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EDITORIAL

Periodicals of various sorts have long recognized the need to address certain topics on a regular basis. That's why computer magazines routinely offer articles such as "Windows Tips and Tricks," and "How to Protect Your Data." Similarly, photography magazines return again and again to articles explaining how to get the most out of wide-angle lenses, how to shoot portraits in natural light, or how to photograph dramatic landscapes. It seems to me that *JSE* editorials might also need to recycle certain topics from time to time, in part because readership changes, and in part because researchers in areas of frontier science can have conveniently short memories (like everyone else), perhaps especially when it comes to matters that are intellectually or professionally challenging or uncomfortable.

The continuing debate over Daryl Bem's recent precognition experiments (see Bem 2011, and the Editorial in *JSE* 25:1) and the similar controversy still dogging work on LENR or "cold fusion" suggests that perhaps it's time to review certain salient facts about the nature of experimental replication in science. What follows is not new. Harry Collins has done outstanding work on this topic (Collins 1992), and I also addressed the issue at length (Braude 2002). For more recent commentary, see also Stefan Schmidt (2009). Apparently, however, what's both obvious and commonsensical is very easy to overlook.

Of course, it's clear enough why so much emphasis is placed on the replication of experiments, not just in parapsychological and LENR research but in other areas of science as well. Experimental replication would seem to be an obvious and straightforward means of legitimizing experimental results. The underlying idea is that if an experiment *E* gives a certain result while attempted replications do not, we have good reason to regard *E*'s result as spurious or inconclusive. And if continued attempts to replicate *E* fail to duplicate *E*'s result, we have (so the story goes) good reason for regarding the outcome of *E* to be due to a flaw in *E*'s experimental design, or to experimental negligence or incompetence, and perhaps even to chicanery. So the received view is that the only legitimate experimental results in science are those that can be repeated reliably, and in this way scientific repeatability has served as a kind of supplementary demarcation criterion (after falsifiability) between science and non-science (or pseudoscience).

I assume that nearly all *JSE* readers are familiar with this story. But I have to wonder how many of them realize that it rests on an unacceptably naïve conception of what experimental repeatability actually is, as well as an even deeper conceptual confusion over the nature of similarity. The former is simply a special case of the latter.

The first point worth considering is that, despite considerable scientific posturing to the contrary, when it comes right down to it—especially in situations when the scientist's own work is on the line—experimental replicability *in fact* is rarely (if ever) considered to be an essential feature of genuine science. Rather, it's typically regarded as such primarily in politically charged debates over psi research, LENR, and some other areas of frontier science. In those debates, defenders of the replicability requirement (let's whimsically call them *replicants*) seem conveniently to forget, first of all, that criteria of (and reliance on) replicability vary considerably from one area of science to another. Not surprisingly, these differences are especially pronounced when we compare behavioral sciences to nonbehavioral sciences. But even in the physical sciences, the importance of (and reliance on) replicability varies greatly—say, from geology and astronomy (not to mention cosmology and meteorology) to physics and chemistry.

But a much more serious problem is that the very *concept* of experimental replication is exceedingly crude. To see why, let's begin by asking: In what respects can replication attempts differ from the original experiment? It's clear, first of all, that no replication attempt can ever be *exactly* the same as the original, if only because of changes in the time and place of the experiments. But of course those differences will be accompanied by differences in the general conditions of the experiment or in the experimental environment. And these may include differences in the actual participants. But even if the participants remain the same, we can expect changes in their attitude or mood, or even in the condition of the experimental apparatus required (especially sophisticated, sensitive, or delicate equipment), all of which might vary subtly or dramatically from one test to another.

Notice that—even in the "hard" sciences—these sorts of differences between experiments and their replication attempts are tolerated all the time (if they're noticed at all). In physics, an experiment conducted at laboratory L with a certain kind of particle accelerator might be replicated at laboratory L' with a different design of accelerator. In microbiology, experiments conducted with microorganism M in solution S might be replicated by studying M in a different solution S' (which may have been more convenient to use, but whose differences are considered insignificant). In fact, even a different microorganism M' might have been substituted and its difference discounted. And of course, despite the expectations of the replicating scientist, it's always possible that such differences between experiments lead to differences in experimental outcome. For example, in physics, some of the differences between experiments and their attempted replications might account for the mixed results of efforts to test the EPR paradox and hidden-variable interpretation of quantum mechanics. In fact, these attempts didn't even study the same particles. One used proton pairs (McWeeny & Amovilli 1999), and the others, photons (e.g., Freedman & Clauser 1972, Aspect, Dalibard, & Roger 1982, Aspect, Grangier, & Roger 1982). Yet they're all considered versions of the same experiment,[1] originally proposed in a thought experiment by David Bohm, but which involved electron-pairs (Bohm 1952a, 1952b, Bohm & Aharonov 1957). At any rate, the standard procedure in cases such as this is to ignore the differences between these experiments so long as their results *more or less* agree, and thus to regard the follow-up experiments as replications of the earlier ones. But if the experiments produce sufficiently dissimilar results, the standard procedure is to regard the later experiments as failing to replicate the former.

There's a very important moral to this story. If we pay attention to the way the business of science is actually conducted, what we find is that criteria of experimental replicability are both very loose and never fully specified. In fact, scientists don't decide whether follow-up experiment E_2 counts as a replication of original experiment E_1 until the results of E_2 are in. It's certainly not decided solely on the basis of formal features of the two experiments—something potentially expressible in a "recipe" or unambiguous and complete list of all relevant procedures. On the contrary, when scientists agree that E_2's results match those of E_1, they will simply ignore the unavoidable and potentially relevant differences between E_1 and E_2, declare that E_1 has been replicated, and (in some cases) conclude that the results lend confirmatory weight to a shared, underlying, and trusted theory. But if E_2 fails to yield the hoped-for (and possibly only approximate) duplication of E_1's results, the standard reaction is to suppose that the inevitable differences between the two experiments in fact made a difference and that this failure does not automatically cast doubt on or discredit the original experiment's results or the shared underlying theory. As a rule, then, both avoidable and unavoidable differences between experiments and replication attempts are tolerated all the time, and ignored so long as the results pan out more or less as expected, but invoked when results go the other way.

Another way to put the point is this: Whether or not the differences between E_1 and E_2 count as relevant is *not* determined independently of the decision as to whether the latter replicates the former. Scientists tend to regard many such differences as important only if the outcomes of the experiments differ. But before knowing the results of E_2, it's pretty much an open question whether the differences between E_1 and E_2 matter. Of course, scientists may claim in advance that the differences don't matter, but if the replication attempt fails to give more or less the same results as the original experiment, they may retract that judgment.

The situation changes somewhat when a series of replication attempts fails to consistently produce results similar to the original experiment. But even then (as we've seen recently with attempts to replicate Bem's experiments), the same general attitude about replicability prevails. When the later experiments fail to produce positive results like those obtained by Bem, the conversation focuses, for instance, on the differences in the protocols, or the different attitudes of the experimenters. And again, it's likely that these differences would also have been ignored had the later results all been positive. After all, some attempts to replicate Bem's experiments *have* been considered successful, and they're not strictly identical to the experiments Bem originally performed. Furthermore, there's nothing inherently suspicious or unsavory about this. That's simply the way science works, and given the inevitable differences between original experiments and replication attempts—magnified in the behavioral sciences by many additional kinds of potentially relevant variables—it's the only way it can work.

Interestingly, many consider replication attempts successful and convincing *only* when they're conducted by someone other than the original scientist. In part, I suppose, it's because they believe that any legitimate experiment can be described in a list of procedures which any competent scientist should be able to follow and produce the same results. For example (and somewhat notoriously), Karl Popper wrote, "any empirical scientific statement can be presented (by describing experimental arrangement, etc.) in such a way that *anyone who has learned the relevant techniques* can test it" (Popper 1959:99, emphasis added). This position is especially dubious when applied to parapsychology, alternative healing experiments, and the behavioral sciences generally, where experimenter expectancy effects and the variability of subject–experimenter interactions are particularly problematical. But it's also an obviously questionable position to take with respect to any area of frontier science, where the relevance of numerous and unavoidable differences between experiments hasn't yet been determined. In fact, I'd say that one of the most important lessons learned from the behavioral sciences, and reinforced by studies in many areas of frontier science, is that it's still an open question whether it's reasonable to expect success when replication attempts are conducted by someone other than the original experimenter. Moreover, it's unclear to what extent this might be an issue in mainstream science, where (as Rupert Sheldrake has noted (1998)), double-blind protocols are typically neither used nor even taught as sound methodology, and where potential experimenter effects are not even on the radar.

As I mentioned above, some difficulties in determining when an experiment has been repeated are not peculiar to the scientific enterprise or to the process of experimentation. Rather, they're an instance of the more general problem of determining when *any* sort of event has been repeated. These problems, in other words, concern the general concept of *recurrence*, and even more fundamentally, the concept of *similarity*.

Suppose that *A* tells a certain joke and that his telling of the joke, *J*, is very funny. But suppose that *B*, who is not as comedically gifted as *A*, tries to tell *A*'s joke using different words, inflection, and timing, as a result of which his joke-attempt *J′* is not funny. How, then, do we answer the question: Is *J′* a recurrence of joke-attempt *J*? The important thing to observe here is that this question has no simple or straightforward answer. There are perfectly acceptable reasons for answering it either affirmatively or negatively. Some might say that although *B* told the same joke as *A*, he didn't do so with the same (or perhaps any) comedic skill. On the other hand, some might claim that, since *A* and *B* uttered different strings of words, and since *J′* was not funny, *A*'s joke had *not* been repeated by *B*.

The important point to grasp here is that neither response is intrinsically better than the other. Whether we take *B*'s performance to replicate *A*'s performance depends on what's appropriate for the context in which the question arises. Suppose people are taking turns telling jokes at a party and that each person is expected to tell a different joke. If *B* were to tell his joke, we might feel justified in complaining that he didn't tell a new joke and in fact that he merely told *A*'s joke rather poorly. On the other hand, suppose the party guests are playing a different game, in which each has to memorize and repeat verbatim what his immediate predecessor says. Suppose, then, that *A* tells his joke and that *B*, whom we may suppose is mnemonically challenged, tries unsuccessfully to repeat *A*'s performance. Even if the *content* of what *A* and *B* said was similar, so that we might consider *B* to have succeeded in producing a *version* of *A*'s joke, *B*'s performance (the string of words produced in the manner produced) would not count in this context as a replication of *A*'s performance. We can imagine even more stringent requirements of replicability. Suppose *B* is studying the comedic arts, and that his task is to repeat, *not* just the same words as those of his teacher *A*, but also *A*'s inflection and timing (and note, criteria of sameness for inflection and timing are not hard and fast; for example, we needn't suppose that *A* and *B* have voices of the same quality). In this context, what *B* does will not be a recurrence or replication of what *A* does, if *B* manages to get only the words exactly right.

The moral of all this is that whether or not *B*'s verbal performance constitutes a recurrence (or replication) of *A*'s joke-telling *J* is not simply a function of formal features of what *A* and *B* do and say. In one context *B*'s sequence of words might count as a recurrence of *J*, while in another it might not.

This is simply a real-life example of a point that applies even to the most elementary examples in mathematics, which likewise demonstrate that the relation "__is similar to__" is not simply a static, two-termed relation between things, but is inevitably tied to contextual and variable criteria of relevance that are not part of an absolute inventory of Nature's furniture. As I've noted many times, this can be easily illustrated by an example from geometry, although mathematicians typically use the term "congruence" rather than "similarity" (for a more elaborate discussion of this example, see Braude, 2007, Chapter 7). In any case, mathematicians know that in the absence of some specified or agreed-upon rule of projection, or function for mapping geometric figures onto other things, no figure is congruent with (similar to) anything else. They recognize that there are different standards of congruence, appropriate for different situations. Depending on which rule of projection we choose, we may consider a given triangle to be congruent only with triangles with the same horizontal orientation and the same angles, or we may consider it to be congruent with any triangle, or even with squares or lines. So in geometry, no property intrinsic to a given triangle determines which other geometrical figures that triangle is congruent with. And that's because no situation is *intrinsically basic*; standards of relevance emerge from living and ephemeral human situations, not from Nature herself. But then no standard of congruence or similarity is inherently privileged or more fundamental than others. And clearly, if this is true even for the comparison of simple geometrical figures, it's true *a fortiori* for the comparison of much more multi-faceted joke attempts and scientific experiments.[2]

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A short but important note on a different matter. This issue contains a letter from Caroline Watt announcing the implementation of a webpage for registering parapsychological experiments. The value of this or any registry has recently been a hot topic for debate among parapsychologists, and, as far as I can tell, there's little consensus among researchers on the matter. Consequently, the *JSE* will remain neutral and allow researchers to decide for themselves whether to avail themselves of this opportunity to register their experiments. As a result, I feel it's important to note that the *JSE* will not require authors reporting parapsychological experiments to register their studies, and that registration will not be a factor in my editorial decisions.

Notes

[1] That's because (as James Spottiswoode was kind enough to remind me—personal communication) quantum mechanics "explicitly predicts that all these particles should show the same behavior. So failure to replicate across particles would have big consequences."

[2] I'm grateful to James Spottiswoode and Michael Ibison for some very helpful communications on the topic of this Editorial.

STEPHEN E. BRAUDE

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RESEARCH ARTICLE

Longitudinal Electromagnetic Waves? The Monstein–Wesley Experiment Reconstructed

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Abstract—We repeat the experiment reported in a controversial publication of Monstein and Wesley (MW), in which they claimed to have detected longitudinal electromagnetic (EM) waves in free space, a phenomenon incompatible with Maxwell's equations. While we are convinced that Maxwell's equations are valid and that longitudinal EM waves do not exist, we recognized that the radiation pattern observed in the MW experiment was itself interesting, while noting that no one had actually repeated MW's experiments. Therefore we constructed a duplicate of MW's apparatus and ran their experiments along with some additional ones. We intended both to test whether MW's results could be duplicated, and to distinguish between their theoretical model and that of a critical article published by Rębilas proposing ground plasma currents as the true cause of the waves observed by MW. We also determined the field pattern of the ball antenna experimentally. Our experimental results actually resemble MW's theoretical pattern more closely than did their own experiment, an interesting result considering that MW's theory is almost universally considered incorrect. However, our experimental results were not compatible with Rębilas' (very plausible) theoretical explanation. Thus we dispute MW's claim on theoretical grounds, and Rębilas' ground plasma currents on experimental grounds. We conclude that a yet-unidentified mechanism must be producing the observed results.

Keywords: Electromagnetic waves—Maxwell's equations—scalar waves—ball antenna—radiation pattern

Introduction and Background

We investigate a controversial publication by Monstein and Wesley (MW) (2002), in which they claimed to have detected longitudinal electromagnetic (EM) waves propagating in free space. Maxwell's equations represent the substance of classical electrodynamics: These four equations, taken together, preclude the existence of a longitudinal electric field component in a free-space wave (Bruhn 2002, Burko 2008, Kühlke 2008); thus the existence of such waves would require rewriting EM field theory. Most claims asserting the existence of such waves have been shown to rest upon obvious fallacies, poor observations, or misinterpretations of data (Meyl 2001, Bruhn 2002). MW's work has garnered attention largely due to their unique experimental design and the unusual character of their published experimental results. Articles both pro and con appeared in response to MW's, while Rębilas (2008) derived a possible alternative explanation for MW's results. None of these responders, however, built the MW apparatus and repeated the experiments.

