrated ability of individuals to describe persons, places, or events from which they are shielded by virtue of space, time, and "blindness" protocols. Viewers, in much the same way that an eyewitness would report their impressions, respond to a task that can only be accomplished by opening to nonlocal consciousness. All their senses report; that is, they can answer questions that involve smells, sounds, colors, shapes, textures, even tastes. The exact mechanism of this perceptual skill is not clear, but hundreds of published successful studies have validated the existence of nonlocal consciousness; this is not a phenomenon of chance (Schwartz 2014). The task of the researcher is to structure the interview session in such a way that normal sensory cues are absent, and that

intellectual access is eliminated. Researchers in an applied remote viewing experiment such as this one are blind to the correct information; indeed, by definition, everyone is, it is a triple-blind protocol. That is what makes it desirable for nonlocal consciousness research. It is also exciting, which makes it highly numinous, and that has been shown to increase accuracy.

Although the execution of the protocol may seem unusual, in fact researchers are essentially faced with a novel manifestation of a familiar engineering problem: searching for data buried in noise. In the case of side-scan sonar, the “noise” is particulate matter in the water, schools of fish, and the like; in this instance, normal sensory awareness and prior knowledge constitute the “noise.”

In addition to the research done by the Mobius laboratory, the research most relevant to the work reported on in this paper was that done for the U.S. Army and the CIA by Puthoff and Targ (Puthoff & Targ 1974, 1976) at SRI International.

The use of nonlocal perception in archaeology entered the literature almost a century ago with explorations of Glastonbury Cathedral in England (Schwartz 1978a:1–56, 353–354 bibliography), and continues (albeit infrequently) to surface periodically in research ranging from Poniatoski’s in Poland (Schwartz 1978a:57–107, 354–355 bibliography), Scott-Elliot in England (Schwartz 1978a:108–127, 355–356 bibliography), Pluznikov in the Soviet Union (Schwartz 1978a:127–135, 355–356 bibliography), Weiant’s with the Smithsonian at Tres Zapotes (Schwartz 1978a:222–238, Weiant 1943, 1960), and Reid’s Ontario Iroquois Indian sites (George McMullen [R3] was the viewer) (Schwartz 1978a:211–221, Schwartz no date). All of this exploration, however, was done with very little emphasis on studying the nonlocal nature of the data acquisition with a formal protocol. All this earlier work was dependent on the input of a single remote viewer, and there was no concept-by-concept assessment of accuracy.

In 1976, the author began developing a consensual methodology using multiple respondents independently and individually responding to the same questions—in conditions of intellectual and sensory blindness. Each was asked the location of archaeological sites, the description of surface geography, and the description of subsurface, or underwater, materials, to be found at that site. This team approach was designed to help improve the information-to-noise ratio previously described. The remote viewers functionally are the survey instruments, and using more than one on the same site is the equivalent of having multiple electronic sensors—satellite reconnaissance, and magnetometer survey, as examples—describe an area and then collectively define what is there.

The first use of this consensual methodology was in 1977, in a deep

ocean experiment series utilizing the research submersible Taurus I. Part of the research involved the location and description of a previously unknown wreck on the sea floor off the coast of Catalina Island in California (Schwartz 1977, 1978b). The program was conducted by Mobius in conjunction with The Institute for Marine and Coastal Studies of the University of Southern California. Known as Project Deep Quest, this field project demonstrated that remote viewers could describe in detail, from distances of up to 4,800 kilometers, a previously unknown wreck at 92+ meters of depth (Schwartz 1978c). They were also successfully able to describe specifics as to what would be found, an accurate description of the site (including drawings), the cause of the ship's sinking, and the approximate period when the disaster occurred. All points were corroborated by fieldwork, literature review, and expert analysis (Schwartz 1978c).

Personnel

To carry out this research program, six teams were assembled, each having responsibility for one aspect of the research. The specialty teams were:

The Historical/Archaeological Team: Fawzi Fakharani, archaeologist, Department of Classical Civilizations, Faculty of Arts, the University of Alexandria; and Mieczyslaw Rodziewicz, archaeologist, Director, The University of Warsaw Archaeological Mission in Alexandria.

The Remote Viewing Research Team: Stephan A. Schwartz, parapsychologist, Mobius; Beverly Humphrey, parapsychologist, SRI; and Kathi Peoples, Mobius staff support.

The Remote Viewers: The two remote viewers taken to Egypt were George McMullen (R3), and Hella Hammid (R5). Although Hammid, like McMullen, had participated in the Deep Quest experiments, only McMullen had any real experience with archaeology, having worked for some years with Professor J. Norman Emerson, chairman of the University of Toronto's Department of Anthropology, as well as with his student Reid (Emerson 1975).³ Neither viewer had ever been to Marea, and they reported they did not even know of its existence. Until asked about Marea, they had no indication that they ever would. Both viewers were "blind" to the questions before these were presented to them, indeed did not even know the project was to take place in Egypt. But even if they had been working for months with archaeologists and specialists in Alexandria, it would not have mattered since the questions were, by protocol, outside the corpus of knowledge.

The Archives and Records Team: Catherine Dees, historian; Kay Croissant, historian; Karen Winters, field log; David Keith, illustrator; and Jacqueline Kendall, staff support.

The Photography Team: Glenn Winters, film camera one; Bradley Boatman, film camera two; Karen Winters, still photography; and Kathi Peoples, camera assistant.

The Audio Team: Sunny Meyers, audio-film; Osama Salama, audio-film; Stephan Schwartz, interviews.

The Photography and Audio Teams were established so that an unimpeachable real-time, audio-visual record of every aspect of the experiment would exist.

Protocol

Our initial plan had been to follow our standard protocol as previously reported (Schwartz 1980, Puthoff & Targ 1974).⁴ However, it proved impossible to find maps of sufficient detail in the United States to carry out the normal pre-expedition map probe. Once in Egypt a search for maps of Marea, at the government map office, finally turned up a single map in Arabic: *Kreir* (Sheet 92/480 *Egypt—Western Desert Province Markay Maryut*, scale 1:25,000 km), which at least located Marea, although the site at this scale covered less than one centimeter, and was useless for location work (Figure 2). On April 10, 1979, McMullen (R3) and Hammid (R5) were each given a photocopy of the map (created to remove colors that might inadvertently cue) and asked to record whatever impressions they could from it. Later in the day each was independently interviewed, but nothing that could be verified was developed at this point. Each simply reported a general sense of constructions relating to several different cultures and historical periods.

I remind the reader that everything described herein, indeed the entire Alexandria Project, was filmed and is publicly available at <https://stephanaschwartz.com/>.

Early on the morning of April 11, 1979, the two remote viewers were placed in separate cars, accompanied by a member of the Remote Viewing Research Team. Fakharani, accompanied by a graduate student assistant, as previously arranged, was isolated from the remote viewers at this stage and traveled in his own car. The cars, led by Fakharani, moved in caravan until 08:30, when all stopped at a site of Fakharani's choosing. Although the exact location was unknown to us, it was pre-agreed the stop would be at least 10 km away, and out of visual range, of the overall Marea site.

On the way out in the car, Hammid (R5), unsolicited, reported impressions relating to a tomb and a mosaic.⁵ McMullen, riding in his car, said nothing about the experiment. Upon arriving at the rendezvous point, the author decided that Hammid should wait with Humphrey, while he and McMullen made the first location attempt. After they had left, McMullen

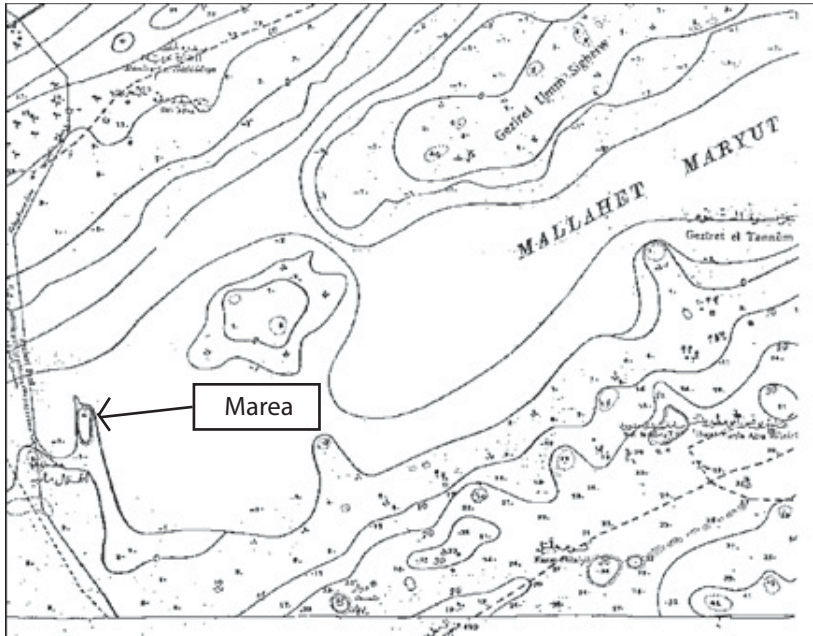


Figure 2. The only map showing the location of Marea was on a scale useless for the usual map location phase of a Mobius archaeological project, as Marea was less than one centimeter in size. Note the small black dot. All colors were removed from the map to prevent inadvertent cueing. Place names were not an issue since most were in Arabic, which neither viewer read or spoke.

was given the following charge by Fakharani:

- A. Locate the ancient city of Marea: It is somewhere within an area roughly 24 km on a side, about 576 square km in size (an area roughly equal to one-half of the city of Los Angeles). After locating the city, locate a building that has either tile, fresco, or mosaic decoration in it.
- B. Within the chosen building, locate the walls, the windows, the doors, and the depth at which the floor would be found.
- C. Describe any artifacts or conditions that would be found within the building site.

With this charge, McMullen, accompanied by only the author carrying a taperecorder and followed by one of the two film crews, headed off across the desert (Figure 3).



Figure 3. Placed kilometers from the buried city of Marea, which no one from Mobius had ever seen, McMullen still seemed to be able to orient himself.

Fakharani and his graduate assistant waited at a distance by themselves. For the next several hours, McMullen proceeded at a fast pace to walk the spine of a ridge, occasionally moving down its flanks. As was typical of McMullen when in the altered state in which he produced remote viewing observations, he was not deterred by temperature in excess of 38 °C (100 °F), nor the strong wind laced with stinging sand and biting flies. Indeed, as he walked, his normally pronounced limp disappeared and he became more animated, carrying on a continual monologue about what he described as a “Bexonine [sic] [Byzantine] . . . culture of grave robbers . . . people who lived off earlier people’s achievements.”⁶ (McMullen throughout the remote viewing sessions mispronounced the word Byzantine, just as he said “Potomie” for Ptolemy and “mosiak” for mosaic. Similar mispronunciations were reported by Emerson. He frequently pronounced them correctly in normal conversation.⁵)

Most of the disclosures were either untestable, or indefensible against criticism from some ordinary source. Finally, McMullen stopped and said, with considerable emphasis, “Okay, I know where I want to go.”⁶

McMullen and the author then walked back to where Fakharani and his assistant were waiting, whereupon McMullen knelt in the sand, sketched



Figure 4. Without ever having seen it, McMullen (left) accurately sketched out for Fakharani (middle) and the author (right) a map of where Sadek had done his survey and Fakharani had done some preliminary excavations.

an outline of Marea as it appears today, and described for Fakharani where the University of Alexandria’s dig was located, and what the area looked like (Figure 4).⁷ Fakharani acknowledged on camera the accuracy of the description.⁷

The cars were brought up, and the group then drove approximately 8 km to the Marea site. Upon arrival, and continuing over the next hour, as he walked around trailed by camera and sound crews, McMullen provided Fakharani and the author with a reconstruction of the city.

Much of the material pertained to specific scenes and individuals of ancient Marea and was inherently untestable. But much was also very specific and testable. McMullen, for instance, located several new sites. Since these were near existing excavation work areas, they were simply noted by Fakharani for subsequent investigation.⁸ By prior agreement, only a totally unknown site was to be evaluated in the context of this particular experiment.