Beginning with a sinusoidal solution to the wave equation and making some assumptions about the nature of the wave propagation, MW derive an equation for the signal intensity of the purported longitudinal EM waves as a function of distance between transmitter (Tx) and receiver (Rx):

$$S = \left(\frac{wkx}{2r_1^3 r_2^3}\right)\left[B^2 r_1^3 + A^2 r_2^3 + ABr_1 r_2 (r_1 + r_2)\cos k(r_2 - r_1) + \left(\frac{AB}{k}\right)\left(r_1^2 - r_2^2\right)\sin k(r_2 - r_1)\right] \tag{1}$$

Here A and B are wave amplitudes,

$$r_1^2 = (h_R - h_T)^2 + x^2 \text{ and } r_2^2 = (h_R + h_T)^2 + x^2$$

where h_R and h_T are the Tx and Rx heights, respectively, x is the Tx-Rx distance along the ground.

The MW experimental apparatus is described briefly as follows. Tx, powered by a 12 V battery, feeds a 433.59 MHz signal into a ball antenna (r = 30 mm). Rx consists of a similar ball antenna coupled to a field-effect transistor and voltmeter, also powered by a 12 V battery. Between Tx and Rx are positioned a pair of rotatable polarizer–analyzer arrays, intended to filter out waves polarized perpendicular to its orientation. MW claim that with both directions perpendicular to the direction of propagation thus blocked, only waves with a longitudinal electric field can pass.

Following MW's publication, a response by Bray and Britton (2004) disputed both their claims: that MW's theoretical analysis was compatible with Maxwell's equations; and that a ball antenna cannot generate a classical TEM wave. They also show that MW's prediction of the behavior of a uniform spherical charge density contradicts the continuity equation. In their response to this criticism, MW (2004) concede that their equation is not compatible with Maxwell's equations, but now assert explicitly that Maxwell's equations must be modified to admit the longitudinal waves that they claim to have detected.

Because MW's experimental results were being cited by those who have argued for the existence of free-space longitudinal waves (Van Vlaenderen 2003, 2005), Rębilas (2008) considered it important not only to document the flaws in MW's theoretical discussion, but also to explain their experimental results using classical electrodynamics. He explained the effect in terms of ground currents and plasma theory, deriving a signal strength equation of the form:

$$S(r) \propto \left[\int_0^{2\pi} \int_0^{\infty} \frac{e^{-\alpha r'} \cos\left(\beta r' + k_a \Delta r\right) \cos\phi}{\Delta r} \, \mathrm{d}r' \, \mathrm{d}\phi \right]^2 + \left[\int_0^{2\pi} \int_0^{\infty} \frac{e^{-\alpha r'} \sin\left(\beta r' + k_a \Delta r\right) \cos\phi}{\Delta r} \, \mathrm{d}r' \, \mathrm{d}\phi \right]^2 \qquad (2)$$

(we have corrected an integration order mistake in the original) where r is the vector from ground zero below Tx to the Rx ball antenna, Δr is the vector from the field point (on the ground) to the receiver, φ is the angle between the Tx-Rx vector and the Tx-field point vector, and k_a is the free-space wavenumber. He superimposed the graph obtained from this equation over MW's experimental data, to make the case that it represents that data more closely than does MW's theory.

Since no one had built a duplicate MW apparatus, we chose to do this and to conduct experiments to assess whether MW's results are reproducible, along with additional experiments that might shed more light on this effect, and also to test Rębilas' theoretical predictions. This was not a trivial task: Bray and Britton previously noted that the nature of the experiment makes it extremely difficult to control the variables against many possible forms of external interference. We reject the claim that free-space longitudinal EM waves can exist and thus that Maxwell's equations need modification; rather, we recognize that the results of the MW experiment have generated interest, and thus we have attempted to duplicate them as a step toward determining the source of the signal amplitude pattern they observed.

Methods and Materials

Following MW's description of the apparatus, we constructed the ball antennae, support stanchions, half-wave dipole antennae for comparison purposes, and a pair of polarizer–analyzers consisting of nine wires in 3 × 3 arrays, a half-wavelength long and a quarter-wave apart, one horizontal and one vertical. The horizontal polarizer can be rotated.

The ball antennae were constructed from solid 3″ diameter aluminum balls (Craig Ball Sales, Seaford, Delaware). Machining was done at the Industrial Technology machine shop at Texas A&M University. The antennae were mounted on 2 m–high stanchions (lower than MW used). The signal was transmitted at 446 MHZ (very close to MW's frequency) using a Realistic HTX-404 440 MHz Amateur UHF transceiver, and the received signal was analyzed using a Signal Hound USB-SA44B Signal Analyzer linked to a Dell Inspiron laptop computer. Power was supplied using 12 V storage tanks.

We performed a wider range of experiments than MW reported. We first tested the antennas indoors, over distances of less than 10 m, with and without the polarizer–analyzer arrays, with the ball antennas as well as half-wave dipoles in vertical and horizontal position. The transmit–receive characteristics of the ball antennae were compared with those of half-wave dipoles. We mapped the radiation pattern of the ball antenna at close range as a function of angle from the apex of the ball.

Next we conducted full-scale tests outdoors, increasing the Tx-Rx distance in 2 m increments at smaller distances, then in larger increments at greater distances. Available space limited our Tx-Rx separation to a maximum of 90 m. We also measured signal strength as a function of angle, with the Tx ball antenna fixed in location and orientation while the Rx ball antenna was moved to positions around it, always with the apex of the Rx antenna pointed toward the Tx antenna. Again this test was performed both with and without the polarizers in place. To provide an additional comparison, we set up the apparatus in an indoor corridor 30 m in length, and took readings every 2 m, both with and without the polarizers in place.

Results

We wrote a program to compute signal as a function of Tx-Rx distance from MW's signal equation (Equation 1 above), and ran it first with their input values, then again with our own input values. In this way we reproduced their graph of signal strength as a function of Tx-Rx distance over the distance of

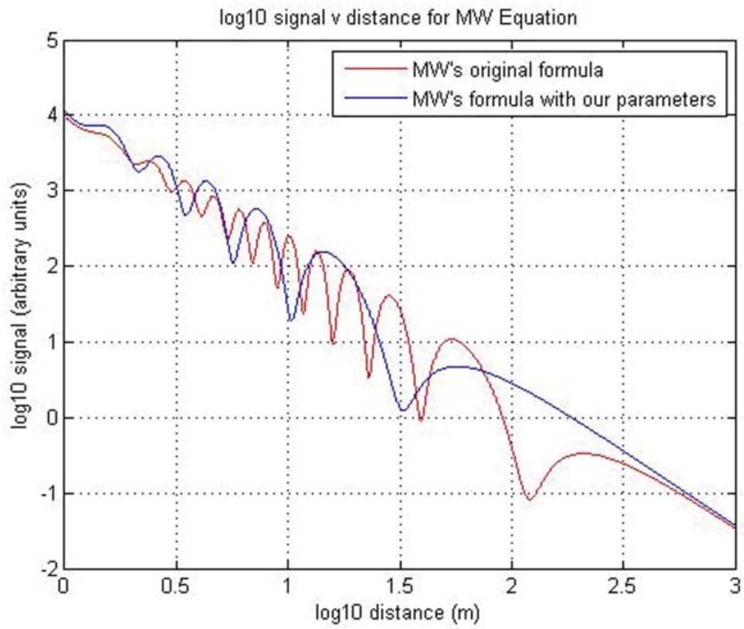
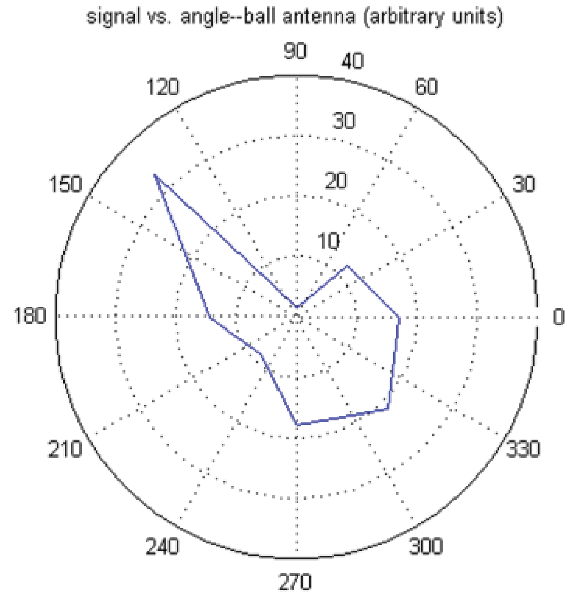


Figure 1. The graphs were generated numerically using MW's signal equation, using MW's parameters (red), and adjusted for our actual input values (blue). The only changes were a reduced height for the transmitter and receiver, and a slightly higher frequency. We extended the study down to a minimum separation of 1 m, whereas MW used 10 m.

10–1000 m, but also extended the range down to 1–10 m to reveal additional minima, shallower and closer together. With our parameters, we generated another graph that is superimposed on MW's graph in Figure 1.

We did not observe the effects MW reported for the polarizer–analyzer arrays. Rotating the array did not produce the power null at a deflection angle of 0° that they show in their Fig. 3. Indeed, the presence or absence of the polarizers had only a small effect on signal intensity as a function of distance. We conclude that they were not in fact polarizing the EM waves during the ball antenna experiments. They did, however, appear to function as polarizers when we used simple half-wave dipole antennae instead of the ball antennae. We further tested the effect of the polarizers by measuring signal intensity as a function of angle, with and without the polarizers in place, outdoors, with the zero angle representing the front face of the Tx antenna. In both cases there appears a peak intensity separated from a null by an angle of 45°. Without the polarizers, the peak intensity appeared at



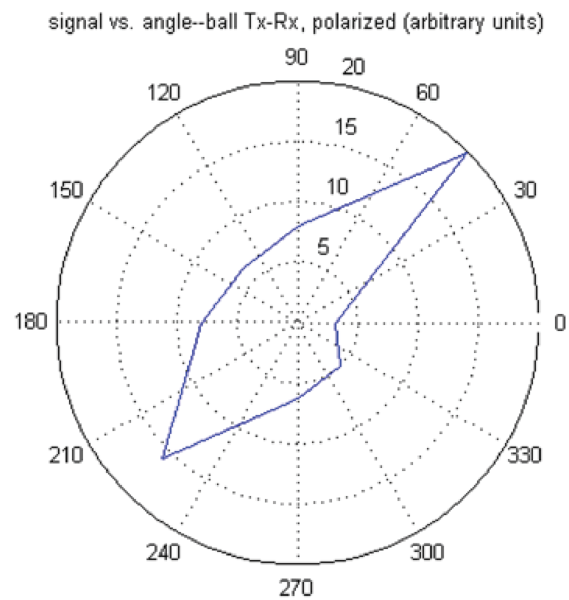


Figure 2. Polar plots of the signal strength when ball antennae are used for both transmit and receive functions, both without (above) and with (below) the polarizers in place. The angle of zero represents the frontal face of the Tx ball antenna. The main effect of the polarizers was to shift the angle at which the signal is strongest by π/2.

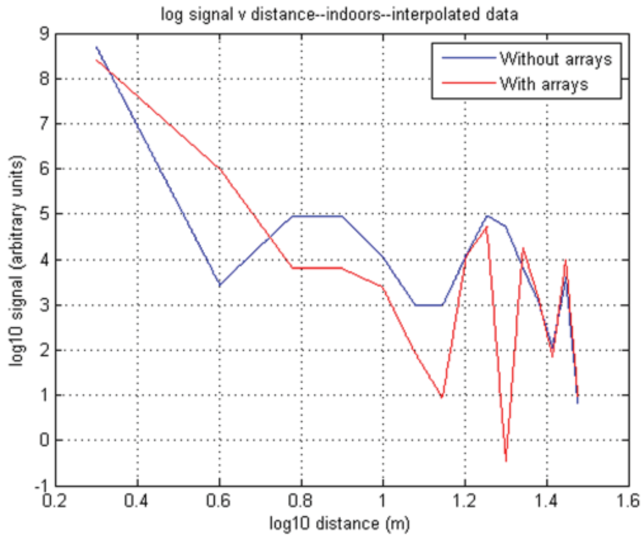


Figure 3. Results of an experiment conducted indoors, in an east–west oriented hallway of length 30 m. We present the signal intensity recorded both without the polarizer-analyzer arrays (blue), and with them in place (red). With a few exceptions, most notably the data point at a distance of 20 m, the shapes of the curves are similar.

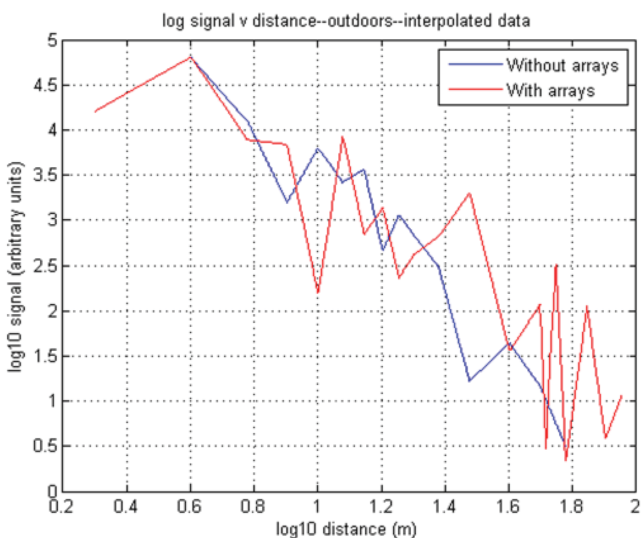


Figure 4. Data recorded outdoors with the transmitter and receiver in an east–west orientation over separations up to 90 m, again without the polarizer–analyzer arrays (blue), and with them in place (red). Some additional data points were obtained in the second case, and we believe the behavior of the signal around the 60 m separation was affected by external interference but have not been able to identify the source. Again, there is a general similarity in the shapes of the curves, with a few exceptions. A few additional data points were obtained with the arrays in place.

an angle of 135°; with them, it appears at 45°. Thus the polarizers rotate the positions of the peak intensity and the null by an angle of 90° in the clockwise direction (Figure 2).

The most interesting measured quantity is the position and depth of the signal minima as functions of transmit–receive distances. We show these in Figure 3 for the indoor study; and in Figure 4 for the outdoor study.

For the various theoretical and experimental plots, the locations of these signal minima are as follows:

MW model, their values (read from their graph, to the nearest meter, 10–1000 m): 12, 16, 23, 40, 120 m
MW model, their parameters (from our numerical representation, to the nearest tenth of a meter, 1–1000 m): 2.1, 3.0, 4.0, 5.0, 6.8, 8.9, 11.8, 15.8, 22.8, 39.4, 120.7 m (Note that the last five of these values correspond very closely to MW's, as expected.)
MW model, our parameters (from our numerical representation, 1–1000 m): 2.2, 3.6, 5.6, 10.4, 32.9 m
MW experimental values (read from their graph, 10–1000 m): 24, 30, 40 m
Rębilas' model (read from his graph, 10–1000 m): 25, 34, 43, 53, 61, 70.5, 81, 87 m
Indoor experiment, without polarizers (1–30 m): 4.0, 12.6, 26.3, 29.8 m
Indoor experiment, with polarizers (1–30 m): 5.9, 13.5, 19.5, 26.3, 29.8 m
Outdoor experiment, without polarizers (1–90 m): 7.9, 15.8, 29.5, 60.3 m
Outdoor experiment, with polarizers (1–90 m): 10.0, 17.8, 39.8, 60.3 m

Discussion

We agree with Bray and Britton that the nature of this experiment makes it impossible to control all variables, so we compensated by running experiments under a broader set of conditions than did MW: in different locations, with the antennae positioned in different orientations, outdoors and indoors, in order to identify some effects that might occur due to interference in a particular situation. Nevertheless, the present results are reported as preliminary: More complex and elaborate experiments are possible with this apparatus.