McMullen was charged again by Fakharani to “locate an important building—one with tile, fresco, or mosaic—something representative. It is for you to tell me where to dig.”⁸

Without hesitation, McMullen proceeded to walk up a hill on the south side of the ancient road. Once there he:

- A. Quickly sketched in the outline of a building with several rooms and stated that the area described was only a part of a larger complex.⁸
- B. Located walls, one doorway, and the corners of the structure.⁸
- C. Indicated that the culture that had built this building was Byzantine.⁸
- D. Described the probable depth to the tops of the walls as being approximately “three feet” (.91 m).
- E. Indicated that there would be debris (dropped there after being taken from a different structure).⁸
- F. Said that one wall, the west one, would have tiles on it.⁸
- G. Explained the culture or cultures that had built or modified the building and its later use for storage.⁸
- H. Felt we should come across “a floor” of the structure at approximately “six to ten” feet (1.8–3 m), although he confessed—somewhat distressed—“I can’t see the floor.”⁸
- I. Said several colors would be associated with the site, but felt green was the one that would stand out, since he perceived it most strongly.⁸

With this remote viewing data filmed and recorded, the first Marea remote viewing session ended, and McMullen was taken by car some distance from the site. That ended the first phase of fieldwork.

The next fieldwork phase called for a repetition of the entire location evolution only with a different viewer who had witnessed nothing of what had occurred in the first phase. Hammid was taken from where she had been waiting to the starting location where McMullen had begun, to establish the same site locale. She had been waiting for more than four hours in over 100 °F heat and strong desert sunlight and was feeling sick. Although still willing to attempt the walk, it was decided to bring her directly to Marea. Once there, she was taken to the general area of the site and told simply to look down into the ground and describe what she saw. Here again, the emphasis from Fakharani was on locating a building with decorations within it. There was nothing in evidence at the site to cue her to McMullen’s observations, and she knew nothing about them. After walking about for a moment, Hammid walked over to the same area previously selected by



Figure 5. Hammid, in hat, described a strange column or pillar buried just beneath her. Note two motion picture cameras recording the scene.

McMullen and sat down, accompanied by the author (Figure 5). Almost immediately Hammid began to describe:

- A. Walls.⁹
- B. A sense of several colors but especially “green.”⁹
- C. “Tiles”—possibly “green . . . on the walls.”⁹
- D. “A northwest corner” which she outlined.⁹
- E. A sense of “a bathroom, something to do with baths and washing.”⁹
- F. A floor of mosaic tiles, which were of “a smooth polished stone, possibly marble, with color” that she saw as “being laid in a design.”⁹
- G. A sense that this was an important building.⁹
- H. A sense that the building beneath her had “more than one room.”⁹
- I. Her strongest perception was that of a small “alcove sort of room containing what looked like a “broken column or statue . . . something round . . . and free-standing . . . but not complete.”⁹



Figure 6. So that there would be no unclarity about exactly where the remote viewers meant the dig to be located, as well as to fix the location of corners and a door, McMullen (left) directed the placement of wooden stakes.

Ending her remote viewing session, Hammid said she felt seriously queasy from the heat and sun and asked to be taken back to the hotel in Alexandria. That ended the second phase.

After Hammid had departed, McMullen, who knew nothing of her session, and who had been waiting some distance away while Hammid worked, was driven back to the general area of the site Hammid and he both had selected, although he did not know that. This time, he was asked to relocate and outline the limits of the building he had previously described. So that there would be no question of his exact location, he was given three-foot-long wooden stakes, which he used to put a stake at each corner of the still-buried building, and a fifth stake to mark what he said there was a doorway (Figure 6).¹⁰ After this second remote viewing session was completed, McMullen was driven back to Alexandria. It had been previously agreed that the remote viewers would not discuss their individual sessions throughout the course of the experiment.

Independent Archaeologist Pre-Excavation Evaluations

After the departure of the remote viewers, Fakharani and the author went over the data they had provided in detail. Although he had witnessed everything, he indicated that he had not always been able to clearly hear every word. So the audiotapes were played back for him, and he was asked to evaluate what he heard. Fakharani appeared to be amused. He stressed that the electronic survey had been unproductive in this area, and he found the idea that remote viewing would succeed, where sophisticated electronic remote sensing had failed, preposterous. If there *were* anything at the location the remote viewers had selected and marked out, he said, he believed it would be the Roman acropolis.¹¹

Asked to comment on the outline of the walls, he responded that walls could no doubt be found all over Marea. When asked to reconcile this observation with the fact that the electronic survey had not turned up walls at this site, he did not respond.¹¹ He reiterated his disbelief that anyone could locate and outline buried walls using remote viewing.¹¹ He said that although the digging might conceivably uncover walls, if it did they almost certainly would *not* be aligned with the stakes laid out by McMullen. Any structure found on the hill, he said, would specifically be oriented differently.¹¹

Additional Remote Viewing Prior to Excavation

On the morning scheduled for beginning the actual excavation, April 17, 1979, prior to leaving the hotel McMullen volunteered two sketches of Marea as he perceived it during the Byzantine period.¹² He further volunteered information elaborating his answer to the question concerning the floor he had been asked about on April 11. He said it had continued to bother him because while he could not clearly remote view any floor at the site he felt:

- 1.) Small tiles would be found at the level of the floor.¹³
 - A. These tiles would be marble, smooth on one side and rough on the other¹³ (Figure 7).
 - B. As part of a floor, the tiles had been laid in a chalky sub-flooring.
 - C. The tiles were square.¹³
 - D. The tiles were 5/8 of an inch (1.59 cm) across.¹³
 - E. The tiles were one color each.¹³
 - F. The tiles at one time had been laid in a colored pattern.¹³

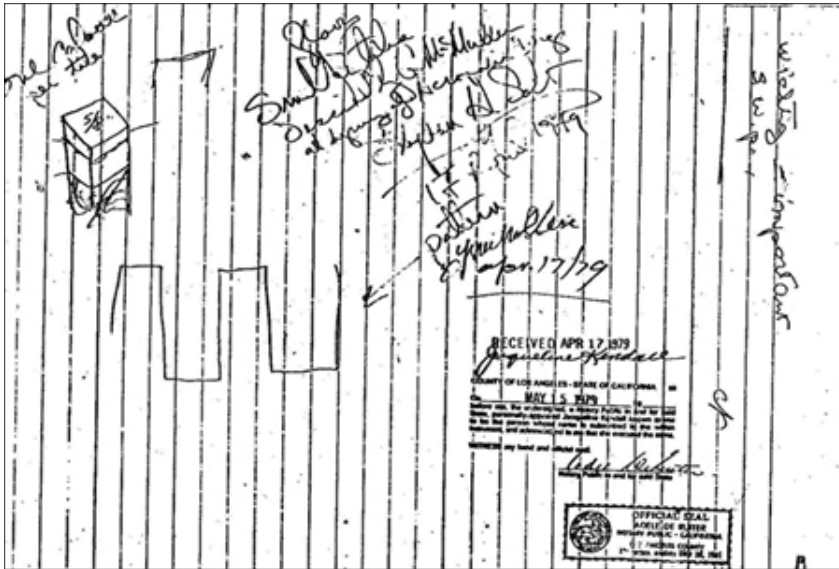


Figure 7. On a pad of lined yellow foolscap, McMullen drew the small tiles he saw, their chalky sub-floor, single color, and pattern. Also note notary's seal to establish an unimpeachable chronology.

Excavation Methodology

In accordance with the pre-agreed protocol, all digging was to be directed by Fakharani; he had the imprimatur of the University of Alexandria as their expert on Marea and was a trained archaeologist. By assigning responsibility for the excavation to an outside observer, we sought to avoid any vulnerability to charges that by controlling the excavation we, in some way, might manipulate the outcome.

Upon arriving at the site at 08:00 on April 17 for the first day of digging, as we drove up we saw, and filmed, Fakharani moving the stakes. He had radically skewed them from their original orientation, and extended the dimensions on the western and eastern sides. He had also directed the workers to begin. This would have gone unnoticed if we had arrived minutes later.

In defense of these actions, Fakharani claimed that the move was necessary to assure that both sides of the walls—if walls there were—would be excavated. He reiterated again that the site had been surveyed electronically and nothing had been found, and asserted again that if there were anything, which he did not think would be the case, it would be Roman, and could not possibly be Byzantine.

Figure 8. Within inches of the depth predicted, walls appeared. It was also possible to see there were multiple rooms, and that Hammid's strange column was in fact exactly where she had described it.



Only about two inches of desert soil had been removed and nothing had been revealed. Most importantly, the holes where McMullen had placed the stakes were still obvious. Against Fakharani's strong objections, the stakes were moved back into their original holes, thus assuring the dig would take place exactly as defined by the viewers, while the triple-blind conditions still prevailed.

The Bedouin laborers Fakharani had chosen could only work in the early mornings before it became too hot for physical labor (the temperature rose as high as 114 °F / 45 °C or even higher by afternoon). As a result, excavation would take almost six weeks.

Additional Remote Viewing After Excavation Had Begun

On April 18, approximately 10 cm of Hammid's column suddenly appeared, and work was stopped. McMullen was asked to remote view it. He said immediately that it was connected with "heat . . . and fire."¹⁴

On April 25 at about 08:30, walls began to appear. The depth was within inches of that predicted by the viewers, and exactly as oriented and staked out by McMullen (Figure 8).

I stopped the work, divided the site into nine quadrants, and asked Fakharani to excavate one quadrant at a time so we could accurately assess each concept advanced by the viewers as we went along. He objected and found this very tedious, but he finally agreed, and that is how work proceeded. On April 26, when excavations were at about .46 m (1.5 feet), two other remote viewing points were volunteered by McMullen:

1. At about eight to ten feet (2.44–3.05m), a ledge running around the walls would be found.
2. Something we would find was associated with baths or bathing. He stressed that he was unclear what this meant, but he emphasized the strength of this impression.

On April 27, McMullen, while standing in the partially excavated site, was asked to remote view it again concentrating this time on decorations. He volunteered, “This is generally Roman, and I would say this is a steam bath . . . ”¹⁵ He could not reconcile the apparent contradiction between his earlier statements describing the site as Byzantine, nor explain why he suddenly was perceiving imagery about baths just as Hammid had. McMullen, who knew nothing of Hammid’s comments, said he simply felt that both observations were accurate.

I believe that in both instances these observations occurred because I, the interviewer, who had given the viewers their task instructions, had failed to formulate the task correctly. In this instance, for example, McMullen was asked to focus his intentioned awareness on decorations, but was not told they should be decorations specific to the construction of this particular ruin. Throughout the Alexandria Project we saw and confirmed several other examples of this kind of displacement, all emphasizing the importance of the framing of the task instruction.

Excavation Results and Evaluations

Since there was no productive location data at all from any aspect of the electronic remote sensing, the historical review, or the topographic survey specific to this site, there is nothing to evaluate beyond the failure of these approaches to locate what was actually found. Only the remote viewing information provided positive testable data. It is also worth noting that no electronic remote sensing technology could have produced comparable data to the remote viewing material pertaining to colors, culture, and artifacts.

Walls: Both remote viewers had predicted walls would be encountered at a depth of “between three and four feet (0.91–1.22 m).” This statement

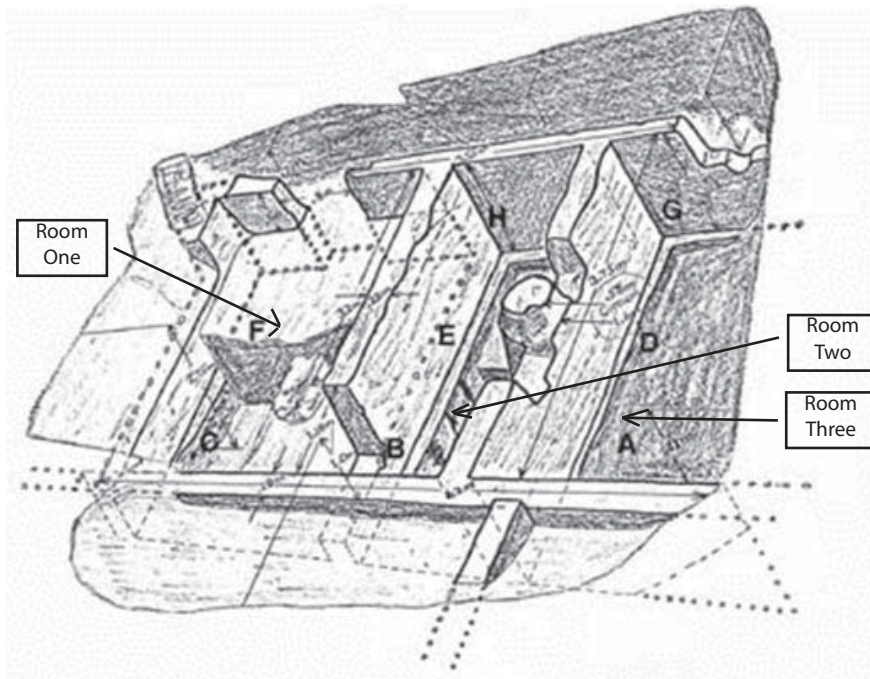


Figure 9. Field sketch of the site completed. Bold printed letters denote quadrants.

was made by McMullen in reference to the western wall he had staked out through Quadrants A, B, and C. Hammid did not specify a particular wall, only the depth at which walls would be found. Excavation revealed wall tops at 3 feet four inches (1.01 m), exactly within the depth predicted (Figure 8).

Orientation: The first part of what later proved to be a wall was found in Quadrant G (Figure 9) and closely approximated and was parallel to the stake orientation laid out by McMullen.

Multiple rooms/part of a larger structure: Over the next week, additional walls were uncovered, clearly defining the three distinct rooms predicted which were obviously part of a larger structure as predicted (Figure 10).

Byzantine vs. Roman: Although the wall stone was well-dressed, from the beginning it was obvious this site was not the remains of a Roman acropolis as Fakharani had insisted. He continued, however, to maintain for some days longer his belief that the site was transitional, that is late Roman or very early Byzantine. This position also became untenable as pottery

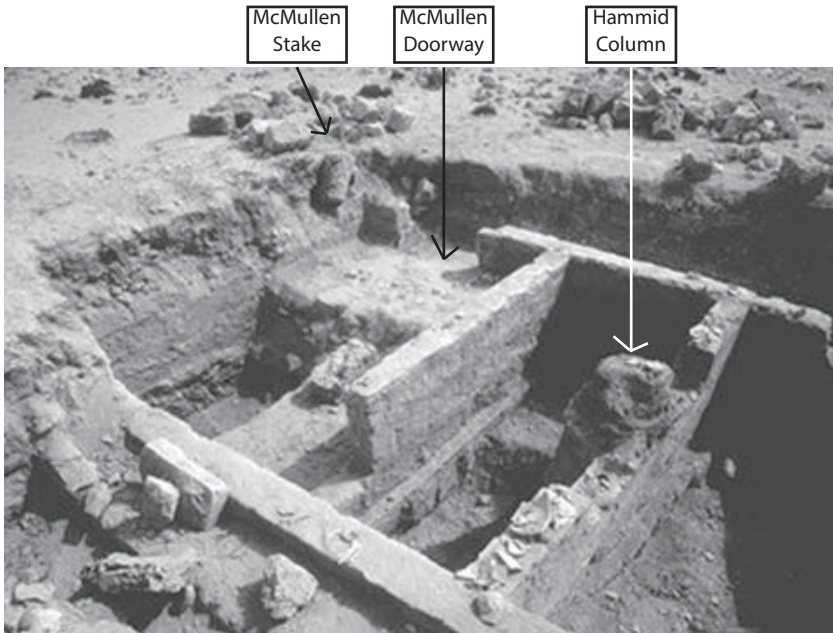


Figure 10. Dig in its final phase, showing all three rooms, doorways, ledges, and Hammid's "column."

sherds emerged. This material was judged to be fill, but almost all of it was of Byzantine origin, and late Byzantine at that (Figure 11).¹⁶

The issue was further defined when a red and white cross in a circle was found painted on the foundations of Room 2 (Figure 12). It was not clear immediately whether this was a consecration mark, or a quarry mark, although most of the archaeologists who were shown the data felt it was the mark of a Christian building. Months later, in November 1979, the issue was settled when Rodziewicz, who had by then evaluated the site in detail, reported his conclusions: It was a "6th Century Byzantine" structure, and this confirmed the remote viewing data.¹⁷

Corners: The locations outlined through remote viewing by McMullen and Hammid proved to be highly accurate, and are particularly impressive from a remote viewing point of view (Figure 10).

Green: At a depth of 1.5 m (4.92 feet), primarily in Room 2 in Quadrant H, substantial quantity (about two-thirds of a rubber worker's basket) of a green clay-like substance was discovered. This material, found in roughly rectangular chunks, crumbled easily between the fingers. Fakharani felt it was a pottery or tile glaze.¹⁸ Whether this meant the room had been used for making pottery or tile, or the material was fill originating at another site, he was not prepared to say.¹⁸ A single piece of dark greenish tile was

Figure 11. One of the several hundred Byzantine sherds excavated from the dig site.



found in Quadrant A on April 25. Whether it was in situ or not has yet to be determined. What is unequivocal is that at a site notable mostly for its tans and sand desert colors, the color that stood out the most was green, just as predicted.

Wall Tiles: Other than the one green tile, no wall tiles were recovered at this site, and this could be considered the greatest “miss” in the remote viewing data. However, in light of the outcome of the floor data, it is also possible that wall tiles had been stripped from the building before it was abandoned.

Doorway: A doorway was found leading into Room 3, exactly where it had been staked out by McMullen (Figure 10). This was apparently a later and cruder exterior entrance than the building’s original entrance, which was not in the section excavated.

Freestanding cylindrical object / column: On April 26, workers taking down Quadrant E uncovered a domed round shape, which proved to be the top of a column. Not a structural column, but one formed of a brown unglazed low-grade pottery, sufficiently sturdy that it maintained its



Figure 12. The red cross found on the “ledge” at the bottom of the foundation.

structural integrity throughout the excavation. It was clearly not original to this site, and was free-standing in a crude breach notched between the wall separating Rooms 2 and 3. It tapered from bottom to top, measuring approximately 1.6 m in circumference near the base, and 0.58 m at the top (Figure 10).

Excavations over the next week revealed that the “column” was unquestionably a later addition, added long after the building had been abandoned by its Byzantine builders, since the wall breach only went down about 0.45 m, and the column measured about 0.4 m in height. It would have toppled over unless one assumed that at the time the breach was made (with no attempt to redress the stone) the bottom of the gap was essentially the building’s floor—the lower portion of the structure having by then been filled with material from other sites at Marea, as well as accumulated sand and dirt blown in by the frequent winds. None of the members of the Historical/Archaeological Team could advance a definitive explanation of its use and identity.

A plausible explanation, however, came from a worker of Libyan heritage. He said it resembled a type of oven he had seen his grandmother use, both for heating and for baking bread. Essentially a primitive kind of thermal bank it allowed a village woman to eke out every joule of thermal

Figure 13. The “ledges” described by the remote viewers were located as predicted. This was the level of the original floor.



energy in a land where wood was scarce and even animal droppings were hard to come by. He said it was his grandmother’s practice to build a solid pillar of “poor pottery” around which coals were heaped to heat it. When the coals were scraped away, he said, flat loaves of bread were laid directly on the pottery form to bake.¹⁹

Of all the finds made in the course of this excavation, the discovery of this column caused the most excitement among the Remote Viewing Research Team. Its very anomalousness—though confusing to the archaeologists—made it attractive to the Remote Viewing Research Team. Such an object could not have been anticipated, and accurate data concerning its presence was, for that reason, all the more impressive. Hammid’s final grace note, that the column was “broken,” was accurate.²⁰

Alcove column room: Hammid said the column would be in a kind of “alcove room,” and Room 2 appeared in just this configuration in relation to the size of the other two rooms. This was, as she predicted, also where the column would be found (Figure 10).

Ledges: On April 27, McMullen’s statements about the ledges was proven out. A ledge was discovered in Room 3 at 1.1 m below the top of the wall. The next day, ledges were found in Room 2, at approximately the same distance below the top of its walls (Figure 13). A ledge was subsequently

found in Room 1. McMullen had seen these ledges as in some way related to seating. This was incorrect and possibly a example of one of the major sources of “noise” affecting remote view perceptions—the tendency of remote viewers to interpret what they perceive, rather than just reporting the image.

Floor . . . no floor: McMullen’s concern about the floor question was resolved at the same time the ledges were discovered. In Rooms 1, 2, and 3, a white gypsum-like hard chalk surface was uncovered. Fakharani felt that this was a sub-floor and that the floor cover that had rested on it had later been stripped away.²¹ This layer was broken through and from that point to below the foundation no other sign of a floor was discovered. In light of this sub-floor discovery, in the absence of the main flooring McMullen’s “floor but no floor” comments suddenly made sense.

Small tiles: At McMullen’s first remote viewing session, on April 11, he had given a brief description of small floor tiles. He augmented this during the session of April 17 by his drawing and his comments about the tiles (Figure 7).²² On April 29, while workers were taking down Quadrant C, in Room 1, they hit the gypsum sub-floor. In the northwest corner of Quadrant C, intermixed with the gypsum and just below it, they found three circular marble objects, rather like thick quarters. Over the next two days a total of eight more of the objects was located (Figure 14). Each one was either red, black, or white.

Each of these disks was 3 cm (1 3/16 inches) by 1 cm (25/64 inch)—McMullen had estimated prior to excavation that they would be 5/8 inch across. He was incorrect in his perception that the tiles were square, but correct that they were smooth on one side, rough on the other, and used on top of a chalky sub-floor. Both he and Hammid were consensually accurate in their description of the tiles as being of one color each, and marble.

Fakharani, who had been notably skeptical of the idea that someone could describe a tile buried several meters into the earth, at first maintained that the objects were weights. Closer examination by Daoud and Rodziewicz, and subsequent conversations with Fakharani produced the consensus that they were, as McMullen and Hammid had predicted, mosaic tiles.²³

Debris: In almost every remote viewing session, there was a clear sense on the part of the remote viewers that “a lot of debris” would be found in the site, and a great deal was, in fact, located— particularly masses of pot sherds. There were also pieces of marble uncovered, which were evaluated by Fakharani as being Roman debris. The author, at the time of the interviews, neglected to properly follow up on these observations in order to elicit further imagery. The remote viewers, however, on one topic volunteered data, as is seen in the next item.



Figure 14. Small tiles were found as predicted by McMullen, who was correct in describing them, except that he saw them as square (they were round), and he was 5/8 inch off in their size.

Bath . . . bathing . . . steam bath: From the very first session at Marea, the remote viewers had provided data related to “baths and bathing.” Hammid had felt it so strongly that she thought it must a kind of analytical overlay suggested by the guidance query about tiles.²⁴

As with the debris question, the issue of baths benefited from subsequent study by members of the Historical/Archaeological Team. In November 1979, Rodziewicz said that he had examined both the site and the artifacts (Figure 15). He had not been told about the observations of the remote viewers concerning baths, bathing, and steam baths.

He reported that he felt that a great deal of the material, particularly the marble fragments, was debris that had originally come from the Roman “baths down the hill. . . .”²⁵

Prior to our work at Marea, Fakharani had just begun excavating a structure at the foot of the hill, across the road from our site. When we were there he said it was a church, an observation with which McMullen disagreed. Work on Fakharani’s site was suspended while we were there, but after we had left Marea it resumed. When we returned to Egypt eight months later, Fakharani, after consultation with Rodziewicz, had changed his mind, and now believed the site he was working was a public baths probably dating to the Roman period. Rodziewicz particularly pointed out



Figure 15. Marble fill found at the site, but later determined to have come from the baths, when they were subsequently identified at the foot of the hill.

evidence he had found of hydraulic mortar, and suggested that the pieces of marble found at our site were probably debris that had originally come from these baths.²⁵

Discussion

Comparison of search technologies: Which approaches, electronic remote sensing or remote viewing, were most accurate and productive? The question is easy to answer in this experiment, because all the electronic and geographical surveys—satellite imagery, magnetometer, and topography—were completely unproductive at this site.

Location accuracy: Remote viewing was highly accurate in providing information for location. The building was outlined within inches, indeed any variance is due more to Fakharani's abortive attempt to move the stakes placed by McMullen than inaccuracy on the part of the remote viewer. The location of the door was an extra, and particularly impressive, addition. But, perhaps most elegant of all was the correct location of the corners. Corners are especially difficult, because they represent the intersection of two planes, and must be precisely located.