Here we compare MW's theoretical graph, their experimental data, Rębilas' theoretical graph, and our experimental data. When we modeled MW's equation, we used a minimum Tx-Rx separation of 1 m rather than 10 m, and we tested the small-separation behavior experimentally by taking measurements down to a minimum separation of 2 m. We did not observe the close (<8 m) minima predicted by MW's formula with our parameters (at least in the outdoor experiments). However, those minima are not as deep as the more distant ones in MW's simulation, and we may not have had the sensitivity to detect them.

MW's experimental data do not show their predicted minima at 12 and 16 m, nor the deep minimum predicted at 120 m or any distinct minima after 40 m, but rather a long tail-off that roughly approximates an inverse-square relation until about 200 m which then falls off more rapidly. Rębilas claims that MW's graph and their experiment are not a good match, and his graph correctly predicts MW's observed first null at 24 m. However, our observation of his graphs does not accord with his claim to have predicted the double minima at 32 and 39 m. He also predicts a series of unobserved smaller minima up to 200 m. Since Rębilas did not provide the actual parameters used in his calculation, we were unable to replicate his graph computationally and instead used his published graph.

The most pronounced difference between MW's and Rębilas' equations is that the former predicts deeper minima at increasing separations with increased Tx-Rx distance, while the latter predicts shallower minima at nearly equal separations with increased distance. It is possible that Rębilas' smaller predicted minima of about 100 m may have been under MW's detection threshold, but our experimental results suggest rather that successive minima do in fact become progressively deeper and farther apart with increasing Tx-Rx distance. This was observed indoors and outdoors, with or without polarizers in place. This result accords at least roughly with MW's predictions, but is incompatible with Rębilas' simulation, and we do not see his theory as providing a better match either to MW's data or to ours. Although his explanation is theoretically plausible, we conclude that his proposed ground plasma currents were not a major contributor to the signal that we observed.

Although our experiments show some general similarities to MW's calculations and experiments, the detailed patterns of minima are quite different. Most likely, the difference between the indoor and outdoor runs is due largely to environmental factors (possible presence of conducting materials, etc.). As explained above, the "polarizer–analyzers" did not function as such in any experiment in which the ball antennae were used for Tx and Rx: They did not null the signal in any orientation. Rather, their effect seemed comparable to the indoor–outdoor differences: They changed the shape of the response curve, and shifted the positions and depths of the minima somewhat. This result suggests that a process other than that described by MW was at work here. In both the indoor and outdoor experiments, a deep signal minimum at a distance of 19.5 m (indoors) and 39.8 m (outdoors) was observed only when the polarizers were used. We cannot explain this minimum at present.

Conclusion

Classical electrodynamics as formulated in Maxwell's equations does not admit longitudinal EM radiation propagating in free space. In agreement with Bruhn, Bray and Britton, and Burko, our reading of MW's theory suggests that it is internally flawed and that it provides no compelling rationale for questioning the foundations of classical electrodynamics. Nevertheless, MW's experiment was of a clever design, and amid speculation as to the true cause of their observed results, it befit us to build the apparatus and conduct their experiment ourselves, along with additional experiments that could further illuminate the subject. While the experiment is difficult to control and we observed evidence of environmental interference, one pattern emerged consistently: The observed signal minima become deeper and farther apart with increasing Tx-Rx distance.

Modeling MW's equation with our input data generates fewer minima, although they still follow the pattern of increasing depth and separation with increasing distance. Because we used a frequency close to MW's, we expect that the lower height of the antennae was a significant factor in this difference. Although we reject MW's theoretical explanation, we note that that their equation does predict the important common feature of minima with increasing depth and separation with increasing Tx-Rx distance. Meanwhile, the effects we observed were completely incompatible with Rębilas' simulation. While his theory of ground plasma currents contains no scientific mistakes and is certainly plausible, we must conclude that it cannot be a major contributor to the observed signal.

Because our principal purpose was to build the apparatus, perform the experiments, and compare our results to MW's theory and experiment as well as to Rębilas' explanation, we did not attempt to develop a theoretical model for the signal. Their theory, however, must still be addressed. Concerning the theory of the ball antenna, MW write that

> The spherically symmetric current density **J** within the ball, that gives rise to the pulsating surface charge, is divergenceless, $\nabla \cdot \mathbf{J} = 0$; so $\nabla \cdot \mathbf{A} = 0$ and $\nabla \times \mathbf{A} = 0$; and no transverse wave can arise.

Bray and Britton note that such a divergenceless source contradicts the continuity equation $\nabla \cdot \mathbf{J} = -\frac{\partial \rho}{\partial t}$ since it would require that the oscillating charge density be zero. Hence if $\nabla \cdot \mathbf{J} = 0$ at the source, no EM wave can arise. Since the ball antenna is clearly emitting EM radiation, we conclude that a very different process must give rise to these waves. We are working to derive a theoretical model for the field pattern of the ball antenna for future publication. It is our expectation that such a model will depend very sensitively on the point within the ball antenna at which it is actually driven.

Some methodological concerns remain as well. MW performed no statistical analysis either on the data acquisition itself or on the comparison between the acquired data and the simulations. Instead they simply "eyeballed" the results, and for the present we have done the same. While this is in part understandable due to the nature of the experiment and the difficulty involved in controlling the environment, an appropriate statistical analysis might provide additional insight into the results. We are currently looking into the possibility of developing appropriate statistical methods both for analyzing the data and for quantifying the comparison between data and theory.

We are planning to conduct more experiments with this apparatus, and we invite collaboration from others interested in this issue.

Acknowledgments

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RESEARCH ARTICLE

The UFO Abduction Syndrome

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Budd Hopkins died in August 2011, while this manuscript was in preparation. Davis and Hopkins developed the test described in this paper and Donderi collaborated with them to evaluate it. Portions of this work were presented as a poster in 2007 (Donderi, Hopkins, & Davis 2007).

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Abstract—Some people say that they have been abducted by extraterrestrials. We obtained responses to 608 true–false questions from 52 self-reported abductees and compared their responses to those of 75 non-abductee controls and to 26 simulators whom we asked to respond "as if" they had been abducted. The entire question set, as well as a subset of 65 questions identified by discriminant analysis, differentiated among self-reported abductees, controls, and simulators. This result helps to define a state of mind that we call the UFO Abduction Syndrome.

Introduction

A nationwide survey led to the conclusion that perhaps two percent of Americans had experienced what the survey sponsors called the "UFO Abduction Syndrome" (Hopkins, Jacobs, & Westrum, 1992). The conclusion was based on "true" responses to four of five questions that Hopkins and abduction researchers David Jacobs and Ron Westrum believed were positive indicators of that experience, and a "false" response to a single question intended to eliminate yea-sayers or "wannabes." The Roper Poll tested almost six thousand households with these questions during three stratified random sampling surveys completed in 1991. The six abduction-related questions were integrated with other questions on lifestyle, political opinions, and the like, so the indicator questions did not stand out as a separate category, and UFO abductions were not mentioned by the pollsters. The startling conclusion from this poll motivated Hopkins and Davis to develop the more comprehensive—and more intensive—screening test discussed here. The test distinguishes the mental state of people reporting that they were abducted by aliens from the mental state of people not reporting an abduction, and also from the mental state of people taking the test "as if" they had been abducted by aliens. The test, called the American Personality Inventory, may help to better define the UFO Abduction Syndrome.

The UFO abduction syndrome has been evaluated as real by Hopkins (1996, 1987, 1981) and Jacobs (2000, 1992, 1998), among others. It has also been explained as the reinstatement of birth trauma (Lawson, 1988), as sadomasochistic fantasy (Newman & Baumeister 1996), and as fantasy-proneness leading to a failure to distinguish between imagination and reality (Clancy, 2005). It has also been a theme of film and TV fiction (*The X-Files, Taken)*.

Ex Post Facto Reasoning

We cannot know whether any measured difference between alien abduction reporters and controls *caused* the reported abduction experience or whether the measured differences *were caused by* the reported experience. No research, including ours, answers—or can answer—this question. But in order to provide a context for our work, we start by reviewing some of the more substantial research on personality aspects of self-reported abductees[1] that may be relevant to understanding our own results.

Bloecher, Clamar, and Hopkins (1985) obtained the cooperation of five male and four female abductees, each of whom was asked to not mention or discuss the abduction experience during the interviews and tests carried out by clinical psychologist Elisabeth Slater, who was led to believe that she was participating in a study on "creativity."

She administered the Wechsler Adult Intelligence Scale (WAIS), the Thematic Apperception Test (TAT), the Rorschach Test, and the Minnesota Multiphasic Personality Inventory (MMPI) to each of the nine participants. The WAIS is a widely used test that measures general knowledge and cognitive ability. The TAT and Rorschach tests are "projective tests" which require the testee to describe in his or her own words what is seen in the series of Rorschach "ink blots" and what he or she experiences while looking at the generally more realistic images of the TAT. The MMPI is a 567-item true–false test whose answers are used to construct a psychological profile of each respondent on a series of scales that reflect potential sources of personality disturbance. All of the participants had high average or above-average intelligence as measured by the WAIS. None demonstrated psychopathology as demonstrated by the MMPI. All nine had been to college and three had been to graduate school, and they were employed in occupations that ranged from secretary to college instructor, to corporation lawyer, to director of a chemical laboratory.

The interviews, TAT, and Rorschach test results led Slater to describe the nine people as "distinctive, unusual, and interesting subjects . . . including some who were downright 'eccentric' or 'odd.'" Slater summarized her conclusions as follows:

> In sum, the formal test results support the earlier stated clinical impression that one has a group of unusual and interesting personalities characterized by relatively high intellectual ability and richly evocative and charged inner worlds. At their best they are highly inventive, creative, and original. At their worst, they are beset by intense emotional upheaval . . . Another factor common to the nine subjects in terms of emotional functioning is a modicum of what is technically termed narcissistic disturbance. It is manifest along at least three dimensions: identity disturbance, lowered self-esteem, relative egocentricity and/or lack of emotional maturity . . . It may also be felt very concretely in terms of impaired body image and/or somatic concerns about one's bodily integrity. (Slater 1983:21–22)

After being told about the experience common to her nine subjects, Slater wrote,

> The first and most critical question is whether our subjects' reported experiences could be accounted for strictly on the basis of psychopathology, i.e. mental disorder. <u>The answer is a firm no</u>. In broad terms, if the reported abductions were confabulated fantasy productions, based on what we know about psychological disorders, they could only come from pathological liars, paranoid schizophrenics, and severely disturbed and extraordinarily rare hysteroid characters subject to fugue states and/or multiple personality. . . <u>not one</u> of the subjects, based on test data, falls into any of these categories . . . In other words, there is no apparent psychological explanation for their reports. (Slater 1983: 33–34)

Slater's report is the first and the most thorough systematic evaluation of the mental state of people reporting being abducted by aliens.

Ring and Rosing (1990) analyzed results from a mail survey of 264 people solicited (with about a fifty percent response rate) from two communities of interest. One hundred thirty-six respondents had either reported a UFO experience or were simply interested in UFOs. They were drawn from mailing lists provided by four different UFO researchers. One hundred twenty-eight respondents had either reported a near-death experience (NDE) or were simply interested in NDEs. They were from Ring's mailing lists or from those of the International Association for Near-Death Studies. Fifty-eight percent of the respondents were women. They completed nine questionnaires which, in addition to basic demographic information, queried experiences and interests, childhood experiences, home environments, tendencies toward psychological dissociation, awareness of paranormal phenomena, life changes, religious beliefs, and opinions about the import of UFOs and NDEs.

The UFO experiencer and the NDE experiencer groups both reported more childhood abuse and trauma than did either of the two groups that were just interested in UFOs or NDEs. Although all the groups were about equal on the Ring and Rosing measures of fantasy-proneness, the UFO experiencers reported more awareness than the other groups of what Ring and Rosing describe as "alternate realities."

Parnell and Sprinkle (1990) reported data collected over an 18-year period from 225 respondents (37% male) who wrote to Sprinkle, a psychologist, about UFOs and had subsequently completed a mail survey. Each respondent completed the 16PF personality index—another personality questionnaire—and the MMPI, and they also described their UFO experience. The respondents were divided into five groups based on whether they reported

1) interest in UFOs but no experience
2) a sighting of a UFO as a "light or object in the sky"
3) a sighting of what appeared to be a spacecraft
4) a sighting of a UFO occupant
5) an abduction

The respondents were also classified based on whether they reported communication between themselves and an extra-terrestrial. (It follows, but was not stated in the report, that most of these people would have been in groups 3 to 5.) Neither the average MMPI scores nor the average 16PF scores showed evidence of psychopathology. Nor were there score profile differences across the five sighting groups.

Rodeghier, Goodpaster, and Blatterbauer (1991) studied 27 people who reported having been taken against their will from normal surroundings by non-human beings, taken to a structure that was assumed to be a spacecraft, and questioned by the occupants either vocally or telepathically. These people responded to a mail questionnaire by completing the Inventory of

Childhood Memories and Imaginings (IMCI) (Barber & Wilson 1982), which is a test that measures fantasy-proneness. They also took the MMPI, the Creative Imagination Scale (CIS) (Wilson & Barber 1978), and a questionnaire that recorded demographics and information about a variety of experiences. The data were augmented by the results of eight more abductees who completed only the IMCI. The average fantasy-proneness score over all 35 respondents was within the normal population range. Two respondents had elevated scores, which is the same proportion found in the normal population.

Personality scale scores from all 19 subjects completing the MMPI were largely within the normal range, but further analysis of the test scores divided the respondents into two distinct groups. One group of 11 people was normal on all scales. A second group of eight people had higher than average scores on seven of the nine MMPI personality scales and a lower than average score on another. This second group also reported a much higher frequency of childhood sexual abuse.

Spanos, Cross, Dickson, and DuBreuil (1993) studied 176 people invited to his laboratory by ads in local papers. One ad invited people "who have seen U.F.O.s" to contact the researcher. Another ad, as well as a classroom recruitment, sought volunteers "for a personality study" (Spanos et al. 1993:625). Spanos et al. compared four groups:

1) 31 people who experienced something like "a craft seen close up" or "missing time"
2) 18 people who saw "lights or objects in the sky that appear to be unusual"
3) 53 people, recruited through a newspaper ad, with no UFO experiences
4) 74 undergraduates with no UFO experience, who received course credit for being tested

Everyone was given 20 different tests including questionnaires about UFO beliefs and paranormal experiences, the MMPI schizophrenia scale, short IQ measures, and other personality measures that included assessments of fantasy-proneness. The two groups with UFO experiences were not significantly different from the other two groups on any of the tests of mental health or emotional stability. Spanos et al. write that "these findings provide no support whatsoever for the hypothesis that UFO reporters are psychologically disturbed." The groups did differ on intelligence. The "non-intensive" UFO group scored higher than all the other groups, and the student control group scored higher than the newspaper-recruited control group. Spanos et al. point out that the UFO groups did differ from the two control groups in one characteristic: “The finding that most clearly differentiated the UFO groups from the comparison groups was the belief in UFOs and in the existence of alien life forms” (Spanos et al. 1993:629). A likely result of observing what you think is an extraterrestrial spaceship or an extraterrestrial life form would be to decide that it is real.