Descriptive accuracy: In contradistinction to laboratory experiments which can measure the variance from a chance outcome, because they work with known target sets, in an applied remote viewing experiment no such baseline exists. Even more importantly, in a laboratory experiment the statistical outcome is the end step; while in an applied remote viewing experiment the viewing is the beginning of the project—the source of location and descriptive predictive data. In this setting the evaluation outcome is the expert analysis by knowledgeable specialists.

Researcher error: A close examination of the data reveals the subtlety of the researcher/remote viewer transaction. The viewers were asked to concentrate on both a building and decorations, and they did so. But many of those decorations came from another site; not terribly important in this experiment, but potentially very important in others. The failure can be traced to the premise question. *Properly, the question should have been to describe "decorations original to this building."* The remote viewers fulfilled their tasks—the excavation demonstrates that—but clearer questions would have elicited clearer answers. The entire issue of how questions are framed so as not to cue or suggest a particular answer, but to precisely elicit the information sought, needs much more thought. This experience also suggests that results which superficially appear to be a failure in remote viewing may, in fact, be the failure of the researchers. Finally, it seems to indicate that the apposite way to see this methodology is, again, in engineering terms, in which the researcher/remote viewer relationship is, at core, the creation of a bio-circuit. The Marea experiment along with the Eastern Harbor research are representative of the current state of understanding in applied remote viewing. Both experiments display with clarity the strengths and current limitations of this search technology, and its potential in archaeology.

Ultimately the contribution remote viewing makes will depend on how honestly archaeologists examine the fruits that it offers, unencumbered by preconception and false perspectives. Archaeology must move beyond serendipity in the finding of sites. Remote viewing is not a total answer to its location problems, but it is, surely, one piece of the puzzle.

Acknowledgments

The author wishes to thank the University of Alexandria for granting permission for the fieldwork so essential to this research, and for the cooperation of its faculty. Thanks are also due to The University of Warsaw and the Archaeological Society of Alexandria for graciously making available several consultants, without whose expertise this project could never have been attempted nor once attempted, analyzed and properly evaluated. The author also wishes to thank John and Pamela Leuthold, Maragert Pereira, Trammell Crow, and Gordon McLendon for the financial support that made this research possible.

Notes

- ¹ Marea can be found by driving southwest from Alexandria 29 km on the desert highway that runs between Cairo and Alexandria (turn at the security guard post where there is a sign with an arrow reading “Archaeological Excavation/The University of Alexandria,” and then go 13 km farther on, to a cluster of yellow Bedouin houses known as the village of El Hauwariya. At a blue and white house in the foreground, turn right and pick up the track leading toward the lake. A small, fenced animal enclosure with trees should appear on the left if you are proceeding correctly. At the end of this track the excavations should come into sight.
- ² Fraser’s (1972) three-volume work is unquestionably the finest overall modern source. References are included in the copious notes, often in abbreviations that may be cryptic to those not thoroughly familiar with the source literature on Alexandria and its environs, including Marea.
- ³ Interview with Norman Emerson, Department Anthropology, University of Toronto, on November 14, 1974; Emerson began reporting on his work using remote viewing in 1974 and continued until his death (Emerson 1975, Schwartz 1978a:356–357 complete bibliography of Emerson’s reported work).
- ⁴ Side scan sonar survey by Harold E. Edgerton (Schwartz 1980, Puthoff & Targ 1974).
- ⁵ Memorandum for the Record by Beverly Humphrey, April 11, 1979.
- ⁶ Remote viewing session in the vicinity of Marea with George McMullen, April 11, 1979.
- ⁷ On-camera exchange with Fawzi Fakharani, George McMullen, and Stephan Schwartz, April 11, 1979.
- ⁸ Remote viewing session #1 with George McMullen, April 11, 1979.
- ⁹ Remote viewing session #1 at Marea with Hella Hammid, April 11, 1979.
- ¹⁰ Remote viewing session #2 at Marea, George McMullen, April 11, 1979.

- ¹¹ Interview at Marea with Fawzi Fakharani, April 11, 1979.
- ¹² Volunteered unsolicited remote viewing drawings by George McMullen, April 17, 1979.
- ¹³ Remote viewing session #3 at Marea with George McMullen, April 17, 1979.
- ¹⁴ Remote viewing session #4 at Marea with George McMullen, April 17, 1979.
- ¹⁵ Remote viewing session #5 at Marea with McMullen, April 27, 1979.
- ¹⁶ All pottery from the excavation was turned over to Fakharani, who was to produce a pottery analysis to be included as part of this report. So far none has been received.
- ¹⁷ Interview in Alexandria with Rodziewicz, November 17, 1979.
- ¹⁸ Interview at Marea with Fakharani, April 25, 1979.
- ¹⁹ Interview with Mohamed Abraham, a Libyan construction worker, April 26, 1979.
- ²⁰ Remote viewing session #1 at Marea with Hella Hammid, April 11, 1979.
- ²¹ Interview at Marea with Fakharani, April 29, 1979.
- ²² Remote viewing sessions #2 and #3 at Marea with George McMullen, April 11 and 17, 1979.
- ²³ Interview with Rodziewicz and Daoud, November 7, 1979.
- ²⁴ Remote viewing session at Marea #1 with Hammid, April 11, 1979.
- ²⁵ Interview with Rodziewicz, November 15, 1979.

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ESSAY REVIEW

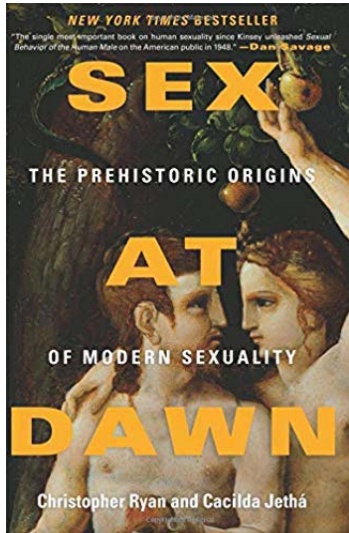
A Treat for Contrarian Thinkers

Sex at Dawn: The Prehistoric Origins of Modern Sexuality by Christopher Ryan and Cacilda Jethá. HarperCollins, 2010. 400 pp. \$26.99 (hardcover). ISBN 978-0-06-170780-3. (Paperback, audiobook, and Kindle versions carry a different subtitle: **Sex at Dawn: How We Mate, Why We Stray, and What it Means for Modern Relationships.**)

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The conventional wisdom across the civilized world has it that among mammals, the human species is characterized almost uniquely by the pairing of individual males and females into monogamy, a lifelong coupling that is best suited to the nurturing of the next generation. *Sex at Dawn* demolishes those beliefs with a host of evidence and logical argument. Instead, it argues, it was only after the nomadic hunter-gatherer lifestyle had given way to agriculture, settlement, and urbanization that societies came to regard pair-bond monogamy as socially desirable—and therefore as natural: “science all too often grovels at the feet of the dominant cultural paradigm” (p. 118).

In the tree of life, we humans are one of the great apes. Our lineage branched from the great-ape stem long after the gibbon (~22 million year ago) and orangutan (~17 million years). Gorillas branched off about 3 million years before humans, and we are separated by about 2 million years from our closest relatives, the chimpanzees and the bonobos. Those closest relatives live in bands or clans of perhaps 100 to 150 or so individuals (give or take on the order of dozens). Their lifestyle is gathering and hunting and moving around, and within the clan everything is shared: obtaining and consuming food and nurturing the next generation. This thorough-going sharing of activities and responsibilities is obviously beneficial to the next generation since it provides insurance against accidents that may strike any individual parent. An important part of this mutuality is that sexual intercourse is freely practiced, any given male having frequent intercourse with many different females and any given female having sex with many different males, conducive to “a more intensely cooperative social group” (p. 64). In sexual behavior, bonobos and humans resemble one another



more than either mimics chimp behavior (pp. 77–78), possibly because a genetic mutation that predisposes to cooperative sociality may be shared by bonobos and humans but not by chimps (p. 72).

One indication that this communally sharing lifestyle is evolutionarily natural to humans is that something like it persists among the surviving examples of hunter–gathering groups. Only when humans settled in fixed locations, made possible by agriculture and domestication of animals, could the very concept have arisen of *ownership* of tangible properties, including *individual* ownership. Thus only in settled, “civilized” societies could the practice of slavery come into being, as well as a patriarchal attitude that wives could be owned chattels of their husbands. Applying economic theory to human behavior as found in modern societies has brought fallacious notions (p. 167) about how or why males and females choose one another (p. 49) for individually mutual exploitation (pp. 57, 270).

Across modern societies there are many taboos and laws regulating sexual intercourse: commonly prohibitions against adultery and against intercourse before marriage, divorce as forbidden or at least stigmatized, homosexuality as unnatural and punishable. Indeed in some groups there is even an underlying attitude that there is something unwholesome about sexual intercourse itself—for example, in the Roman Catholic Church that the Servants of God preserve their virtue and purity by remaining celibate. The modern conventional wisdom, apparently supported by studies in “evolutionary psychology” (p. 52), also discounts female libido, with nymphomania classed as pathological; why then so many practices to curb it?—chastity belts, genital mutilation, isolation in harems (p. 39).

Those present-day practices are obviously incompatible with the hypothesis that lifelong monogamy is natural to the human species, that we have evolved in that manner. If monogamy were built into our genes and our instincts, societies would not need prohibitions against deviating from it; the widespread prevalence of prohibitions and punishments for deviating from lifelong monogamy is a powerful argument that this is not the lifestyle that evolution intended for us. Instead, what is natural to human beings is what is also natural to chimps and especially bonobos: freely practiced,

promiscuous (but not indiscriminate!) sexual intercourse that serves to cement the bonds of the larger community.

Sex at Dawn draws for its assertions on a tremendous range of evidence from a variety of respectable and quite orthodox sources. Some points are quite surprising, and the whole book is written in a delightfully witty style. Here are a few of the many other points worthy of thought:

- “Many biologists advocate reclassifying humans, chimps, and bonobos together to reflect our striking similarities” (p. 23), namely lack of a tail, spending much time on the ground, highly intelligent, intensely social (p. 63).

- Monogamy is not found in any primates that live in large social groups (p. 97). Gibbons, the only monogamous apes, live in “small family units . . . isolated in a territory of thirty to fifty square kilometers” (p. 64).

- That humans have evolved for frequent sexual intercourse is demonstrated by (a) male “testicles larger than any monogamous primate would ever need” and “vulnerably outside the body where cooler temperatures help preserve stand-by sperm cells for multiple ejaculations”; (b) “the longest, thickest penis found on any primate”; (c) pendulous female breasts; (d) female capacity for multiple orgasms with “cries of delight (*female copulatory vocalization*)” (pp. 12–13, 224, 230, 235).

- The insistence that monogamy is nature-intended entails confusing sexual desire with love between human beings and makes for unrealistic expectations of lifelong sexual “fidelity” in marriage (Chapter 8).

- Men as well as women tend to be happier in matriarchal societies (pp. 72, 133–134).

- Much of the conventional wisdom about prehistoric humans is based, wrongly, on extrapolating backwards from what is observed in present-day societies (e.g., p. 75). The book calls this “Flintstonization”; historians use the term “whiggishness” for the mistake of judging the past by standards of the present. The book also coins the useful term “Yucatán,” short for “Remember the Yucatán,” to describe unwarranted, misleading interpretations: The Yucatán peninsula in Mexico was so named by the Spanish explorer Cortés through misinterpreting what the natives had said to him, which meant, in their native language, “I don’t understand you” (p. 19).

- Reminder: Natural evolution does not mean that things get *better* from our point of view (p. 36). Nor does what we call progress make everything better: “Stone age [pre-agriculture] populations lived healthier lives than did most of the people who came immediately after them. . . . And maybe than people who came *long* after them” (p. 175). Through inept statistical analyses, data from prehistoric skeletons have been misinterpreted as to average height and life expectancy (pp. 200 ff.).

- The Malthus argument, evolution by natural selection under the pressure of population growth, does not apply to humans (p. 156); foraging groups do not experience appreciable population growth (p. 159). Agriculture set off the increasingly rapid growth of human populations.