Clancy, McNally, Schacter, Lenzenweger, and Pitman (2002:456) also recruited people to their study by advertising in newspapers, first for “people who may have been contacted or abducted by space aliens” and then for “people to participate in a memory study.” Eleven people reported conscious memories of an alien abduction following waking up paralyzed at night. Nine people thought they might have been abducted but did not consciously remember it. Thirteen people who did not claim to have been abducted were the control group. Everyone completed measures designed to assess post-traumatic stress disorder (PTSD), depression, dissociative experiences, hypnotic susceptibility, and schizotypal aspects of personality. All of them were then tested on a version of a test for “false memory” (the Deese/Roediger–McDermott paradigm, Roediger & McDermott 1995). This test presents participants with spoken lists of words having a common theme (e.g., candy, sugar, taste, nice) followed first by a recall test (“say the words that you heard before”) and then by a recognition test presented as a list (“check off the words on this list that you heard before”). The checkoff list includes a semantically similar word (e.g. sweet) that was not spoken. The false memory error is to respond by including the semantically similar word in spoken recall or to check it off on the word-recognition list.

The 20 people who said that they had been abducted by aliens made more false memory mistakes than did the controls. The group that consciously recalled an abduction made more false memory mistakes than the abduction group without conscious recall. The two abduction groups had higher schizotypical personality measures than the controls. In addition, whether you were an abductee or a control, a higher score on the absorption scale, the Beck Depression Inventory, the Magical Ideation scale, the PTSD measure, and the dissociative experiences scale all predicted more false recall (and to a lesser extent, more false recognition) on the memory test.

McNally, Lasko, Clancy, Macklin, Pitman, and Orr (2004) recruited six women and four men (average age 48) reporting having been abducted by aliens. All of them had close to clinical levels of PTSD. All of them reported sleep paralysis that they associated with the presence of aliens. Twelve controls from the community (average age 50) were not assessed for PTSD, but the abductee sample was much higher on scales of absorption and trait anxiety than the control sample.

The experimenters prepared two short narratives recapitulating experiences that each of the abductees had reported to the researchers. Three other scripts were prepared: a non-abduction but stressful script, an emotionally positive script, and an emotionally neutral script. Each subject was instrumented to record heart rate, skin conductance, and EMG. Then each abduction subject and a matched control listened to the five scripts provided for that abductee. The subjects were given relaxation instructions and then were asked to visualize the script as they listened to it, were asked to imagine each script after having heard it, and then were instructed to relax before hearing the next script. The physiological measures collected during each script and the questionnaire responses collected after each script showed that the abductees reacted with much greater emotional stress than did the linked control subjects not only to the abduction scripts, which were based on their own experiences, but also to the stress-inducing but non-abduction-related scripts. Their physiological responses were on a par with those of PTSD patients' response to scripts describing their own trauma.

Hough and Rogers (2007) posted notices on UFO/abduction websites and called people they knew to have reported an abduction in order to obtain a sample of 26 abduction reporters. They also obtained 26 control subjects who did not report abductions, from the English cities of Preston and Liverpool. Each group contained 20 women and 6 men. The groups were similar demographically except that the abductees had slightly less formal education. Each person completed four self-report measures at home and returned the results by mail. The measures were:

1) an alien abduction experience scale, summing the number of abduction experiences of each person
2) a fantasy-proneness scale called the Creative Experiences Questionnaire
3) the Self-report emotional intelligence test
4) the Ten-Item personality inventory, a short test that locates each respondent on the "Big Five" personality dimensions of extraversion, agreeableness, conscientiousness, emotional stability, and openness to experience.

Except for the expected differences on the scale measuring abduction experiences, there were no significant differences between the two samples on the measures of fantasy-proneness, emotional intelligence, or overall personality profile.

Summary of the Studies

Slater's nine abductees, the eleven MMPI–normal abductees of Rodeghier, Goodpaster, and Blatterbauer, Spanos et al.'s 31 people who had "seen a craft close up" or who had experienced "missing time," and Hough and Rogers' 26 abductees were all within the normal range on the tests used to assess them, including tests of intelligence, the MMPI, other general personality measures, and tests of fantasy-proneness. These 77 "normal" people reported 77 very abnormal experiences. Parnell and Sprinkle's sample of 225 people included people who reported seeing a craft "close up," or an occupant, or an abduction, and their sample group averaged within the normal range on the MMPI.

Other abductees were less psychologically normal. Eight of Rodeghier et al.'s abductees had MMPI scores well beyond the normal range on eight of nine scales, and they also reported experiences of childhood sexual abuse. Ring and Rosing's UFO experiencers reported more childhood sexual abuse than the non-experiencers in their sample. Clancy et al.'s 13 abductees scored higher than normal on a scale of schizotypy, and also produced more "false positives" in a memory test than did controls. And ten other abductees in the McNally et al. study tested at barely sub-clinical levels of PTSD and responded with extreme physiological measures when listening to accounts of their own abduction narratives.

The meaning of "abductee" varies over these studies from the one extreme of someone independently contacting an abduction researcher to the other extreme of having answered a newspaper or website advertisement for research subjects, with a minimum of explanation. This adds to the uncertainty of who—as well as what—is being measured. The purpose of our study was to evaluate a new test that might be able to distinguish among three groups of people. The first group is people claiming to have been abducted by aliens and who were subsequently interviewed and studied by abduction researchers. The second group is people not claiming to have been abducted by aliens and who were recruited to take the test thinking that the researchers were collecting normalizing data for a new personality inventory. No mention was made to them about UFOs, aliens, or alien abductions before they took the test. The third group were "simulators" who were primed with leading questions about UFOs and alien abductions and then asked to pretend, based on their cultural knowledge of the abduction phenomenon, to have been abducted and to answer the questions on the test "as if" they had experienced an alien abduction.

Method

The American Personality Inventory

The American Personality Inventory (API) consists of 608 true–false questions relating to attitudes and emotions that we thought might be modified by experiencing an alien abduction. Abduction researchers assume that not all abduction experiences are consciously recalled, so no API question actually mentions an abduction experience. Instead, the questions were designed to define and measure an emotional and cognitive profile that characterizes someone who had experienced an abduction, whether or not it was consciously remembered. The test was constructed by Davis and Hopkins on the model of the well-known Minnesota Multiphasic Personality Inventory (MMPI). Its name was chosen to be unrelated to the UFO or abduction phenomena.

The questions were organized into 23 non-independent scales. Each scale included statements whose answer would contribute to defining a characteristic mental attitude that might be produced by an abduction experience. Table 1 includes the scale titles, the total number of questions contributing to each scale, and a typical scale question. Answering each question in the direction (true or false) that contributed to the scale increases the scale score by 1; answering the question in the other direction decreases the score by 1. The normalized scale score was the sum over questions divided by the number of questions, making the maximum score +1 and the minimum, –1. The pencil-and-paper version of the API consisted of an 18-page question booklet and a five-page answer sheet where a T or F was circled to respond to each question. The computer-presented test prompted the user to click a button to display each question in order. It then recorded both the answer (T or F) to the question and the elapsed time in seconds from when the question was presented to when it was answered. No use was made of the elapsed time information in this study.

Participants

Fifty-two abductees (26 men and 26 women) were recruited among people who had reported abductions to Hopkins and Davis or to several other abduction researchers. Twenty abductees recalled their abduction experience spontaneously without hypnosis while 32 had undergone one or more regressive hypnosis sessions before the API was administered. Twenty-two non-abductee controls were recruited by Hopkins and Davis from the New York City area and 53 controls were recruited by Donderi and his students from the Montréal area. All the controls said (after their test was completed) that they had never experienced a UFO abduction.

TABLE 1
Scales of the American Personality Inventory (API) with the Number of Questions in the Scale, a Typical Scale Question, and the Score (T or F) That Adds to the Abductee Profile

Scale	N	Typical Question	Score
Fear	49	I am more afraid of the dark than anyone my age should be.	(T)
Animal	18	People who talk to animals instead of other people are annoying.	(F)
Fake	18	Simple medical procedures always make me anxious.	(T)
Medical	29	All doctors lie to you.	(T)
Wannabe	23	I find it almost impossible to fly in an airplane.	(T)
Anomalies	62	I have seen an unusual fog or haze in my home.	(T)
Wrong	108	If my employer were to know everything about me, I would immediately lose my job.	(T)
Sleep	37	I avoid sleep until I can no longer function without it.	(T)
Sexual	35	I find talking about sex to be enjoyable.	(T)
Dreams	66	In my dreams I often picture my own bedroom.	(T)
Break-in	18	I am very afraid of someone breaking into my house at night.	(T)
Missing	19	Someone in my life has witnessed my being unexplainably missing for a period of time.	(T)
Environment	21	The oceans aren't as polluted as we have been led to believe.	(F)
Helplessness	20	If you just do your best everything usually works out in the end.	(F)
Babies	27	People who don't like to hold babies are strange.	(F)
Insect	17	I am no more afraid of insects than other people are.	(F)
Water	16	The problem with swimming in groups is that you can't trust the others you are with.	(T)
Clowns	18	One of the best parts of the circus is seeing the funny shows put on by the clowns.	(F)
Eyes	11	I have trouble making eye contact with others.	(T)
Light	6	A white room lit by a bright unseen light source would be relaxing.	(F)
Child	42	As a child I often experienced great sadness for no particular reason.	(T)
Playmate	14	When I was a child I never had an imaginary friend.	(F)
Poison	8	I have a dim memory of once being nearly poisoned.	(T)

Twenty-six simulators were recruited by Donderi and his students from the Montréal area. The simulators also said that they had never experienced an alien abduction. The age and gender distribution of each group is reported in Table 2.[2]

TABLE 2
Participant Age, Gender, and Scores on the American Personality Inventory (API)

Group	Participants		Age			API scores	
	Men	**Women**	**Average**	**Range**	***SD***	**Average**	***SD***
Abductees	25	27	43	21–60	9.8	–0.11	0.23
Controls	34	41	30	19–54	14.3	–0.54	0.11
Simulators	5	21	30	19–69	12.4	0.12	0.50

The API was administered to the 52 abductees by several abduction researchers.[3] The control subjects were told only that we were collecting normative data for a new personality inventory. The simulator subjects completed two short questionnaires before they completed the API. The first questionnaire, called the Media Exposure Questionnaire, included a long list of UFO and abduction-related books, films, and TV series. We asked the simulators to place a check by all of them that they had read or seen, and to add any that we had missed on blank lines at the bottom of the questionnaire. The second questionnaire, called the Unusual Personal Experiences Questionnaire, actually consisted of the questions from the Roper Poll survey described earlier (Hopkins, Jacobs, & Westrum, 1992). It asked about the participants' own experiences (had you seen a UFO, had you experienced "missing time", etc.—none of the simulators reported any unusual personal experiences). After the simulators had completed those questionnaires, they were asked to use their own knowledge gained through media exposure to answer the API questions "as if" they had been abducted.

Administering the API

The API was administered as a paper-and-pencil test to the 52 abductees and 22 non-abductee controls from New York and to 19 of the 53 controls tested in Montréal. It was presented on a computer to 34 of the Montréal controls and to all 26 of the simulators tested in Montréal. Either version of the test took between 45 minutes and one hour to administer. The results were tabulated and analyzed by Donderi.

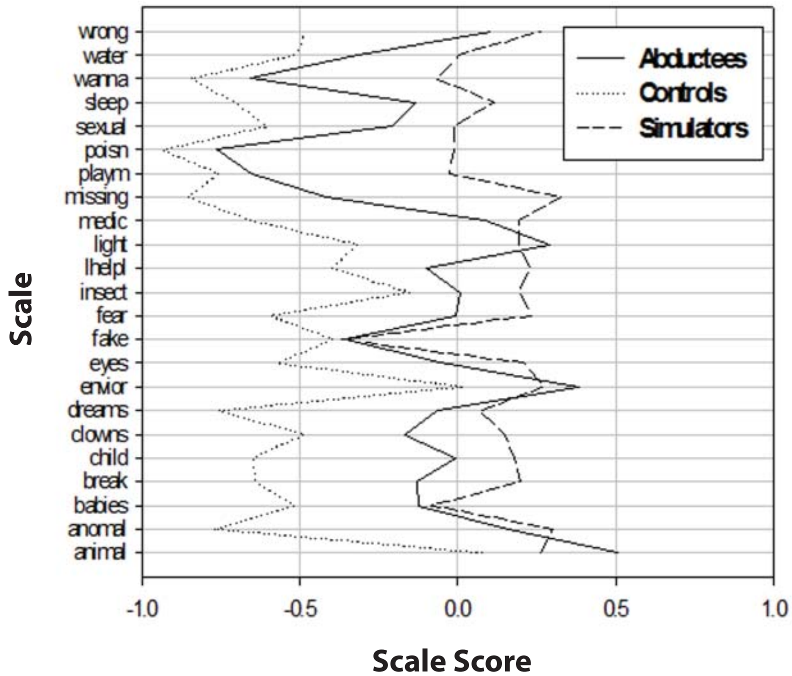

Figure 1. Scale Scores on the American Personality Inventory for the abductee, simulator, and control groups.

Results

The simulators' mean scale score was higher than the abductees' mean scale score on 21 of the 23 API scales. The abductees were higher than the simulators on the other two scales. The control participants scored lower than either the abductees or the simulators on all of the scales (Figure 1, Table 3). The answer (T or F) to each question was scored as conforming to (+1) or deviant from (−1) the expected abduction profile. The average score for each participant across all questions ranged from a maximum possible +1 to a minimum possible −1. Each participant's average score across all 608 questions was treated as the independent variable in an analysis of variance (SAS general linear model) that compared the mean test score across groups using the most conservative tests (type III sums of squares) available in the model. There were significant differences between the participant groups, and planned comparisons showed that the mean of each

TABLE 3
American Personality Inventory Scale Scores for the Abductee, Control, and Simulator Groups

Scale	Abductees (A)		Controls (C)		Simulators (S)		Order
	Mean	*SD*	Mean	*SD*	Mean	*SD*	
Animal	0.51	0.40	0.08	0.36	0.26	0.49	ASC
Anomalies	0.16	0.47	−0.77	0.15	0.30	0.66	SAC
Babies	−0.12	0.44	−0.52	0.27	−0.09	0.62	SAC
Break	−0.13	0.49	−0.64	0.31	0.20	0.71	SAC
Child	0.00	0.36	−0.65	0.17	0.18	0.54	SAC
Clowns	−0.17	0.51	−0.49	0.32	0.15	0.63	SAC
Dreams	−0.06	0.40	−0.76	0.15	0.07	0.59	SAC
Environment	0.39	0.37	0.01	0.29	0.27	0.42	SAC
Eyes	−0.06	0.50	−0.57	0.24	0.21	0.59	SAC
Fake	−0.36	0.26	−0.40	0.30	−0.37	0.35	ASC
Fear	−0.01	0.38	−0.59	0.22	0.23	0.59	SAC
Insect	0.01	0.31	−0.15	0.29	0.19	0.37	SAC
Helpless	−0.10	0.40	−0.40	0.30	0.23	0.63	SAC
Light	0.29	0.41	−0.32	0.24	0.19	0.48	ASC
Medical	0.09	0.54	−0.65	0.28	0.20	0.68	SAC
Missing	−0.42	0.56	−0.86	0.21	0.33	0.75	SAC
Playmate	−0.65	0.36	−0.76	0.25	−0.03	0.80	SAC
Poison	−0.76	0.39	−0.94	0.15	−0.01	0.89	SAC
Sexual	−0.21	0.47	−0.60	0.27	−0.01	0.69	SAC
Sleep	−0.13	0.41	−0.71	0.18	0.12	0.68	SAC
Wannabe	−0.66	0.28	−0.85	0.16	−0.07	0.65	SAC
Water	−0.31	0.47	−0.51	0.32	0.00	0.62	SAC
Wrong	0.11	0.38	−0.48	0.24	0.27	0.54	SAC

group was significantly different from the means of the other groups (Table 4). A *t*-test comparison between the mean scores of those abductees who had experienced hypnotic regression before taking the API (Mean = −0.09, *SD* = .23) and those who recalled their abduction experience spontaneously and took the API without previous hypnotic regression (Mean = −0.15, *SD* = .24) showed that there was no significant difference between the two groups of abductees ($t = 0.94$, $df = 40$, ns).

TABLE 4
Analysis of Variance: Total Score on the American Personality Inventory

	Mean Square	*F*	*p*
Between groups	5.449	82.33	<.0001
Within groups	0.066		
Planned comparisons among groups			
Abductees versus controls	5.696	86.07	<.0001
Abductees versus simulators	14.65	14.65	0.0002
Controls versus simulators	8.596	129.87	<.0001

Discriminant Analysis

Using stepwise discriminant analysis as a statistical tool, we sought to find a subset of questions that would distinguish among the three groups. We found two linear discriminant functions, based on a subset of 65 questions, that discriminated perfectly among the 153 subjects across the three groups. The values assigned to each subject on the two canonical discriminant variables are shown in Figure 2. The 65 computer-presented discriminant analysis questions (along with an additional 15 "fillers" added to lengthen the test, but not included in the scoring) can be administered in less than one-half hour. Since the API is intended as a screening test, the short form has practical advantages.[4]

Discussion and Conclusions

Our study shows that the API separates people thought by several abduction researchers to have experienced an alien abduction from people who profess no knowledge or suspicion about having been abducted, and from people who we asked to simulate having had an abduction experience. Our simulators "stand in" for people trying to fool researchers or the public by claiming that they have been abducted by aliens, but they do not "stand in" for self-deluded abductees.

We have no data from our API respondents on the MMPI, other personality tests, intelligence tests, or tests of fantasy-proneness, so we cannot directly relate their API performance to the significant personality variables identified in some of the earlier studies reported here. The most challenging counterexamples defined by the previous studies are the abductees tested by Clancy et al., who make more substitution errors than controls in false memory tests, and the abductees tested by several

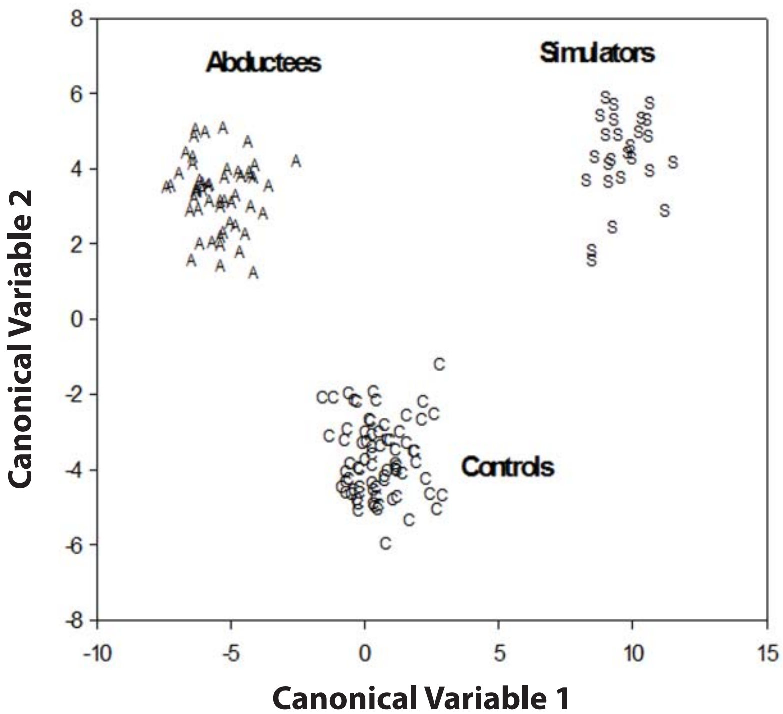

Figure 2. Separation of the abductee, control, and simulator groups on two canonical variables based on 65 questions from a discriminant analysis of the 608 questions of the American Personality Inventory.

researchers reporting high rates of childhood sexual abuse. We do not know whether any of the 52 abductees tested in our study fall into either of those groups.

Twenty of our 52 abductees (9 men and 11 women) reported their experiences by unaided recall before taking the API. The other 32 abductees underwent hypnotically induced memory retrieval that elicited the abduction report before they took the API. Based on their API scores, these two subgroups were indistinguishable. Therefore hypnosis as a memory retrieval tool did not influence the API score of the abductees, nor did it influence the API's separation of abductees from non-abductee controls and from simulators.

Because only a few witnesses claim to have seen someone abducted into a UFO (Hopkins 1996), an API score can place someone only with one of the response groups that were defined in this study. But the people with API scores in the range of the abductee group are like the twenty abductees who took the API before undergoing hypnosis. Those twenty abduction reports are therefore direct testimony based on consciously recalled experience.

Based on the results from our own abductees and data from the other researchers whose findings were summarized earlier, we hypothesize that some abductees report abductions because they confuse fantasies based on popular culture with memories based on real events, either as a defense against remembering childhood abuse or because they are inclined to fantasize as a matter of personality style. But we also hypothesize that many of the people reporting an alien abduction experience who are found to be psychologically normal when tested afterward are reporting an experience that actually happened to them. There is more to learn about the personality characteristics of people who report an alien abduction experience, and more to learn about the experience itself, before either hypothesis can be confirmed.

Ultimately the API can only serve to reinforce or weaken confidence that someone reporting an abduction narrative has an emotional and attitude profile like those of people reporting abduction narratives to researchers who found those narratives convincing. Deciding whether any of those narratives are true requires additional evidence. But it is an important decision. One verified abduction will change what we know about our place in the universe.

Notes

1 Self-reported abductees will from here on simply be called abductees.

2 The age, background, previous involvement with abduction researchers, and other details about the abductees are available from the corresponding author (dcdonderi@sympatico.ca).

3 The abductee participants were obtained and tested as follows: Hopkins, 20 men and 16 women; David Jacobs, 1 man and 9 women; John Carpenter, 2 men and 2 women; Ted Davis, 1 man; Oliver Kemenczky, 1 man.

4 We do not present either the 608-item full test or the 80-item shorter version (65 discriminant questions and 15 fillers) here so as to maintain their confidentiality, but we will send either test version to qualified investigators who would like to use them. Contact the corresponding author (dcdonderi@sympatico.ca)

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RESEARCH ARTICLE

Description of Benveniste's Experiments Using Quantum-Like Probabilities

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Abstract—Benveniste's experiments (also known as "memory of water" or "digital biology" experiments) remain unresolved. In some research areas, which have in common the description of cognition mechanisms and information processing, quantum-like statistical models have been proposed to address problems that were "paradoxical" in a classical frame. Therefore, the outcomes of the cognitive state of the experimenter were calculated for a series of Benveniste's experiments using a quantum-like statistical model (i.e. a model inspired by quantum physics and taking into consideration superposition of quantum states, non-commutable observables, and contextuality). Not only were the probabilities of "success" and "failure" of the experiments modeled according to their context, but the emergence of a signal from background was also taken into account. For the first time, a formal framework devoid of any reference to "memory of water" or "digital biology" describes all the characteristics of these disputed results. In particular, the difficulties encountered by Benveniste (reproducibility of the experiments, disturbances after blinding) are simply explained in this model without additional ad hoc hypotheses. It is thus proposed that we see Benveniste's experiments as the result of quantum-like probability interferences of cognitive states.

Keywords: Memory of water—quantum cognition—quantum-like probabilities—entanglement—experimenter effect—contextuality—nonlocal interactions

"There is no objective explanation of these observations."
—Maddox (1988a)

The Everlasting Story of the "Memory of Water"

The above quote of John Maddox, a former Editor of the journal *Nature*, is from the Editorial of the 30 June 1988 journal issue containing an article that shortly after became famous as the starting point of the "memory of water" controversy (Davenas et al. 1988). Actually, the story of the "memory of water" began in the early 1980s. Due to industrial contracts with two homeopathic firms, scientists from Unit 200 of INSERM (the French biomedical and public health research institution), led by Jacques Benveniste, assessed with biological models the effects of solutions obtained according to the principles of homeopathy. After serial ten-fold or hundred-fold dilutions, the probability of finding a biologically active molecule becomes close to zero in high dilutions. However, in some experiments with white blood cells containing polymorphonuclear basophils, a variation in basophil counts was observed repeatedly, thus suggesting that high dilutions had an effect on cells. Initially skeptical about homeopathy and its principles (from another age), Benveniste began to revise his opinion.

After several years of extensive experimental work, Benveniste convinced himself that trivial explanations such as contamination could not explain these odd results, and he decided to bring them to the attention of the scientific community. A long negotiation then began with the journal *Nature* in June 1986. Successive versions of an article were written, including new experiments requested by the reviewers. In its last version, the manuscript described experiments in which highly diluted immunoglobulins decreased the counts of basophils stained by a classical method and counted under a microscope. Meanwhile, two articles on the high dilutions were published by Benveniste's team in other scientific journals (Davenas, Poitevin, & Benveniste 1987, Poitevin, Davenas, & Benveniste 1988). However, *Nature*'s Editor and reviewers of the manuscript continued to express their skepticism regarding the idea of a "biological effect without molecules."

Unexpectedly, at the end of May 1988, John Maddox, the Editor of *Nature*, decided to publish the article for the next month provided that Benveniste accept an investigation into his laboratory (Davenas et al. 1988). Strangely, this investigation would take place after publication of the article. Details on the survey performed in Benveniste's laboratory and on the whole story of the "memory of water" can be found elsewhere (de Pracontal 1990, Alfonsi 1992, Kaufmann 1994, Schiff 1998, Benveniste 2005, Beauvais 2007).

Maddox himself was a former theoretical physicist, and none of the investigators was a specialist in the research done in Benveniste's laboratory or more generally had a background in biology. Indeed, the trio of investigators formed by Maddox had an a priori: They were certain that Benveniste acted in good faith, but that someone was playing tricks without his knowledge. The other investigators were Walter Stewart, an American chemist disputed in academic circles for his investigations on cases of scientific fraud, and the stage magician James Randi, star of many entertainment shows in the United States (and also debunker of pseudo-science). The role of Randi (as he himself said later) was to inconspicuously monitor the members of Benveniste's laboratory (Beauvais 2007). On the third day of the inquiry, Randi had to go to the evidence: He did not observe any suspicious behavior. In addition, the experiments performed during these three days (including one blind experiment) confirmed the results of the published article.

Consequently, for the next two days, the investigators decided to organize a new series of experiments, and they involved themselves in the experiments that they were supposed to control: Stewart not only blinded the experimental samples but also pipetted the cell suspensions containing stained basophils, which were then counted under a microscope by two members of Benveniste's team. Despite repeated remarks on the poor quality of some cell samples, the investigators insisted that these counts be completed "for statistics" (Beauvais 2007). The results obtained with these latter experiments did not support the alleged effect of high dilutions. A few weeks later *Nature* published a report concluding that the results claimed in the article were a "delusion" and were the consequence of both observer bias and ignorance of statistical laws (Maddox 1988b, Maddox, Randi, & Stewart 1988). For many people, the report from *Nature* was the last word on the story of the "memory of water." In the years following this harmful episode, Benveniste continued his research in this disputed area with a reduced team, using other biological systems and developing new devices as described in the next section.

From High Dilutions to "Digital Biology"

After the episode in 1988, some authors, including Benveniste's team, attempted to reproduce the results of the *Nature* paper and published negative (Ovelgonne, Bol, Hop, & van Wijk 1992), ambiguous (Hirst, Hayes, Burridge, Pearce, & Foreman 1993), or positive results (Benveniste, Davenas, Ducot, Cornillet, Poitevin, & Spira 1991, Belon, Cumps, Ennis, Mannaioni, Sainte-Laudy, Roberfroid, & Wiegant 1999, Brown & Ennis 2001); see also the review in Ennis (2010). Meanwhile, Benveniste's team explored other biological models that were hoped to be more persuasive than the basophil model. The most notable results were obtained first with the isolated heart model and some years later with an in vitro coagulation model.

The results with the isolated heart model (using Langendorff apparatus) are very helpful to understand Benveniste's issues with "reproducibility." Less famous than the basophil experiments, these results nevertheless were published as abstracts and posters at international congresses from 1991 to 1999. Moreover, during the period 1992–1997, Benveniste and his team regularly organized “public demonstrations” where scientists were invited to blind experimental samples or files to convince themselves of the reality of the alleged phenomena. These demonstrations were carefully designed with a written protocol, and, after completion, the participants received a detailed report with raw data. Therefore, valuable data that could be analyzed were available.

All these experiments have been described in detail elsewhere (Beauvais 2007). In the present article, biological systems will be considered simply as black boxes with inputs and outputs. Indeed, the aim of the article is to describe the logical aspects and the underlying mathematical structures of these experiments.

Briefly, the Langendorff apparatus allows for the maintaining of a rodent heart while different parameters (beat rate, coronary flow, muscular tension) are recorded continuously; the variations related to the addition of pharmacological agents are studied. Benveniste’s team focused on the flow rate of the coronary arteries, which initially appeared to respond significantly to high dilutions. After each run, the intensity of flow change allowed discriminating “active” samples (10% or more of maximal variation of basal flow) from “inactive” samples (below 10% variation, i.e. not different from background noise).

The advantage of the isolated heart model (Langendorff apparatus) over the basophil model was the possibility of showing in real time the biological effect of high dilutions to scientists visiting the laboratory. Indeed, the changes of baseline flow (20%–30%) were easily seen in the series of tubes that collected (one tube per minute) the physiological solution from coronary circulation. However, the recurrent criticism of contamination of samples containing high dilutions was not discarded.

In 1992, Benveniste alleged that a low-frequency amplifier allowed the “electromagnetic transfer” of the “activity” from a biologically active solution (inserted in an electric coil) to naïve water. Interestingly, this device could use water in a sealed vial. Therefore, explaining the observed effects by contamination was less relevant. New “progress” was accomplished in 1996 when Benveniste used a personal computer with a sound card to “record” and to store as a digital file the “activity” of a solution placed into the electric coil. The “replay” was performed in naïve water put inside an electric coil wired at the output of the sound card. Positive results comparable with those observed with high dilutions were obtained.

For Benveniste, this was a new era for biology and medicine, and he coined the expression “digital biology.” These new experiments, however, encountered more skepticism (if possible) than the previous high-dilution experiments. Further progress was achieved by positioning the electric coil (which diffused the "electromagnetic information") directly around the column of physiological liquid that perfused the isolated heart. With this modification, the experiment could be piloted directly from the computer, without an intermediary water sample. The electromagnetic field of the electric coil became the unique link between the computer and the apparatus. The contamination argument seemed to be definitively discarded.

Despite these successive improvements, however, an issue literally poisoned the demonstrations aimed to provide "proof" of the reality of the "memory of water." As explained in the next section, this issue was more particularly evidenced after blinding of the experimental samples during the "public demonstrations."

Contextuality as the Central Issue: In-House Blinding vs. Blinding by Outside Observer

All participants in these experiments, including Benveniste himself, acknowledged that besides the very impressive, convincing, and "clean" experiments, other experiments cast doubt on the reality of the alleged phenomena (Benveniste 2005, Thomas 2007, Beauvais 2008, Poitevin 2008). This was particularly evident after blinding of samples—not for in-house blinding, which led to statistically significant correlations, but for blinding during public demonstrations with "outside" observers. Even the early experiments with basophils were not free from blinding disturbances. Thus, the usual large and regular waves of biological activity related to high dilutions and routinely obtained by some teams became unnoticeable during large-scale blind experiments (Benveniste, Davenas, Ducot, Cornillet, Poitevin, & Spira 1991, Belon, Cumps, Ennis, Mannaioni, Sainte-Laudy, Roberfroid, & Wiegant 1999). With the Langendorff apparatus and with the coagulation model, the blinding issue became a central concern. Moreover, a phenomenon that was already suggested by basophil experiments became obvious: "Better" results were obtained with some "gifted" experimenters (Beauvais 2007).