- Claimed knowledge of chimpanzee behavior is flawed: Data from captive chimps are no more reliable than studies of human prisoners would be characteristic of inherently human behavior (p. 67); Jane Goodall’s conclusions about violence and selfishness were based on studies that disturbed the natural order (pp. 67, 187–189).

Suggestions that male violence including warfare was common among hunter-gathering societies are also based on invalid examples (pp. 183–187).

➤ Humankind’s deadliest infectious diseases came from domesticated animals, following agriculture and settled living (pp. 206–207).

I hope these points will act as teasers to persuade everyone to read this book; its range of evidence and sources and scrupulously logical argument can only be appreciated by reading it all. One learns of inferences that can be drawn from variations in sexual dimorphism between species, and between human races;¹ the World Health Organization guidelines call for condoms to be of different diameters for various parts of the world (pp. 240–241). There is a discussion of *sperm competition* and how this can explain much about specifically human penises and the duration of human copulation (Chapter 17).

Part V of the book discusses how present-day cultural dogmas, notably about monogamy as biologically natural, are dysfunctional and the cause of much misery, setting out at length the basis for the remark in Chapter 1 that “*many, if not most, sexually dysfunctional marriages are nobody’s fault*” (p. 23, italics in original). The authors offer their insights as sound information, but say “We are not advocating any particular response to the information we’ve put together.”

Nevertheless, we should all be grateful for this opportunity to better understand ourselves and our interactions with others.

Note

¹ Humans differ biologically in ways that correspond to common definitions of race, and this is acknowledged for example in treating diseases and in choosing medications and doses. It is not *racism* to recognize that fact, understanding at the same time that these racial characteristics are *statistical averages and tendencies*. Racism is the misinterpretation that such stereotypes apply universally to the same degree in every individual, and that these *biological* differences imply and justify discrimination on intellectual, moral, or social grounds (see, e.g., Ruth Benedict, *Race and Racism*, 1942 and later editions).

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BOOK REVIEW

Forbidden Science 4: The Spring Hill Chronicles. The Journals of Jacques Vallee, 1990–1999 by Jacques Vallee. San Antonio, TX: Anomalist Books, 2019. 550 pp. \$22.95 (paperback). ISBN 978-1949501056.

DOI: <https://doi.org/10.31275/2019/1555>
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As Jacques Vallée is an inveterate scribe, *Forbidden Science 4: The Spring Hill Chronicles* is an excellent compendium of contemporaneous notes for the period between January 1990 and December 1999. While there is a customary focus on the business at hand, UFOs, he also includes a wide variety of personal information such as his health, emotional state, cinema exploits, tips on restaurants, and books that he and his wife, Janine, have read. The weather, especially if cold, also draws commentary.

Importantly, the role Janine played in Jacques' life cannot be overstated and she permeates the text, her guidance, counsel, and companionship vital to the exploration. Occasionally poems are included, and there are infrequent comments on the Dow Jones Industrial Average. He reminds us that it was in December, 1999, that the 10,000 mark was broken for the first time. Things have changed. Also of interest may be the running narrative of the rapidly changing geopolitical circumstances that exert influence on all topics being considered. Conditions in France and America predominate, but emerging exigencies in many other countries are given consideration.

That the Soviets may have been concerned about a real extraterrestrial threat is learned early in the book. Many other surprising revelations soon follow. Hardly a topic on anomalies is missed, from remote viewing and abductions by extraterrestrials (ET) (and maybe the military) to fastwalkers and cattle mutilations. There is a dazzling array of contacts Jacques has made over the years, and very few of the extensive list of characters involved in such research are missing. This is a treasure trove of information about researchers; either he personally knows them or he has heard about them. One has but to explore the 43 pages of the index to conclude that he has extensive experience in several fields.

Like me, you may not run through the book very quickly. I found it necessary to have my computer handy and to engage in frequent Google searches on all manner of topics. As an example, he mentions being in the

woods at a meeting of “witches and warlocks” and singing *The Blood of the Ancients*. Damned if I didn’t find the song available and sung on the Internet.

Adroitly written, this book may make you want to reach for a dictionary from time to time (at least I did), and that does not count the sporadic inclusion of the French language. He employs consummate control of the vocabulary, and I admit looking up the meaning of *lugubrious* and *manzanitas*. The rich tapestry of this chronicle reveals background material on a vast array of topics. What is guaranteed is that you will learn things about subjects you never knew existed. One encumbrance is that the text is replete with comments that beg fact-checking or just invite further study. As extracts from Jacques’ diaries, many entries are tantalizing clues that continually will exercise the curiously minded reader.

One memorable quote by a French publisher, Robert Laffont, regarding Jacques’ earlier book, *Revelations*, will ring true for many SSE authors: “Rational books never sell as well as fake ones.” Another comment by his friend, Dan Tolkowsky, raises a perennial issue; “He said there was certainly something there, but was it a scientific problem?” Many SSE members believe that science can address the multitude of counterintuitive observations reported, while many of us have serious reservations about the confluence of materialistic definitions and seemingly ineffable experiences.

This is a veritable *Who’s Who* in the study of UFOs and other phenomena. In addition, also mentioned are friends, politicians, business acquaintances, and researchers from other fields. That includes literally hundreds of people. Writing this personal compendium, Jacques often uses the first names of his friends even if they have been absent for many pages. As an admonition, at times it may seem difficult to follow his train of thought or remember whom he is addressing.

There are interesting and probingly insightful commentaries. As an example, while contemplating international affairs in October 1996, Jacques writes, “America’s creative chaos turns to sordid apology for crime, its technology an excuse for sloppy information, virtual worlds of noxious drugs and rotten greed.” This visionary entry portended the devolution of information that vomited forth on the Internet infecting the intellectually minded and general population alike.

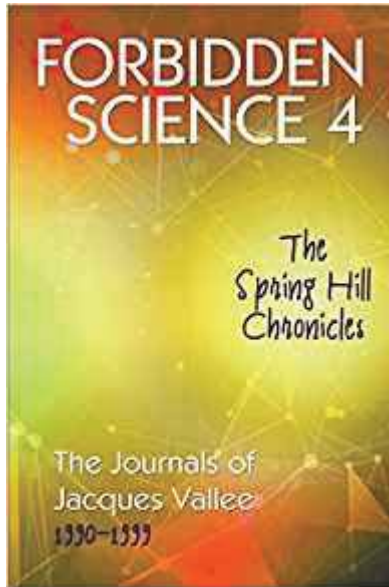
Sub-rosa context points to the fundamental flaws in UFO and related studies of various phenomena. They include endemic secrecy (both official and otherwise), a reluctance/failure to communicate, and egoism that leads to intellectual cannibalism. It is my opinion that these phenomena are at least as complex as cancer and require an integrated approach with extensive data sharing. Such is not the case today.

One can't help but note a recurring strain of frustration generated by the notion of ineptitude in many contemporary researchers in the field. We certainly agree on a significant problem related to all groups studying UFOs or other phenomena. When funds are provided, "one can only work on theories that are aligned with the particular framework, that of the sponsor." Repeatedly we have witnessed dilettantes entering these fields, always expecting quick answers to complex problems, but only those in their specific area of interest. Another area that Jacques and I certainly agree on is summed up in his quote: "The point that irks me most in the ufology dogma is the absurd idea that the Aliens gave us modern technology." Like a zombie, that notion keeps coming back to life no matter the evidence to the contrary. But then, we now are living in a post-truth world.

There is little doubt that many readers will not agree with some of Jacques' analysis and conclusions on certain cases. Such is the lot of all UFO researchers and enthusiasts. The pieces never all quite fit together in a consistent pattern. That may be part of the cerebral attraction of the field for some—as well as a piece of the repulsion for others. What will be found are many enticing clues sprinkled like a trail of crumbs waiting for intrepid investigators to follow. Exactly where they will lead is unknown. Can we assume that a *Forbidden Science 5* will continue the exploration with the turn of the century?

For any serious researcher or student of UFO phenomena, Jacques Vallée's *Forbidden Science 4* is not just *suggested*, rather it is *mandatory* reading. While you probably will not agree with all of his observations (and this community never agrees on anything), your understanding of the seemingly perverse interrelationships among people, concepts, and objectives will be broadened.

In his final reflections, Jacques addresses the personal nature of his diary. He indicates, "I have no ideology to present, and no theory to hype, no politician to lobby, and no axe to grind." As always, he remains an independent thinker.



For truth in advertising, readers should know that Jacques and I have interacted for a long time, and we did share a very moving experience. At 5:04 p.m., on 17 October, 1989, I was standing in his office on Sand Hill Road in Menlo Park, California, when the 7.1 Loma Prieta Earthquake struck. It killed 17 people and injured thousands while sending us diving for cover. Memorable indeed. Generously, he also provided a Foreword to my book *UFOs: Myths, Conspiracies, and Realities*, and he has included many references to me in this book.

—**JOHN B. ALEXANDER**

BOOK REVIEW

Our Secret Powers Telepathy, Clairvoyance, and Precognition: A Short History of (Nearly) Everything Paranormal by Terje G. Simonsen. Pari, Italy: Pari Publishing, 2018. 531 pp. \$14.23 (paperback), \$4.99 Kindle. ISBN: 978-88-95604-33-6.

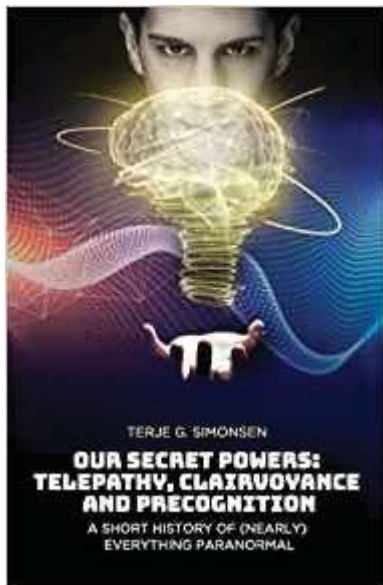
DOI: <https://doi.org/10.31275/2019/1561>
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Terje G. Simonsen is a Norwegian journalist who has written a very readable, inexpensive overview of the psi field for the interested lay public. It is fairly comprehensive, covering a wide swath of topics from anthropology and archaeology through laboratory experimentation to consciousness studies, philosophy, and research on reincarnation. It also includes references and links for each chapter, and separate indexes for subjects and names. These are very useful for the reader who would like to follow up and learn more about the many subjects covered.

The writing is clear, entertaining, and at times delightfully droll. (Some examples from his section headings are: “Indiana Schwartz and the Lost Treasures of Alexandria,” “Which is Further Away: York or New York?—Entanglement and Non-locality,” “Ian Stevenson: Reincarnation, Again, and Again and Again . . .”). He presents dry data in interesting and amusing ways, and the book is sprinkled with lots of intriguing anecdotes.

Not that there isn’t anything for those of us who are familiar with most of what’s presented here: I found much that I had no knowledge of prior to reading it, for example some information about psi research in Simonsen’s homeland, and a good review of some material I hadn’t considered in a long time. For instance, I had known a bit about the Bulgarian psychiatrist Giorgi Lozanov, but I had only a hazy memory of his work with a woman called Baba Vanga, a quite extraordinary psychic medium and healer who was actually given a state salary at one time. The story is engrossing and ends with the author’s meeting with the then-octogenarian Lozanov who by that time had been held under house arrest, banned from contact with Westerners, and had most of his research material confiscated by that same state.

When asked how the amazing psychic feats of Baba Vanga were possible, Lozanov answered with what I think is perhaps the most important concept the reader should come away with: “The limitlessness of Mind!”



Lozanov elaborated further saying that he believed that all learning and developing, “normal” and “paranormal,” had to do with the steady opening up to more of this limitlessness. In other words, as Simonsen explains, “Consciousness is not limited to the brain, but . . . also has nonlocal and non-temporal dimensions. . . .” (p. 97)

Much of the psi field is covered—and covered well—in this “short history,” but as the subtitle suggests by the qualifying “(nearly) everything,” there are some omissions. I was disappointed to find an inadequate discussion of macro-PK phenomena. He discusses Matthew Manning’s “poltergeist” experiences, but there is

no mention of the in-depth research into RSPK at all. Simonsen very briefly mentions Rudi Schneider and Nina Kulagina, but he gives the impression of seriously doubting or not taking seriously their abilities. He spends a lot more time on Uri Geller and seems more convinced of the Israeli’s abilities, but there is not a word about D. D. Home, Eusapia Palladino, Sir William Crookes, or Charles Richet, to mention a few glaring omissions, anywhere in the more than 500 pages. Neither is there any discussion of psychic photography: You will not find Serios or Eisenbud in the name index. This is an unfortunate omission in an otherwise fine overview of the field.