Initially, it was proposed that uncontrolled parameters in the environment, such as electromagnetic waves or quality of water, were probably responsible for these discrepancies. Indeed, detecting a weak signal amid a noisy background could be the reason for poor results. In retrospect, however, it now appears that the difficulties of reproducibility were unusual. This was particularly obvious during the "public demonstrations" that Benveniste organized to convince other scientists that the phenomenon he described was not imaginary. These demonstrations were usually performed in two steps. First, negative and positive samples were produced (e.g., high dilutions, samples of "informed water" or digital files) and were blinded (the initial label was replaced by a code) by an observer not belonging to Benveniste's team. It is important to emphasize that some negative and positive samples were kept open. Second, the samples were brought back to Benveniste's laboratory where the team tested all samples (blind and open-label) on the biological system. Note also that the samples kept open were nevertheless frequently blinded by a member of the team before being given to the experimenter dedicated to the testing. When all measurements were made, the results of the experiments were sent to the outside scientists who assessed the concordance of observed results with expected results.

In these demonstrations, the mean biological effects after repeated experiments (on several biological preparations) were usually clear-cut, and active samples were easily distinguished from inactive samples. However, the results of blind samples were almost always at random and did not fit the expected results: Some samples with "control" labels were clearly active on the biological system whereas some samples with "active" labels had no significant effect. Table 1 describes an example of an experiment involving a participating outside observer.

In a first approach, it could be hypothesized that active samples had been "erased" by an external disturbing influence. However, it is more difficult to explain the mechanisms that transformed inactive samples into specific "active samples." And even if we assume the hypothesis of a "noisy" environment, how do we explain the open samples (positive and negative samples), which were prepared, transported, and tested at the same time and in the same conditions as blind samples, giving systematically "correct" results (i.e. expected correlations between supposed causes and biological outcomes)?

After each failure of public demonstration, Benveniste's team improved the experimental setting, and either open-label or in-house blind experiments confirmed that "good" results were obtained with the new device or with the modified experimental design. Nonetheless, despite the successive technical improvements of the different experimental systems, the weirdness persisted.

To avoid any interference with the environment (including the experimenter), Benveniste's team constructed an automatic robot analyzer based on a new promising biological model, the coagulation system. After filling it with consumables, the whole process was automatic, from the random choice of files (to be "played" to naïve water) to the printing of the results. At this time, Benveniste's "digital biology" attracted the attention of the Defense Advanced Research Projects Agency (DARPA), of the US Department of Defense responsible for the development of new technology. In 2001, a multidisciplinary team was commissioned by DARPA to study these

TABLE 1

Example of Random Correlations between Labels and Biological Outcomes in an Experiment Involving a Participating Outside Observer (Type-1 Observer)

Experimental Samples [a]	Biological Outcome	Unblinding of Blind Files	Expected Biological Outcome
Blind files			
File #1	Signal	*IN* #2	No
File #2	Background	*IN* #3	Yes
File #3	Signal	*AC* #3	Yes
File #4	Background	*IN* #1	Yes
File #5	Signal	*IN* #1	No
File #6	Signal	*IN* #3	No
File #7	Background	*AC* #1	No
File #8	Background	*AC* #1	No
File #9	Signal	*AC* #1	Yes
File #10	Signal	*AC* #2	Yes
Open files [b]			
IN #A	Background	-	Yes
IN #B	Background	-	Yes
AC #C	Signal	-	Yes
AC #D	Signal	-	Yes
"Classical" positive control	Signal	-	Yes

[a] For this experiment of "digital biology" (an avatar of "memory of water") performed in September 1997, 10 blind files and 4 open-label files of digital recordings of different samples were produced in a foreign laboratory and then blinded by the participating outside observer (type-1 observer) (Beauvais 2007, 2012). Five "active" (*AC*) labels and five "inactive" (*IN*) labels were blinded; two *IN* and two *AC* labels were kept open. The 4 open-label files were nevertheless in-house blinded before measurements. In Benveniste's laboratory, experiments were performed with each file and the associated biological outcome was recorded: either "background" ("$\downarrow$") (i.e. outcome below cutoff at 10) or signal ("$\uparrow$") (i.e. outcome above cutoff). The biological device was a Langendorff apparatus, which allowed measuring the variations of an isolated rodent heart. After completion of the measurements in Benveniste's laboratory, the results were sent to the participating outside observer who assessed the number of concordant pairs (*IN* with$\downarrow$ and *AC* with$\uparrow$) and discordant pairs (*IN* with $\uparrow$ and *AC* with $\downarrow$). This experiment is representative of many other "public" experiments detailed elsewhere (Beauvais 2007). Despite repetitive measurements for each file and coherence of the results for each file, blind files were associated randomly with "signal" (biological outcome >10) and "background" (biological outcome <10). In contrast, expected results were obtained with the in-house blind files. Even though such an experiment dismisses the hypothesis of "memory of water" or "digital biology," the presence of signal remained puzzling.

[b] Labels kept open by the participating outside observer (type-1 observer), but nevertheless in-house blinded (type-2 observer) before measurement.

TABLE 2
Concordant and Discordant Pairs in Different Experimental Conditions in Benveniste's Experiments with the Langendorff Apparatus

Experimental Situations	Number of Experimental Points	Outcome ↓ (Background)	Outcome ↑ (Signal)	P-Value [a]
Open-label experiments [b]				
Label *IN*	N=372	**93% (CP)**	7% (DP)	$<1 \times 10^{-83}$
Label *AC*	N=202	11% (DP)	**89% (CP)**	
Experiments blinded by type-2 observer				
Label *IN*	N=118	**91% (CP)**	9% (DP)	$<1 \times 10^{-26}$
Label *AC*	N=86	15% (DP)	**85% (CP)**	
Experiments blinded by type-1 observer				
Label *IN*	N=54	57% (CP)	43% (DP)	0.25
Label *AC*	N=54	44% (DP)	56% (CP)	

Summary of results presented in Beauvais (2012).
Bold type numbers are statistically significant concordant pairs.
CP, concordant pairs; DP, discordant pairs; *IN*, "inactive" labels; *AC*, "active" labels.
[a] Chi-square test.
[b] See also Figure 1.

potentially interesting experiments. After completion of the experiments performed in part with the help of Benveniste's team, the experts concluded they could not confirm that an effect related to "digital biology" was involved, while they did confirm the importance of the experimenter for the outcome. Indeed, they suggested that unknown "experimenter effects" could explain these unusual results, but that a theoretical framework was necessary to understand them. They added: "Without such a framework, continued research on this approach to digital biology would be at worst an endless pursuit without likely conclusion, or at best premature" (Jonas et al. 2006).

The Different Experimental Situations with or without Correlations

In our previous reappraisal of Benveniste's experiments, we defined three experimental situations (open-label, in-house blinding, and blinding by a participating outside observer) that led to "success" or "failure" (Beauvais 2012). Table 2 summarizes the results of this reappraisal.

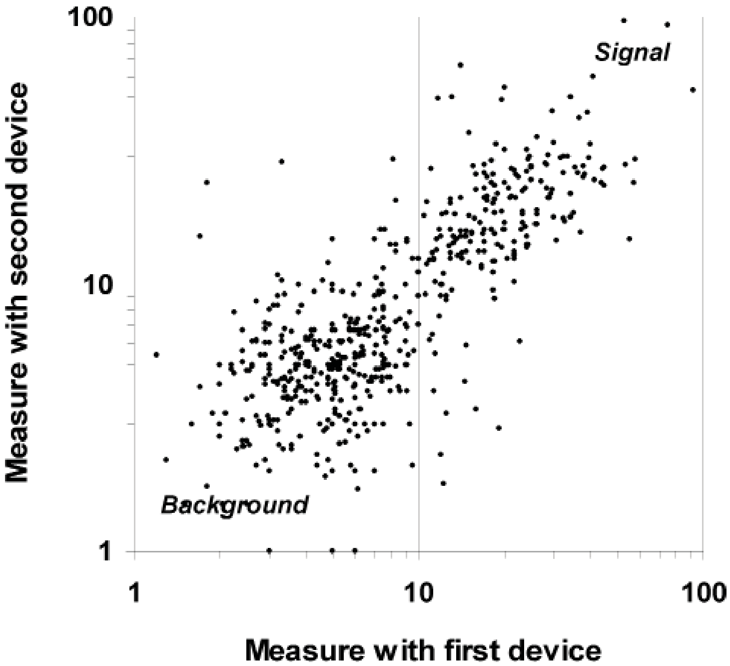

Figure 1. Correlations of measurements on two parallel devices.
These plots (574 pairs of measures) summarize a systematic analysis of large-scale experiments performed from 1992 to 1996 by Benveniste's team (Beauvais 2012). The limit between background and signal was set at 10. For these experiments each measurement was performed in duplicate on two devices (this was done to guarantee results). Note that the probability of obtaining a signal (with respect to the background) for a second measure was high if a signal (with respect to the background) was obtained for the first measure. Even if "memory of water" is dismissed, we have to explain 1) how a signal emerged and 2) how a correlation was obtained.

The open-label experiments led to "correct" correlations between labels ("inactive" or "active") and device outcomes (background or signal). For open-label experiments, background was observed in 93% of the cases with "inactive" label, and signal was observed in 89% of the cases with "active" label (Table 2 and Figure 1).

In-house blind experiments, i.e. blinding performed by an "inside" observer, also led to significant correlation. The "inside" observer will now be named *type-2 observer*. For experiments blinded by a type-2 observer, background was observed in 91% of the cases with "inactive" label, and signal was observed in 85% of "active" label cases.

The difference of effect between “inactive” and “active” samples was statistically very significant in these two experimental situations (no blinding, or blinding by type-2 observer) (Table 2). Therefore, these experiments were usually considered as successes; it was as if a causal relationship existed between the alleged causes and the outcomes.

The crucial issue was observed when the blinding of the samples was performed by a participating outside observer (e.g., the public demonstration described in Table 1). The participating outside observer will now be named *type-1 observer*. When all measurements had been carried out by the experimenter on the Langendorff apparatus, the results were sent by Benveniste’s team to the type-1 observer who held the code of the blinded samples and who compared the two series (biological outcomes and labels of the corresponding samples). In this situation, the biological outcomes (signal or background) were distributed at random according to the initial label (“inactive” or “active” samples) (Table 2). For these experiments, background was observed in 57% of “inactive” labels and signal in 56% of “active” labels. These experiments were thus usually considered as failures; the alleged relationship between labels and outcomes appeared broken.

In summary, correlations were evidenced either in open-label experiments or in experiments blinded by a type-2 observer; in sharp contrast, in blind experiments involving a type-1 observer, the correlations vanished. Nevertheless, in all cases, a signal emerged from background.

Benveniste’s Experiments Free of the Memory-of-Water Hypothesis

In our previous article, we analyzed the experiments with the Langendorff system, and we concluded that they did not support the hypothesis of the “memory of water” (Beauvais 2012). We did not reach this conclusion because the known physical properties of water did not support memory in this liquid as argued by some authors (Teixeira 2007), but simply because a subset of results from Benveniste’s experiments themselves dismissed this hypothesis.

In a first step, we analyzed a set of experiments obtained by Benveniste’s team in the 1990s. We quantified the relationship between “expected” effects (i.e. labels of the tested samples) and apparatus outcomes, and we defined the experimental conditions to observe significant correlations. We observed that the results were amazingly identical despite the various “stimuli” thought to induce a signal (high dilutions, direct “electromagnetic transfer” from a biological sample, “electromagnetic transfer” from a stored file, and transfer of the “biological activity” of homeopathic granules to water). Moreover, a diversity of electronic devices was used, particularly electric coils with various technical characteristics. In other words, the dynamic range of the “measure apparatus” used to evidence “informed water” seemed to be exceptionally large for the "input" but was nevertheless associated with a monotonous response for the "output." What appeared to be the "cause" of the outcome was the "label" of the sample ("inactive" or "active") and not the specific physical process that had supposedly "informed" the water.

We concluded that the results of these experiments were related to experimenter-dependent correlations, which did not support the initial "memory of water" hypothesis. Nevertheless, the fact that a signal emerged from background noise remained puzzling.

Therefore, in a second step, we described Benveniste's experiments according to the relational interpretation of quantum physics (Beauvais 2012). This interpretation allowed for the elaboration of a first quantum approach of Benveniste's experiments: The emergence of a signal from background noise was described by the entanglement of the experimenter with the observed system.

Although our hypothesis did not definitely dismiss the possibility of "memory of water," the experimenter-dependent entanglement was an attractive alternative interpretation of Benveniste's experiments. However, quick decoherence of any macroscopic system is an obstacle to the general acceptance of such an interpretation.

In the next section, we propose a parallel between Benveniste's experiments and classical interference experiments. This parallel allows for a description of a more complete formalism of Benveniste's experiments.

The Single-Particle Interference Experiment

Single-particle quantum interference is one of the most important phenomena that illustrate the superposition principle and highlight the major difference between quantum and classical physics. The two-slit interferometer of Young can be used for one-particle interference experiments, but the Mach-Zehnder device has the advantage of ending only with two detectors (D1 and D2) and not with a screen (i.e. a great number of detectors) (Scarani & Suarez 1998). Figure 2 (upper drawing) depicts the Mach-Zehnder device. Light is emitted from a monochromatic light source: 50% of the light is transmitted by the beam splitter (BS1) in path T and 50% is reflected in path R. In BS2, the two beams are combined and 50% of the light is transmitted by the beam splitter in detector D1 and 50% in detector D2.

If light is considered a wave, it can be calculated that waves from the two paths are constructive when they arrive in D1 and destructive in D2. Therefore, clicks after light detection are heard only in D1. This is indeed what experiment shows, and it is an argument for the wavy nature of light.

On the contrary, if we consider light a collection of small balls (photons), they should randomly go into path T or R (with a probability of

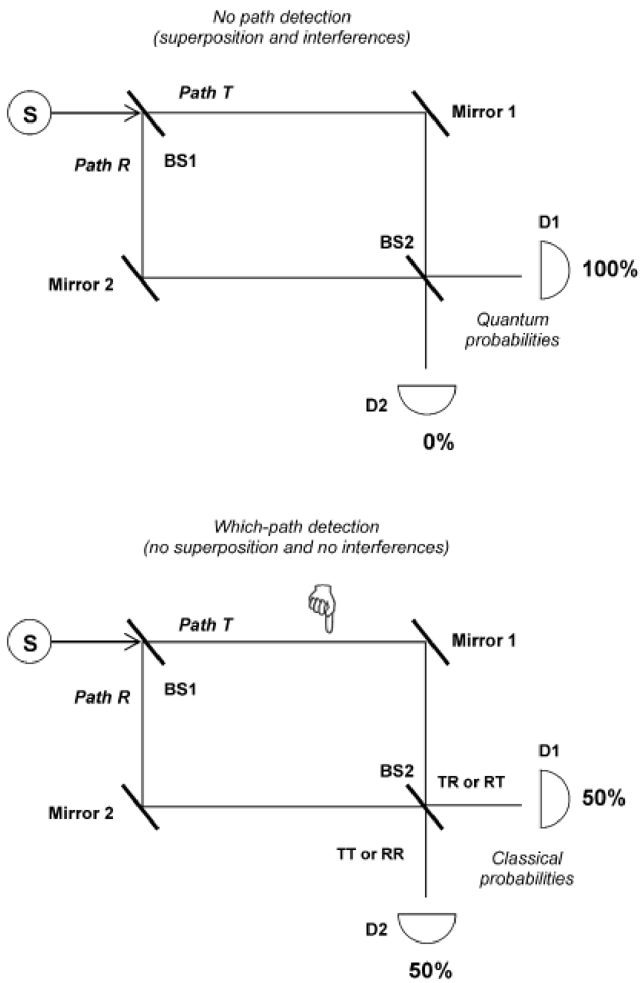

Figure 2. Single-particle interference in a Mach-Zehnder interferometer with or without which-path measurement.