Perhaps Mr. Simonsen will look further into the area of macro PK and put out another book on the subject. He might start with my book (unabashed plug) *The Spirit of Dr. Bindelof* (2006) and follow it up with Stephen Braude’s *The Limits of Influence* (1986, 1991). And while it might be beyond his boggle threshold, he could take on Michael Grosso’s excellent *The Man Who Could Fly* (2016).

The only other “nit” I have, also having to do with a type of macro PK in this otherwise excellent book, is with the description of Kulagina’s stopping of a frog’s heart in a Russian laboratory and the implication that she might also be able to stop a human heart. (It caused quite a stir at the time.) The truth is the frog’s heart had been surgically excised and was kept beating in a saline solution with electrodes attached to it to record its activity. Kulagina was asked to try to make the heart beat faster, which she

did, and then to stop it using her PK. The experiment was misrepresented in the journalists' Ostrander and Schroeder's popular, but not always accurate, book *Psychic Discoveries Behind the Iron Curtain* (1974), which Simonsen cites as one of many references. This story and subsequent claims by a former KGB officer suggesting that psychotronic weapons caused Boris Yeltsin's heart attack, only added to the public's—and many scientists'—fear of psychokinesis.

But I was glad to see in Simonsen's book a discussion of the taboo among so-called “hard” scientists about parapsychology. In a chapter on archaeology he writes:

For a scientist to meet with a psychic is likely to be a *liaison dangereuse*, which might easily develop into a brutal career killer. If one is perceived to be unscientific or unserious it could swiftly become *adios* to collegial respect, promotions, scholarships, and support. (p. 48)

Later on, in discussing the lack of this kind of research in his own country of Norway, he acknowledges that the most likely reason is “the *stigma* that goes with superstition, or ‘parapsychosis’ as it was humorously dubbed by . . . Helge Krog.”

He then quotes the poet Andre Bjerke 's witty description of the stigma:

It is more dangerous for a professor to be caught red-handed doing research [on the paranormal] than it is for a Conservative politician to be found dead drunk in a brothel. What hitherto has dictated university science's sentiment towards parapsychology is an anxious sidelong glance towards colleagues: 'If it becomes known that I've seen a ghost, I'm done for at the faculty.' (p. 225)

Unfortunately this same attitude prevails in the U.S. and in many other countries as well. Perhaps books such as Mr. Simonsen's will help to change some attitudes.

—ROSEMARIE PILKINGTON

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BOOK REVIEW

Living in Flow: The Science of Synchronicity and How Your Choices Shape Your World by Sky Nelson-Isaacs. Berkeley, CA: North Atlantic Books, 2019. 320 pp. \$19.95 (paperback). ISBN 978-1-62317-311-1.

DOI: <https://doi.org/10.31275/2019/1563>
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While Sky Nelson-Isaacs is not the first physicist to be interested in the phenomenon of synchronicity—others who come to mind are Wolfgang Pauli, F. David Peat, and Walter von Lucadou—Nelson-Isaacs' new book *Living in Flow* is notable for its engaging and highly readable presentation of his particular theory about the relationship between quantum physics and synchronicity. Like the great idealist philosophers before him, Nelson-Isaacs takes mind to be the primary reality, and his theory explains how the contents of our minds—in particular the qualities of the experiences we anticipate having—shape the evolution of the physical world through the process of “meaningful history selection.” Nelson-Isaacs also links his theory to psychologist Mihaly Csikszentmihalyi's concept of flow, positing that we are best able to shape the evolution of the world in accordance with our desires when we are in the state of flow.

Nelson-Isaacs begins his book with the psychological component of his theory and works up to the quantum physics near the end, but I'm going to take the opposite approach here.

Those who have some familiarity with the basic ideas of quantum theory will be aware that the mathematics of quantum mechanics provides us with information about a physical system in the form of a collection of superposed states and their associated probability amplitudes. However, it has long been a matter of debate what causes one of these superposed states to become the state we ultimately observe. Many theories have been proposed, but none has been universally agreed upon. Nelson-Isaacs takes the position that it is the act of observation by a mind that causes a physical system to assume a determinate state (at least with regard to that observer). Furthermore, he hypothesizes that the qualitative experience anticipated by the observing mind influences which state becomes actual, with states more conducive to the anticipated qualitative experience being more likely to occur.

Nelson-Isaacs offers the concrete example of a woman rushing to catch a subway train so she can see a play at the theater. The woman is a bit late and is rushing specifically because she doesn't want to miss out on the delicious popcorn served at this theater—she is vividly anticipating the taste of this salty snack on her tongue. Nelson-Isaacs says that the woman's vivid anticipation is able to influence which possible states of her environment become actual, weighting the various possible states in favor of those that are more likely to produce the qualitative experience she's anticipating. For instance, if there's a cyclist boarding the train at the time that the woman is rushing to catch it, her mental anticipation of popcorn at the theater could increase the likelihood of the cyclist's getting their bike stuck in the train doors, so that the train is delayed in leaving the station and the woman has enough time to board. Or the woman's anticipation could increase the likelihood that the driver of the train will be momentarily distracted so that the train doors close a few seconds later than they normally would. There are many different ways that the woman's anticipated, qualitative experience of salty popcorn at the theater could be promoted by the way events unfold around her, and some of them might not even involve her making it to the play on time. As Nelson-Isaacs points out, it could be that she makes it to the play too late to be seated but then discovers that a nearby movie theater is playing a film she's been wanting to watch, and so she ends up in the movie theater munching on *their* salty popcorn and enjoying the very qualitative experience she'd been anticipating, albeit in a different context.

Nelson-Isaacs notes that his theory allows us to explain synchronistic experiences without appealing to supernatural powers of influence over other people or objects. This woman isn't *causing* the bicycle or the train driver to behave in any particular way. Rather, the quantum processes occurring in her environment are evolving in response to the qualitative experience on which she's focusing her mental energy.

Nelson-Isaacs also points out that his theory avoids the thorny paradoxes that are often associated with causation backward in time. Many synchronistic experiences seem to be built on past events being affected by what we are thinking or desiring *now*, but in Nelson-Isaacs' theory our anticipated qualitative experiences don't go back in time and change events that have already occurred, a situation that could lead to *Back to the Future*-type paradoxes, where one could be responsible for preventing one's own birth. Rather, says Nelson-Isaacs, the past only takes on a definite state or "history" when we observe it or its effects (and it only takes on that definite state for the mind that has made the observation). Since we have observed that we were in fact born, the past is (for us, at least) determinate with respect to that event, but it can remain indeterminate with respect to

many other details that we have not yet observed, including, perhaps, the specific details of our conception. Nelson-Isaacs calls this aspect of his theory “retroactive event determination,” and according to it the qualitative experiences we are anticipating now can, without paradox, influence the yet-indeterminate portions of the past toward possible histories that are conducive to the production of these experiences. As Nelson-Isaacs puts it, “*The end result makes the history fall into place*” (p. 208). And this is why he calls his overall theory one of “meaningful history selection.”

Now for the psychological component of Nelson-Isaacs’ hypothesis. Nelson-Isaacs admits that the universe doesn’t always seem to be giving us what we want. In fact, sometimes it can seem that events are continually falling into place so as to *thwart* our desires. What’s going on here? For one thing, many people besides ourselves are influencing the evolution of events in our world, and many of them may be influencing those events in a manner contrary to our own desires. But Nelson-Isaacs also emphasizes that his hypothesis is that the physical world responds to the experiences we *anticipate*, not necessarily to the experiences we *desire*. We may really want a certain thing to happen, but if we spend all of our mental energy focusing on how horrible we will feel if it *doesn’t* happen, then the physical world is going to evolve in a way that promotes that negative qualitative experience, rather than the positive one we would prefer. Furthermore, we may not always be consciously aware of the experiences we are subtly anticipating. Because the physical world so faithfully mirrors those underlying anticipated experiences back at us, paying attention to our environment can actually be an excellent way of noticing our inner thought processes and how they run counter to our expressed desires.

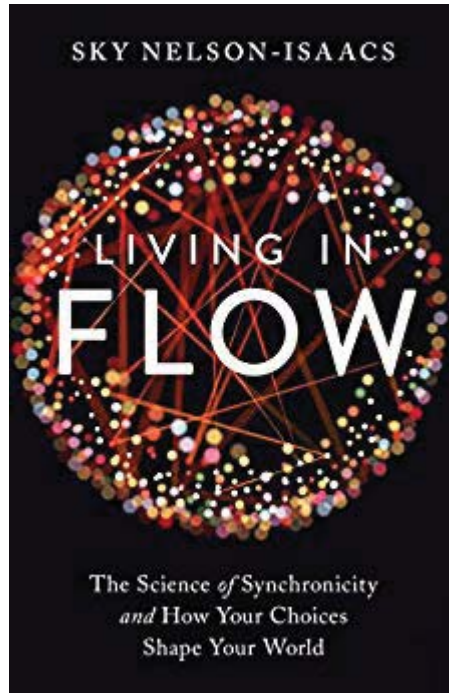
Nelson-Isaacs encourages readers to cultivate mental and emotional clarity so that “the cosmos can respond to [their] highest ideals, not [their] worries and fears” (p. 47), and he offers some practical advice regarding how to do this. He says that positive synchronicities—events that are in accordance with what we most deeply desire—are promoted by entering the state of flow described by psychologist Mihaly Csikszentmihalyi. In flow, says Nelson-Isaacs, there is a balance between inner drive and responsiveness to the environment. We are in touch with our highest inner purposes as well as in tune with the messages that our circumstances are sending us about the most fruitful paths to achieving those purposes, given what all the other minds around us are anticipating. In flow, we are not focused on the possible negative outcomes of events around us but instead are calmly aware of what we deeply desire as well as receptive to the creative ways in which the universe may respond to us. Nelson-Isaacs offers six practical steps for promoting flow, summarized by the acronym LORRAX: Listen,

Open, Reflect, Release, Act, and Repeat(X). When we are able to desire in this detached way, he says, the universe turns out to be highly responsive to our desires.

In the end, I find both the quantum mechanical and psychological components of Nelson-Isaacs' theory plausible. I've been carefully studying the phenomenon of synchronicity for almost a decade, and Nelson-Isaacs' theory aligns with many of the basic features of this phenomenon I have observed. (For instance, many people who have experienced strong synchronicities have reported their being accompanied by a relaxed but focused state of mind that it seems reasonable to connect to Csikszentmihalyi's

concept of flow.) It is certainly refreshing to hear someone who is well-versed in quantum mechanics—Nelson-Isaacs has an MS in physics and specializes in the foundations of quantum mechanics—put forward a theory that many less-qualified authors have only been able to gesture at. Physicists will probably wish that he went into more technical detail, and it's likely that philosophers and psychologists will also wish for more detail regarding the aspects of the theory that touch on their own specialties. However, one of the strengths of Nelson-Isaacs' book is the way in which he brings these disciplines together to create a unified theory that can spur the imaginations of lay readers and specialists alike, and create a framework for much future detailed technical work in all of these areas. And those who are interested in the quantum mechanical details of Nelson-Isaacs' theory can consult his scholarly articles on the subject, including one in this journal.

But, if I can understand why Nelson-Isaacs didn't give more technical details in this popular work, I do think his book would have been strengthened, even for popular audiences, by considering a wider range of synchronistic experiences. The particular real-life examples that he offers are rather mild and could be easily explained as mere chance events. It's hard to see why these particular experiences would motivate one to adopt



a revolutionary physical theory. More striking examples of synchronicities are certainly out there, as I show in my own recent book *The Source and Significance of Coincidences* (2019), and I believe Nelson-Isaacs' book could have benefited from including a few cases of this more compelling variety, some of which border on blatant psychokinesis.

I also discuss in my book evidence that many of the synchronistic events in our lives have their origins in sources external to us, only some of which are other living human beings. Indeed, it appears that synchronicities can at times deliver very pointed messages from other minds. I believe that Nelson-Isaacs' theory can accommodate and even help to explain this aspect of the phenomenon, and his book would be even stronger if it contained a discussion of this possibility.