0.5 for each path) and then in BS2 they go into D1 or D2 randomly (again with a probability of 0.5 for D1 or D2). As a consequence D1 should click in 50% of cases and D2 in 50% of cases.

However, if photons are emitted one by one (by decreasing light intensity), the interference pattern persists (100% of clicks in D1). This is a quite counterintuitive result. Even more astonishingly, this unexpected (nonclassical) behavior disappears if the initial path (T or R) is detected by

TABLE 3
Parallelism between Single-Photon Interference Experiment with Mach-Zehnder Interferometer and Benveniste's Experiments

	Interferometer Experiment	Benveniste's Experiments [a]
First path	Path T	A_{IN}
λ_1^2	Prob (path T)	Prob (A_{IN})
Second path	Path R	A_{AC}
λ_2^2	Prob (path R)	Prob (A_{AC})
Superposition (quantum probabilities)	Path T *and* Path R	A_{IN} *and* A_{AC}
Outcome 1	100% detector D1	100% "concordant" pairs [b]
Outcome 2	0% detector D2	0% "discordant" pairs [c]
No superposition (classical probabilities)	Path T *or* Path R	A_{IN} *or* A_{AC}
Outcome 1	50% detector D1	50% "concordant" pairs [b]
Outcome 2	50% detector D2	50% "discordant" pairs [c]

$\downarrow$, background; $\uparrow$, signal.
A, cognitive state of the experimenter; IN, "inactive" labels; AC, "active" labels; T, transmission; R, reflection.
[a] For an experiment with optimal correlations between labels and biological outcomes (and with $\lambda_1^2 = \lambda_2^2$)
[b] A_{IN} with $A_{\downarrow}$ or A_{AC} with $A_{\uparrow}$.
[c] A_{IN} with $A_{\uparrow}$ or A_{AC} with $A_{\downarrow}$.

any means: Then either D1 or D2 clicks, each in 50% of cases (classical probabilities apply) (Figure 2, lower drawing).

We made a parallel between Benveniste's experiments and the one-particle interference experiment, which appeared to have isomorphic underlying mathematical structures. Indeed, according to the context of the experiment, either only concordant pairs (equivalent to detection in D1) or both concordant/discordant pairs (i.e. equivalent to random detection by D1 and D2) were obtained (Figure 3 and Table 3).

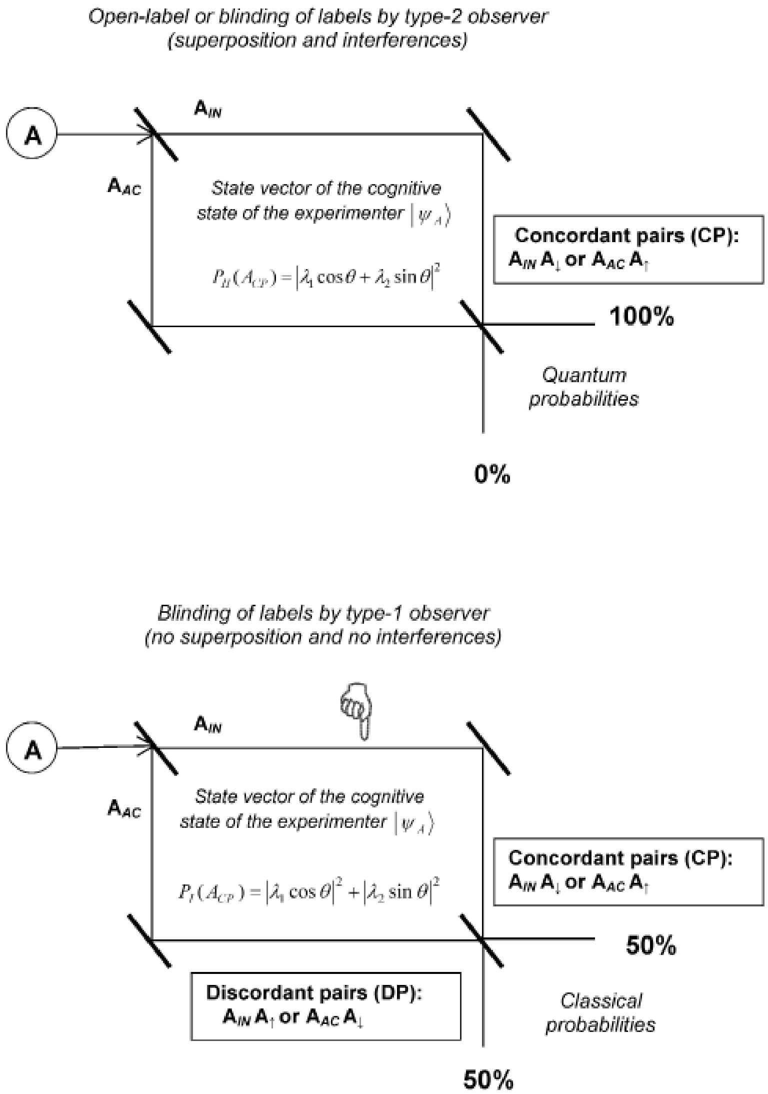

Figure 3. Interpretation of Benveniste's experiments as a consequence of quantum-like interferences (for an experiment with an optimal interference term).

If the sample labels are not blinded or blinded by a type-2 observer, then the cognitive state of A (described by the state vector $|\psi_A\rangle$) is able to interfere with itself (as a single particle interferes with itself) and the rate of correlated pairs is high. If a type-1 observer blinds the sample labels, then the cognitive state of A cannot interfere with itself (there is no superposition) and the rate of correlated pairs is not better than random. In both cases, the signal is observed.

The Quantum Formalism in Brief

The objective of our study is to describe the possible outcomes of the cognitive states of an experimenter in different contexts. Mathematically, a state is represented by a vector in a Hilbert space. Using the quantum formalism, the cognitive state of the experimenter is represented by the state vector $|\psi_A\rangle$, which summarizes all the information on the quantum system.

A key ingredient in the quantum formalism is the principle of superposition. According to this principle, the linear combination of any set of states is itself a possible state. Thus, if $|A_1\rangle$ and $|A_2\rangle$ are two possible states of the system, then $|\psi_A\rangle = \lambda_1 |A_1\rangle + \lambda_2 |A_2\rangle$ also is a possible state of A (with λ_1 and λ_2 real or complex numbers). This is due to the linearity of the Schrödinger equation: Any linear combination of solutions to a particular equation will also be a solution to it.

Therefore, a physical system exists in all its particular and theoretically possible states. When it is "measured," only one state among the possible states is observed by the experimenter. The quantum formalism states that the probability to observe $|A_1\rangle$ is the square of the probability amplitude λ_1 associated with this state.

An example of superposition that is directly observable is the interference pattern observed in the two-slit experiment. Interferences are the hallmark of superposed states and are the heart of quantum physics. Quantum interference is the consequence of non-commutable observables, as described in Figure 4.

In a single-photon interference experiment, if one can (even in principle) distinguish the path each photon has taken, then interferences vanish and classical probabilities apply. In the setup depicted in Figure 2, the initial path cannot be distinguished in the upper drawing, and interferences occur; in the lower drawing, paths are distinguished by measurement, and consequently classical probabilities apply (without the interference term).

The formalism of single-particle interference has been widely described and we propose to use it to describe Benveniste's experiments (Table 3 and Figure 3).

Type-1 Observer (Wigner) and Type-2 Observer (Wigner's Friend)

The distinction that we made between the type-1 ("outside") observer and the type-2 ("inside") observer is reminiscent of the thought experiment proposed by the physicist Eugene Wigner in the early 1960s and known as "Wigner's Friend" (D'Espagnat 2005). In this thought experiment, Wigner's friend performs a measurement on a quantum system in a superposed state

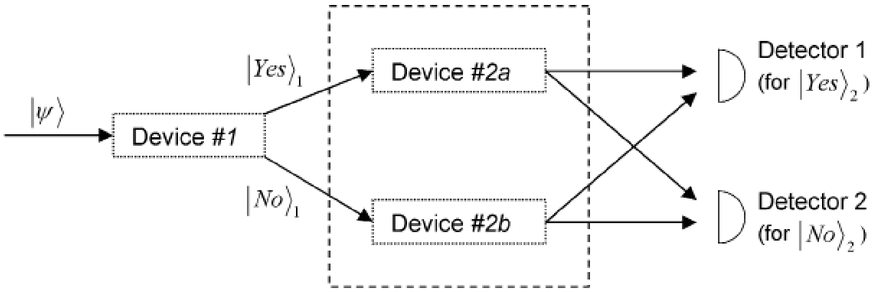

Figure 4. Design of an experiment exhibiting quantum-like interferences. The quantum object is measured through two successive devices named #1 and #2. First, device #1 splits the state $|\psi\rangle$ into two new orthogonal states, denoted $|Yes\rangle_1$ and $|No\rangle_1$. These two states are then fed into two identical devices, #2a and #2b, and each device splits the states into two new orthogonal states, $|Yes\rangle_2$ and $|No\rangle_2$, such that they are recombined at detectors D1 and D2. It is assumed that the observables associated with the first device do not commute with observables associated with the second device. If the events inside the box are not measured, the system is in a superposition of states, which is not equal to either one. The consequence of superposition is that quantum probabilities to observe $|Yes\rangle_2$ (or $|No\rangle_2$) in detector 1 (D1) (with respect to detector 2 (D2)) are different compared with classic probability due to the interference term.

(namely, a Schrödinger's cat); a second experimenter (Wigner) remains outside the laboratory. Inside the laboratory, from the perspective of Wigner's friend, the cat is either dead *or* alive at the end of the experiment ("collapse" of the quantum wave from a superposed state). Outside the laboratory, from the perspective of Wigner, the quantum system and Wigner's friend are described in a superposed state with the two possible outcomes (cat dead *and* cat alive). If Wigner enters the laboratory, he sees the cat (and his friend in the corresponding state) either dead *or* alive ("collapse" of the quantum wave from a superposed state). Therefore, there are two valid—but different—descriptions of the same quantum state with apparent "collapse" of the quantum wave at different times: This is the so-called "measurement problem." For Wigner (the physicist), this discrepancy between the inside and outside perspectives illustrated the role of consciousness, which seems to play a role by "ending" the chain of quantum measurements.

We can make a parallel with on the one hand the type-1 and type-2 observers in Benveniste's experiments, and on the other hand Wigner and his friend, respectively. The type-2 observer (i.e. Wigner's friend) belongs to the same "branch of reality" as Benveniste's experimenter (i.e. Schrödinger's cat) whereas the type-1 observer (i.e. Wigner) considers that the type-2 observer (or the experimenter) is in a superposed state until he interacts with him.

The Quantum-Like Formalism Applied to Benveniste's Experiments

We define $\mathrm{P_I}(\mathrm{A}_{CP})$ as the probability for the cognitive state (named A) of the experimenter to be associated with concordant pairs (*CP*) according to *classical probabilities*; $\mathrm{P_{II}}(\mathrm{A}_{CP})$ is the probability of A being associated with concordant pairs according to *quantum probabilities*. $\mathrm{P_I}(\mathrm{A}_{DP})$ and $\mathrm{P_{II}}(\mathrm{A}_{DP})$ are the respective $\mathrm{P_I}$ (classical) and $\mathrm{P_{II}}$ (quantum) probabilities for discordant pairs (*DP*).

We describe the experimental situation from the point of view of an observer who knows the initial state of the system and does not perform any measurement/observation on it. The state vector of the cognitive state of the experimenter is described in terms of the eigenvectors of the first observable (cognitive states of A indexed with labels *IN* and *AC*):

$$|\psi_A\rangle = \lambda_1 |A_{IN}\rangle + \lambda_2 |A_{AC}\rangle$$

(λ_1^2 and λ_2^2 are the probabilities associated with the states A_{IN} and A_{AC}, respectively).

We develop the eigenvectors of the first observable on the eigenvectors of the second observable (concordance of pairs). We postulate that the cognitive states of A indexed with "labels" and the cognitive states of A indexed with "concordance of pairs" are non-commutable observables:

$$|A_{IN}\rangle = \mu_{11} |A_{CP}\rangle + \mu_{12} |A_{DP}\rangle$$

$$|A_{AC}\rangle = \mu_{21} |A_{CP}\rangle + \mu_{22} |A_{DP}\rangle$$

Therefore, we can express $|\psi_A\rangle$ as a superposed state of $|A_{CP}\rangle$ and $|A_{DP}\rangle$:

$$|\psi_A\rangle = (\lambda_1\mu_{11} + \lambda_2\mu_{21})\,|A_{CP}\rangle + (\lambda_1\mu_{12} + \lambda_2\mu_{22})\,|A_{DP}\rangle$$

The probability of A_{CP} is the square of the probability amplitude associated with its state:

$$P_{II}\,(A_{CP}) = |\lambda_1\mu_{11} + \lambda_2\mu_{21}|^2$$

$$P_{II}\,(A_{CP}) = \lambda_1^2\,\mu_{11}^2 + \lambda_2^2\,\mu_{21}^2 + 2\,\lambda_1\lambda_2\mu_{11}\mu_{21}$$

Similarly, $P_{II}\,(A_{DP})$ is calculated:

$$P_{II}\,(A_{DP}) = \lambda_1^2\,\mu_{12}^2 + \lambda_2^2\,\mu_{22}^2 + 2\,\lambda_1\lambda_2\mu_{12}\mu_{22}$$

If a type-1 observer has blinded the labels, the context of the experiment changes. This is formally equivalent to a which-path measurement in single-particle interference. Indeed, we have to take into account the path information; therefore, classical conditional probabilities that include path data must be used for calculation of the probability for A to be associated with concordant pairs:

$$P_I(A_{CP}) = P(A_{IN}) \times P(A_{CP}\,|\,A_{IN}) + P(A_{AC}) \times P(A_{CP}\,|\,A_{AC})$$

with $P(A_{CP}\,|\,A_{IN}) = \mu_{11}^2$ and $P(A_{CP}\,|\,A_{AC}) = \mu_{21}^2$

$$P_I(A_{CP}) = \lambda_1^2\,\mu_{11}^2 + \lambda_2^2\,\mu_{21}^2$$

And similarly, $P_I(A_{DP}) = \lambda_1^2\mu_{12}^2 + \lambda_2^2\mu_{22}^2$.

We conclude that $P_{II}(A_{CP}) \neq P_I(A_{CP})$ in the general case. In the squaring of the sum, we have obtained an additional term $2\,\lambda_1\lambda_2\mu_{11}\mu_{21}$, which is typical of all quantum mechanical interference effects.