Nelson-Isaacs' book could also have benefited from a comparison between his ideas and other theories that connect synchronistic phenomena to quantum physics: for instance, the entanglement theory proposed by Walter von Lucadou, Hartmann Römer, and Harald Walch (2007).

In spite of these minor deficiencies, however, *Living in Flow* is a highly valuable addition to the popular literature on synchronicity. There are not enough works out there that emphasize the role we play in creating our own synchronistic experiences, and this book is an important step toward a better understanding of the process by which our own minds determine the evolution of the world we observe.

—**SHARON HEWITT RAWLETTE**
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BOOK REVIEW

Redemption of the Damned: Vol. 1: Aerial Phenomena, A Centennial Re-Evaluation of Charles Fort's 'Book of the Damned' by Martin Shough with Wim van Utrecht. San Antonio, TX/Charlottesville, VA: Anomalist Books, 2019. 412 pp. \$39.95 (paperback). ISBN 978-1-949501-07-0.

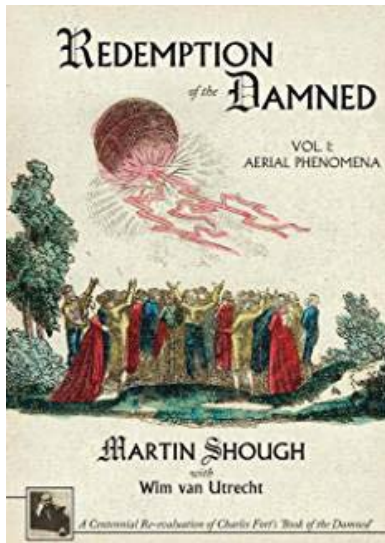
DOI: <https://doi.org/10.31275/2019/1579>
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Reading Charles Fort at an early age did my subsequent career and income no favor, but it did make my life more interesting. My admiration for Fort's cheeky attitude toward received wisdom has only expanded with time. I will always be, first and foremost, a Fortean.

The omnibus *The Books of Charles Fort* (1941) was what I read after consuming Edward Ruppelt's *The Report on Unidentified Flying Objects* (1956). So I signed up as a ufologist; Fort, after all, was the first of the species. In *The Book of the Damned* (1919), he argued for the reality of interplanetary visitation, his serious purpose only thinly masked in jokey prose. In the summer of 1947, when flying saucers became an inescapable presence, it was Fortean who alerted otherwise clueless journalists to the long history of intriguing aerial phenomena, thus introducing what would be called the extraterrestrial hypothesis (ETH) into popular culture and subsequent discourse.

On the occasion of *Damned's* first century, Scottish UFO scholar Martin Shough, assisted by Belgian colleague Wim van Utrecht, offers up a kind of revisionist answer to Fort. *Redemption of the Damned, Vol. 1: Aerial Phenomena* uses the Internet and other tools to examine Fort's sources and to analyze how they look through the lens of current scientific knowledge. Fort helpfully provided future anomalies chroniclers with source citations that let us know where his data came from. Overwhelmingly, they were from period journals and newspapers. Shough with Utrecht finds his way to those and reconsiders the cases at considerably greater length, which Fort had typically summarized, often with accompanying wisecracks, in no more than brief sentences and paragraphs.

Redemption seems an odd word to choose in the present circumstance, however, inasmuch as the results might be better characterized as Corlissean



rather than as Fortean. As *JSE* readers know, the late William R. Corliss collected reports of anomalies from the professional literature of various disciplines and compiled them into a series of books. With a handful of exceptions (most notably accounts of living amphibians entombed in rocks), the reported phenomena were unusual, unexpected, but not paradigm-shattering, likely to interest reasonably open-minded specialists more than radical anomalists, i.e. Forteans.

The research that has gone into *Redemption* is nothing short of staggering. Shough appears to know everything, only starting with the meteorology or astronomy or biology relevant to the circumstances but also embracing technology, transportation, and infrastructure as well as eyesight capacity, biography, culture, and political, social, and geographical contexts. Beyond that, Shough, already possessed of a considerable reputation within ufology for smarts *and* fairness (often enough not synonymous), inspires trust even when his argument sails well above the intellectual ground where the reader feels more at ease in traversing.

There is, one might say, an abundance of debunking going on, but not of the ideological sort that has given the practice a certain unpleasant odor over the years. As those who have read his deeply researched writings on UFOs (such as his and Chris Aubeck's *Return to Magonia: Investigating UFOs in History*, 2015) know, he has no problem with the notion that UFOs exist as a real and puzzling phenomenon. Moreover, he has proved himself, time and again, an articulate and effective critic of the effusions of professional skeptics.

Thus *Redemption* is a worthy and necessary addition to the small library of scientifically and informationally weighty UFO volumes. From this day forward, no naïve reader ought to plunge into Fort's *Book of the Damned* without Shough with Utrecht close at hand.

That said, one should be warned that read from cover to cover, *Redemption* is, to be charitable, heavy-going. One can imagine that someone trained in one or the other of the sometimes arcane disciplines on which the treatments draw might possibly read it for pleasure. For the rest

of us, though, it's best employed as a reference work to be taken off the shelf whenever we encounter a *Book of the Damned*-derived ostensible-UFO datum and seek a second opinion. We couldn't ask for a more credible one.

—**JEROME CLARK**
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BOOK REVIEW

Diabolical Possession and the Case Behind “The Exorcist”: An Overview of Scientific Research with Interviews with Witnesses and Experts by Sergio A. Rueda. Jefferson, NC: McFarland, 2018. 265 pp. \$35 (paperback), \$9.99 (Kindle). ISBN 978-1476673844.

DOI: <https://doi.org/10.31275/2019/1587>
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Sergio Rueda’s book *Diabolical Possession and the Case Behind “The Exorcist”* is a detailed case study of the real events that inspired William Peter Blatty’s 1971 novel *The Exorcist* (1971) and subsequent movie, released in 1973, of the same name (Blatty et al. 1973). This book’s connection to the popular fiction novel and the even more popular horror genre film that followed creates a prominent marketing appeal. However, to Rueda’s credit, references to Blatty’s book and movie are made judiciously throughout the text and are then relevant connections with the real case. That being said, however, there is little in the way of an actual manifestation of a demonic possession that can equal the spin that Hollywood special effects can present to theatergoers (no pun intended). The victim of possession that Rueda describes from the actual case never has his head spin 360 degrees, nor projectile-vomited green vomit, nor appeared in all His demonic glory to be Satan himself. Blatty’s story is highly fictionalized and presents the “dark inner voice” we all possess as an archetypal force in the collective unconscious in a literal, yet also metaphoric, way. The real thing is quite a bit subtler, and thus easier to sweep under the table.

The actual case that Blatty’s *The Exorcist* is based on, and the subject of Rueda’s book, is a demonic possession case that took place in 1949 and involved a 14-year-old boy named Ronald who lived with his mother and father in Mount Rainier, Maryland, just minutes away from Washington D.C. Rueda claims that this Mount Rainier case is the most-documented possession case in history, and consequently the most famous. Blatty was indeed inspired by the case and based, sometimes loosely, his even more famous tome on the 1949 events. Most of Rueda’s book, and I would assume most of Blatty’s book as well, was inspired by a document Rueda calls “The Jesuit Report” which was a written narrative by several of the priests involved with the Ronald possession case. Rueda claims to have found this report, along with pertinent correspondence between J. B. Rhine, the

celebrated parapsychologist who headed the Duke Parapsychology Labs at Duke University from 1935 to his retirement in 1965, and Rev. Luther Miles Schulze, one of the first witnesses to Ronald's alleged possession. Rueda stumbled across these documents during his tenure at the Foundation for the Research on the Nature of Man, an organization the retired Rhine established in 1965. Quite an intriguing story already, and again, to Rueda's credit, one of the most informational, and credible, foundations for his book—that and the interviews he conducts with some of the key participants, including Ronald himself, more than 50 years after the actual events.

There have been several other books and articles written about this case—Rueda cites a few of them throughout his book, most notably *Possessed: The True Story of an Exorcism* by T. B. Allen (1984). When asked by a colleague why yet another book on Ronald's possession, the author remarks:

... the issue remains important at the present time, not only because *The Exorcist* is still a scary movie, but also because the book and the film frame the long and ongoing religious and philosophical struggle between good and evil, and people from all walks of life are still interested in this subject. My effort also sheds new light on the investigation of this type of demonic possession cases from scientific, psychological, and religious perspectives, which will expand the knowledge about the case and clarify the truth behind the famous film with up-to-date dramatic information. (p. 7)

Rueda, himself a clergyman, clearly has a personal and spiritual interest in what he reports in his book. However, he makes great efforts to keep his conclusions objective and does attempt to be scientific in his approach even though he is not a trained scientist—open minded, reasonably skeptical, and careful to investigate all possible angles, including fraud.

The book, apparently an adaptation of a Ph.D. dissertation, is laid out in a typical fashion—a preface, introduction, 16 chapters, 5 appendices, notes, a bibliography, and a (regrettably) minimal index. Rueda spends the first pages describing the sources for his narrative (including the aforementioned “Jesuit Report”) as well as more detail on Blatty's book and movie *The Exorcist*—describing the similarities, differences, and important points Blatty found necessary to exploit in his fictional accounts of the case. Rueda also brings forth ideas regarding the importance of the public's responses to Blatty's work—where a demonic possession of a human being constellates collective unconscious archetypes even in our modern, scientifically objective, psyche. Rueda then proceeds with a chronologically inspired order of chapters beginning with the first events of the case when young Ronald begins to experience strange phenomena such as odd personal

behavior—feeling unusually sick, throwing his pillow at the ceiling light in his bedroom, being abnormally belligerent or defiant, as well as external physical phenomena—various noisy creaks and knocks, his bed violently shaking, and mysterious writing appearing on his body (although it should be noted that on several occasions only the parents could make out actual English words on Ronald’s body, others saw only rash-like markings)—among other things. Subsequent chapters investigate the ensuing procedures to bring young Ronald back to normalcy, which, in this case, become quite complicated.

There is much room in the proceedings of this case to doubt credibility, as Rueda is careful to point out. The whole family situation, for example, is odd—a boy who doesn’t want to attend school, an aunt who was a spiritualist and to whom the boy was close dies early on in the story, séances, Ouija board consults, parents who change faith from Catholicism to Lutheranism and believe they are being punished for doing so, the initial involvement of the Catholic Church to perform an official exorcism, among many other unusual details about the family and the clergy involved. In fact, Rueda declares that Ronald experienced two actual exorcisms, and even contemplates that an official possession, as described by the Catholic criterion set forth in the official doctrine of Satanic exorcism, *The Roman Ritual*, was not ascertained until the second exorcism actually began.

Rueda makes great effort to explore all possibilities, with several chapters dedicated to subjects such as “Science in Search of the Truth” and “The Art of Trickery.” In the latter chapter, Rueda methodically examines every possible explanation for the phenomena experienced by Ronald et al. Some of these possible explanations border on the ridiculous and seemingly are implausible (such as the father being in collusion with Ronald’s efforts to not attend school and through his skill as an amateur tinkerer concocts devices that, when planted under the floorboards of Ronald’s bedroom, could cause, on remote demand, scratching and thumping sounds). Rueda concludes his heroic effort at skepticism with this remark:

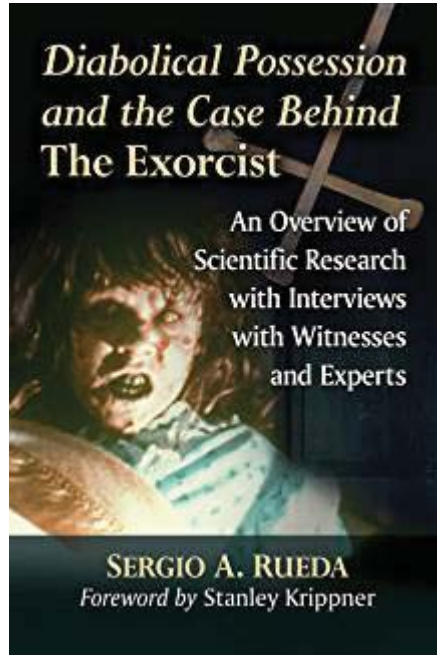
It is, however, important to remark that the fraud hypothesis cannot by itself explain all the phenomena in this case, such as the spontaneous marks that later appeared on the boy before several witnesses—particularly those that appeared on his body during the exorcism that took place in St. Louis at the Alexian Brothers Hospital. In this particular event, the witnesses are much more reliable than the parents, as they included priests and probably some mental health professionals or medical doctors, who may well have participated in the process before the exorcism resumed, even though we have so far no actual records of such participation. Marks spontaneously appeared on the boy while many people were looking at him, and it is hard to imagine that all the participants were deceived by him. (p. 87)

Schulze, the original Lutheran clergyman who first examined Ronald, considered himself a die-hard skeptic. He was, however, convinced that the phenomena he experienced with Ronald were authentic. He said, “The boy was not faking it. I wondered if I was going screwy” (p. 101). There were many other witnesses whom Rueda cites, who came to the same conclusion—trickery was not a factor.

Rueda also investigates other possibilities than demonic possession. He makes a clear effort to distinguish a differences among possession, poltergeist activity, Dissociative Identity Disorder (at the time of Ronald’s experience this disorder would have been called Multiple Personality Disorder), and other psychological conditions, both natural and supernatural, that could explain all that happened in this case in 1949. He spends much time in this section examining classic poltergeist activity, and comes to the conclusion that although there are many attributes of the case that fit the poltergeist criteria, there are too many that do not. He in several spots in the narrative suggests that possibly the exorcism itself turned what could have been deemed an “ordinary” obsession (and possibly poltergeist) situation into a bonafide possession case. He said:

The Mount Rainier case constitutes a good example of how superstition and belief in demonic possession may combine to create a real case of demonic possession. In this regard, Oesterreich (1966) observes, “At so primitive levels of culture and with patients of such enhanced autosuggestibility, it is not surprising that a state of possession should readily arise.” (p. 95)

It is also important to note that most of the possession material presented in this book is through a Roman Catholic frame of reference, or at the very least a Christian-Biblical perspective. It does seem to be assumed in our modern culture that the Roman Catholic Church “wrote the book” on demonic possession. This is primarily due to our Western cosmology, which is deeply influenced by a Judeo-Christian paradigm. Although this



may seem comfortable to our Western sensibilities, it is by no means accurate. Demonic possession, as well as any variety of spirit possession, can be found in nearly every culture throughout the world (Laycock 2015). Since the Mount Rainier possession case was handled predominately by the Catholic Church, it stands to reason that Rueda's book is going to be presented predominately through a Roman Catholic lens.

Rueda also makes it clear that the officials from the Catholic Church, who were either assigned to the task or took possession of it by their own authority, could have incompetently handled the whole process. He feels that not enough effort was made to investigate a psychological or physiological etiology to the phenomena, an effort that would have been exhaustive and conclusive in today's climate. Even the Roman Ritual demands that a subject meet certain clear criteria before being deemed officially possessed. As said before, there is some question whether Ronald met these criteria only *after* the final exorcism began, rather than them being the basis for a church-sanctioned exorcism.

Toward the end of the book, Rueda recaps the previous investigations into the various possibilities that the Mount Rainier case presented—fraud, natural (psychological) etiology, poltergeist, and demonic possession. Even if one concludes it was indeed a demonic possession, was it then induced by suggestion? This reader was reminded throughout the experience of reading this book of the New Thought concept of creating one's own reality—it is done unto you as you believe. How many spirits, evil or benevolent, are injected into some sort of reality, material or non-material, through intention, belief, and thought? Rueda hints at this himself, making a comment in the final chapters of his book,

The human condition becomes malignant under the influence of the devil, which has deteriorated the values of society with wars, exploitation of the disadvantaged, and injustices of many kinds. It seems to me that this message is more important than whether or not the devil or demonic possession exists, which is, of course, a matter of faith for those who believe it. (p. 158)

The book's chapters are followed by several appendices, which prove to be some of the most interesting parts of the book. Here Rueda expands upon many of the evidential aspects of his narrative, such as his sources of information, letters, details on the Jesuit Report, and several very interesting interviews (most notably an interview with Luther Schulze, the first clergyman called into the case). He also speaks with Ronald himself on the phone for a brief conversation, and although not very informative (Ronald said he would speak to Rueda but would not talk about the case) it

adds a bit of grounding to an otherwise “other worldly” story.

Unfortunately, Rueda’s writing style leaves a bit to be desired, nothing a good editor could not have remedied. It also seemed at times that his chapters were originally written as stand-alone articles, and much that had already been thoroughly explained before is in each subsequent chapter reiterated. That being said, those interested in exorcism and spirit possession in general, and the Mount Rainier case in particular, would likely benefit from reading this book. Rueda makes great effort to present the facts about the case in much detail, and adds his own questions, thoughts, conclusions, and uncertainties to augment an already richly detailed account. He thoroughly examines the case from scientific, religious, parapsychological, and skeptical (fraudulent) perspectives. The connection to Blatty’s book and movie *The Exorcist* is passively interesting but could have possibly been omitted or only casually mentioned. However, Blatty’s views on the real purpose behind his work (a commentary on the morals exemplified in a 1970s deteriorating society, on page 158), as Rueda points out, are clearly relevant.

—TODD HAYEN

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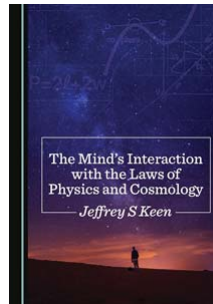
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CORRECTION

Erratum for the “ESSAY REVIEW: Subtle Energies and Quantum Mechanics,” which appeared in the Summer 2019 *Journal of Scientific Exploration*, 33(2), pp. 277–287.

DOI: <https://doi.org/10.31275/2019.1605>
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The Essay Review began by listing the two books under review, where the price for the second book, *The Mind's Interaction with the Laws of Physics and Cosmology* by Jeffrey S. Keen, was incorrect, and which is here shown with the correct price of **\$33.95**:



Subtle Energies and Quantum Mechanics

DOI: <https://doi.org/10.31275/2019.1453>
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From Quantum Physics to Energy Healing: A Physicist's Journey to Mind and Healing by Johanna Blomqvist, translated by Tuula Yrjö-Koskinen. Helsinki, Finland: Mindstream Publishing, 2018. 294 pp. \$16.16. ISBN 978-9529404186.

The Mind's Interaction with the Laws of Physics and Cosmology by Jeffrey S. Keen. Cambridge Scholars Publishing, 2018. 435 pp. **\$33.95** (paperback). ISBN 978-1527513648.

This price has been corrected in the online versions of the Review on both the Society for Scientific Exploration website:

<http://www.scientificexploration.org/journal/volume-33-issue-2-2019>

and the Journal of Scientific Exploration website:

<http://journalofscientificexploration.org/index.php/jse/article/view/1453>

The editorial staff of the *Journal of Scientific Exploration* regrets the error.



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<https://www.scientificexploration.org/conferences/2020>

In addition, the SSE is also offering a 50% discount on future meeting registrations for any student member who brings one student friend to our conferences (one discount per student). We are eager to see student clubs or SSE discussion groups established at various academic institutions or in local communities. Contact us at education@scientificexploration.org to start your own group!

C. M. Chantal Toporow, Ph.D., SSE Education Officer
education@scientificexploration.org

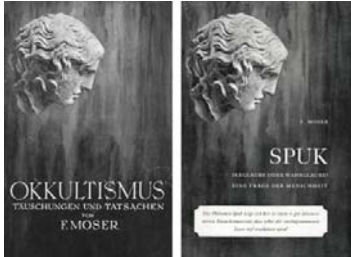


Institut für Grenzgebiete der Psychologie
und Psychohygiene e.V. (IGPP)
Freiburg im Breisgau

Fanny Moser Award



Description: Dr. Fanny Moser (1872–1953) was one of the first women to study medicine and natural sciences in Freiburg, Zurich and Munich. She received her doctorate in 1902 with a zoological thesis. In 1914, she took part in a mediumistic séance and witnessed a spectacular table levitation that shattered her scientific world view. In the following decades, supported by a unique source collection and research library, Fanny Moser undertook a critical examination and reappraisal of the entire field of Mesmerism, Hypnotism, Spiritism, Occultism and early parapsychological research up to the 1930s, which led to the publication of her opus magnum *Okkultismus – Täuschungen und Tatsachen* (München 1935, Reprint 1977). Together with her second major volume *Spuk - Irrglaube oder Wahrglaube? Eine Frage der Menschheit* (Zürich, 1950 Reprint 1977), published in 1950, Fanny Moser bequeathed – from a historical point of view – two groundbreaking works on German-language parapsychological research.



In her will, Fanny Moser decided to create a foundation to establish and secure research in the tradition of her two works. She assigned this task to the pioneer of academic parapsychological research after the Second World War, the Freiburg professor of psychology Hans Bender (1907–1991), and to the Institut für Grenzgebiete der Psychologie und Psychohygiene e.V. (IGPP) founded by him in 1950. Fanny Moser thus became the IGPP's first patron, and her legacy enabled a part of the Institute's research and counseling work to be carried out in a modern way for decades.

The testamentary decree also stipulated that a prize should be awarded regularly for the "best work" on the research topics she herself had studied. This prize was awarded for the first time in 1982; Eberhard Bauer (IGPP Freiburg) is the only winner to date. On the occasion of the 70th anniversary of the IGPP in 2020, the testamentary decree is to be fully enacted. The Fanny Moser Award is endowed with 3,000 euros and is to be awarded regularly every three years.

Requirement: The prize is to be awarded to a scientifically published work (including outstanding qualification work). The publication must be explicitly related to Fanny Moser's research on paranormal and anomalous experiences and phenomena. The subject can be empirical-experimental, theoretical-conceptual, clinical-therapeutic, natural scientific, art historical, cultural scientific, social scientific or historical. The publication of the work should not have taken place more than three years ago and should document an outstanding academic achievement.

Modalities: Proposals and applications must be sent to the jury by email by **15 January 2020**. They should contain the following documents: a copy of the scientific work to be considered, a letter of application and a curriculum vitae. The award ceremony is expected to take place in Freiburg in May 2020.

Email: Fanny-Moser-Preis@igpp.de

Internet: <http://www.igpp.de>

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Thank you. Dani Caputi, University of California, Davis, djcaputi@ucdavis.edu

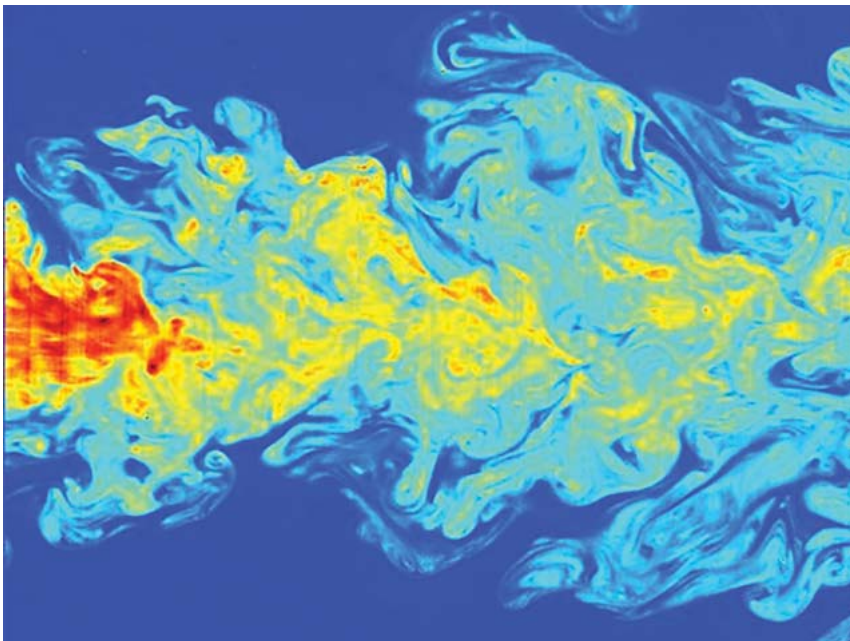


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THE BIAL AWARD IN BIOMEDICINE 2019 seeks to recognize a work published in the biomedical field (free theme) from January 1, 2010, onward, the results of which are considered to be of high quality and scientifically relevant.

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