Numerical Application with Data from Benveniste's Experiments

Useful Mathematical Formulas

We search the μ values for $P_{II}(A_{CP})=|\lambda_1\mu_{11}+\lambda_2\mu_{21}|^2$ and $P_{II}(A_{DP}) = |\lambda_1\mu_{12}+\lambda_2\mu_{22}|^2$. Since $\mu_{11}^2 + \mu_{12}^2 = 1$, $\mu_{21}^2 + \mu_{22}^2 = 1$, and $P_{II}(A_{CP}) + P_{II}(A_{DP}) = 1$, we can easily calculate that $\mu_{11}\,\mu_{21} = -\,\mu_{22}\,\mu_{12}$, $\mu_{11}^2 = \mu_{22}^2$, and $\mu_{12}^2 = \mu_{21}^2$.
Then, we can write:

$$|\,A_{IN}\,\rangle \;=\; \mu_{11}\,|\,A_{CP}\,\rangle \;+\; \mu_{12}\,|\,A_{DP}\,\rangle$$

$$|\,A_{AC}\,\rangle \;=\; -\,\mu_{12}\,|\,A_{CP}\,\rangle \;+\; \mu_{11}\,|\,A_{DP}\,\rangle$$

We note that the matrix for change of basis is a rotation matrix:

$$\begin{pmatrix} \mu_{11} & \mu_{12} \\ \mu_{21} & \mu_{22} \end{pmatrix} = \begin{pmatrix} \mu_{11} & \mu_{12} \\ -\mu_{12} & \mu_{11} \end{pmatrix} = \begin{pmatrix} \cos\theta & \sin\theta \\ -\sin\theta & \cos\theta \end{pmatrix}$$

Therefore,

$$|\,\psi_A\,\rangle \;=\; (\lambda_1\mu_{11} + \lambda_2\mu_{12})\,|\,A_{CP}\,\rangle \;+\; (\lambda_2\mu_{11} - \lambda_1\mu_{12})\,|\,A_{DP}\,\rangle, \quad \text{or}$$

$$|\,\psi_A\,\rangle \;=\; (\lambda_1\cos\theta + \lambda_2\sin\theta)\,|\,A_{CP}\,\rangle \;+\; (\lambda_2\cos\theta - \lambda_1\sin\theta)\,|\,A_{DP}\,\rangle$$

The formulas of P_{II} and P_I become:

$$P_{II}(A_{CP}) \;=\; |\lambda_1\cos\theta + \lambda_2\sin\theta|^2 \;= \lambda_1^2\cos^2\theta \;+ \lambda_2^2\sin^2\theta \;+\; 2\lambda_1\lambda_2\cos\theta\sin\theta$$

$$P_{II}(A_{DP}) \;=\; |\lambda_2\cos\theta - \lambda_1\sin\theta|^2 \;= \lambda_2^2\cos^2\theta \;+ \lambda_1^2\sin^2\theta \;-\; 2\lambda_1\lambda_2\cos\theta\sin\theta$$

$$P_I(A_{CP}) \;= \lambda_1^2\cos^2\theta \;+ \lambda_2^2\,\sin^2\theta$$

$$P_I(A_{DP}) \;= \lambda_2^2\cos^2\theta \;+\; \lambda_1^2\,\sin^2\theta$$

In a previous article, we presented Benveniste's experiments in different experimental situations. These results, summarized in Table 1, allow for calculating the parameters of the model in different experimental situations, as detailed in the next subsections.

Open-Label Experiments

For the open-label experiments, experimental data were obtained with $P(A_{IN}) = \lambda_1^2 = 0.65$ and $P(A_{AC}) = \lambda_2^2 = 0.35$ (Table 4).

We find $\cos^2\theta = 0.88$ and $\sin^2\theta = 0.12$, indeed:

$$P_{II}(A_{CP}) = |\lambda_1 \cos\theta + \lambda_2 \sin\theta|^2 = |\sqrt{0.65} \times \sqrt{0.88} + \sqrt{0.35} \times \sqrt{0.12}\,|^2 = 0.92\,.$$

$$P_{II}(A_{DP}) = |\lambda_2 \cos\theta - \lambda_1 \sin\theta|^2 = |\sqrt{0.35} \times \sqrt{0.88} - \sqrt{0.65} \times \sqrt{0.12}\,|^2 = 0.08\,.$$

Experiments Blinded by a Type-2 Observer

For experiments blinded by a type-2 observer, experimental data were obtained with

$P(A_{IN}) = \lambda_1^2 = 0.58$ and $P(A_{AC}) = \lambda_2^2 = 0.42$ (Table 4).

We find $\cos^2\theta = 0.88$ and $\sin^2\theta = 0.12$, indeed:

$$P_{II}(A_{CP}) = |\lambda_1 \cos\theta + \lambda_2 \sin\theta|^2 = |\sqrt{0.58} \times \sqrt{0.88} + \sqrt{0.42} \times \sqrt{0.12}\,|^2 = 0.88\,.$$

$$P_{II}(A_{DP}) = |\lambda_2 \cos\theta - \lambda_1 \sin\theta|^2 = |\sqrt{0.42} \times \sqrt{0.88} - \sqrt{0.58} \times \sqrt{0.12}\,|^2 = 0.12\,.$$

Experiments Blinded (or Not) by a Type-1 Observer

For the experiments blinded by a type-1 observer, experimental data were obtained with $P(A_{IN}) = \lambda_1^2 = 0.50$ and $P(A_{AC}) = \lambda_2^2 = 0.50$ (Table 4). Suppose first that we are not aware of the blinding of the experiment by a type-1 observer. We use quantum probabilities and we find $\cos^2\theta = 0.996$ and $\sin^2\theta = 0.004$:

$$P_{II}(A_{CP}) = |\lambda_1 \cos\theta + \lambda_2 \sin\theta|^2 = |\sqrt{0.50} \times \sqrt{0.996} + \sqrt{0.50} \times \sqrt{0.004}\,|^2 = 0.56.$$

$$P_{II}(A_{DP}) = |\lambda_2 \cos\theta - \lambda_1 \sin\theta|^2 = |\sqrt{0.50} \times \sqrt{0.996} - \sqrt{0.50} \times \sqrt{0.004}\,|^2 = 0.44.$$

These results indicate that the interference term is low and we obtain results close to classical probabilities:

$$P_I(A_{CP}) = \lambda_1^2 \cos^2\theta + \lambda_2^2 \sin^2\theta = 0.50 \times 0.996 + 0.50 \times 0.004 = 0.50\,.$$

TABLE 4
Extraction of the Different Parameters from Experimental Results and Use of Quantum Probabilities for Modeling

	EXPERIMENTAL SITUATION		
	Open-Label	**Blinding by Type-2 Observer [a]**	**Blinding by Type-1 Observer [a, b]**
Experimental data [c]			
$P(A_{IN}) = \lambda_1^2$	0.65	0.58	0.50
$P(A_{AC}) = \lambda_2^2$	0.35	0.42	0.50
$P(A_{CP})$	0.92	0.88	0.56
$P(A_{DP})$	0.08	0.12	0.44
Calculated parameters and modeling			
$\mu_{11}^2 = \mu_{22}^2 = \cos^2\theta$	0.88	0.88	0.996
$\mu_{12}^2 = \mu_{21}^2 = \sin^2\theta$	0.12	0.12	0.004
$P_I(A_{CP})$ (classical) [d]	0.61	0.56	0.50
$P_I(A_{DP})$ (classical) [e]	0.39	0.44	0.50
Interference term [f]	0.31	0.32	0.06
$P_{II}(A_{CP})$ (quantum) [g]	0.92 (0.61 + 0.31)	0.88 (0.56 + 0.32)	0.56 (0.50 + 0.06)
$P_{II}(A_{DP})$ (quantum) [h]	0.08 (0.39 – 0.31)	0.12 (0.44 – 0.32)	0.44 (0.50 – 0.06)

$\downarrow$, background; $\uparrow$, signal; CP, concordant pairs; DP, discordant pairs; *IN*, "inactive" labels; *AC*, "active" labels.

[a] For definition of type-1 observer (Wigner) and type-2 observer (Wigner's friend), see text.

[b] For experiments with type-1 observer including both open-label and blind samples, see text.

[c] These experimental data are from experiments described in Table 1 and in Beauvais (2012).

[d] $P_I(A_{CP}) = \lambda_1^2 \cos^2\theta + \lambda_2^2 \sin^2\theta$

[e] $P_I(A_{DP}) = \lambda_2^2 \cos^2\theta + \lambda_1^2 \sin^2\theta$

[f] $2\lambda_1\lambda_2 \cos\theta \sin\theta$

[g] $P_{II}(A_{CP}) = \lambda_1^2 \cos^2\theta + \lambda_2^2 \sin^2\theta + 2\lambda_1\lambda_2 \cos\theta \sin\theta$

[h] $P_{II}(A_{DP}) = \lambda_2^2 \cos^2\theta + \lambda_1^2 \sin^2\theta - 2\lambda_1\lambda_2 \cos\theta \sin\theta$

We have seen, however, that in the same experimental session supervised by a type-1 observer, both blind and open-label samples were included (as in the experiment described in Table 1). To model this case, it is reasonable to suppose that the values of sin θ are the same regardless of label blinding. For probability calculations, we take the values for $\cos^2\theta$ and $\sin^2\theta$ (0.88 and 0.12, respectively) as calculated in the subsections ***Open-Label Experiments*** and ***Experiments Blinded by a Type-2 Observer***:

For open labels,

$$P_{II}(A_{CP}) = |\lambda_1 \cos\theta + \lambda_2 \sin\theta|^2 = |\sqrt{0.50} \times \sqrt{0.88} + \sqrt{0.50} \times \sqrt{0.12}\,|^2 = 0.92\ .$$

After blinding by the type-1 observer, classical probabilities apply:

$$P_I(A_{CP}) = \lambda_1^2 \cos^2\theta + \lambda_2^2 \sin^2\theta = 0.50 \times 0.88 + 0.50 \times 0.12 = 0.50\ .$$

Therefore, the difference for probability of concordant pairs in open labels vs. blind labels *in the same session with a type-1 observer* is well-described by the proposed formalism: The probability of observing concordant pairs is high with open-label samples (Probability = 0.92), but lower for blind samples (Probability = 0.50) and not better than random in this case.

Comments on the Quantum-Like Formalism Applied to Benveniste's Experiments

Non-Commutable Observables and Emergence of Signal

If $\theta = 0$, then the observables are commutable:

$$|A_{IN}\rangle = \cos\theta \times |A_{CP}\rangle + \sin\theta \times |A_{DP}\rangle = 1 \times |A_{CP}\rangle + 0 \times |A_{DP}\rangle = |A_{CP}\rangle$$

$$|A_{AC}\rangle = -\sin\theta \times |A_{CP}\rangle + \cos\theta \times |A_{DP}\rangle = 0 \times |A_{CP}\rangle + 1 \times |A_{DP}\rangle = |A_{DP}\rangle$$

In this particular case, the observation of concordant pairs is always associated with label *IN* (i.e. *IN* is always associated with "↓") and the observation of discordant pairs is always associated with label *AC* (i.e. *AC* is always associated with "↓"). Therefore, *no signal* is observed with commutable observables; only background is associated with both *IN* and *AC* labels.

This shows that non-commutable observables are necessary not only for high rates of concordant pairs, but also for signal emergence. Note also that the signal must be one of the possible states of the system, even one with a low probability. In other words, the signal must be present in the

TABLE 5
Summary of the Quantum-Like Model Describing Benveniste's Experiments

	Non-Commutable Observables ($\theta \neq 0$)		Commutable Observables ($\theta = 0$)
	With Interference Term (Superposition)	Without Interference Term (No Superposition)	
Presence of signal	Yes [a]	Yes [b]	No [c]
Concordance of labels and outcomes [d]	High [e]	Low [f]	NA
Probability of concordant pairs: P(A_{CP})	$\lvert\lambda_1 \cos\theta + \lambda_2 \sin\theta\rvert^2$	$\lambda_1^2 \cos^2\theta + \lambda_2^2 \sin^2\theta$	λ_1^2
Probability of discordant pairs: P(A_{DP})	$\lvert\lambda_2 \cos\theta - \lambda_1 \sin\theta\rvert^2$	$\lambda_2^2 \cos^2\theta + \lambda_1^2 \sin^2\theta$	λ_2^2
Corresponding experimental situations	Open-label or blinding by type-2 observer	Blinding by type-1 observer	Unqualified or untrained experimenter

NA, not applicable.

[a] $P_{II}(A_\uparrow) = \lambda_1^2 \times P_{II}(A_{DP}) + \lambda_2^2 \times P_{II}(A_{CP})$

[b] $P_I(A_\uparrow) = \lambda_1^2 \times P_I(A_{DP}) + \lambda_2^2 \times P_I(A_{CP})$

[c] Observables are commutable with $\cos\theta = 1$ and $\sin\theta = 0$; then $P(A_\uparrow) = 0$ and $P(A_\downarrow) = 1$ (only background is observed by A; there is no signal).

[d] Concordant pairs: A_{IN} associated with $A_\downarrow$ or A_{AC} associated with $A_\uparrow$.

[e] For $\sin\theta = \lambda_2$ (and consequently $\cos\theta = \lambda_1$), the quantum interference term is maximal with $P_{II}(A_{CP}) = 1$ and $P_{II}(A_{DP}) = 0$.

[f] For $\lambda_1^2 = \lambda_2^2 = 0.5$, concordance of pairs is not different than random (whatever θ value).

background; thanks to entanglement, the emergence of the signal is made possible.

"Which-Path" Measurement and Contextuality in Benveniste's Experiments

In the proposed formalism, there is neither success nor failure of the experiments (Table 5). Simply, as in a single-particle interference experiment, we can decide to observe either "waves" or "particles" by modifying the setting of the experiment. In the two-slit experiment of Young, observing "waves" (interference pattern on the screen) is not considered as a success whereas observing "particles" (no interference pattern after which-path measurement) is not considered as a failure.

In Benveniste's experiments, the decision to observe "particles" (concordant pairs plus discordant pairs) or "waves" (only concordant pairs) is related to the design of the experiment (Figure 3). If the "cognitive state" of the experimenter is able to interfere with itself (as a single particle interferes with itself), then the probability of "success" is high. In case of blinding by a type-2 observer, quantum probabilities also apply since the respective cognitive states of the experimenter A and of the type-2 observer O are on the same branch of reality (as Wigner's friend observing Schrödinger's cat). Therefore, there is no formal difference for open-label vs. blinding by a type-2 observer. The same outcomes are obtained since the state vector that describes their cognitive states is:

$$|\Psi_{AO}\rangle = (\lambda_1 \cos\theta + \lambda_2 \sin\theta)\, |A_{CP}\rangle|O_{CP}\rangle + (\lambda_2 \cos\theta - \lambda_1 \sin\theta)\, |A_{DP}\rangle|O_{DP}\rangle$$

If the sample blinding is performed by a type-1 observer, then conditional classical probabilities that take into account the "which path" information apply. In this case, the cognitive state of the experimenter cannot interfere with itself (there is no superposition). When the experimenter and the type-1 observer meet together after a series of blind experiments, they assess the rate of concordant pairs and they both agree that the probability of concordant pairs is low. We have to insist that, even with blinding by a type-1 observer, the signal is present if $\sin\theta \neq 0$.

Cognitive Aspect of the Formalism

The concordance of pairs is optimal for $\cos\theta = \lambda_1$ and $\sin\theta = \lambda_2$; indeed, in this case,

$$P_{II}(A_{CP}) = |\lambda_1 \cos\theta + \lambda_2 \sin\theta|^2 = 1 \quad \text{(Table 5).}$$

The probabilities of concordant pairs were 0.88 and 0.92 for open-label experiments and blind experiments with a type-2 observer, respectively (Table 4). This should not surprise us; it simply indicates that correlations in "real" experiments were not optimal and probabilities of concordant pairs were slightly <1.

Moreover, in a cognitive context, the fact that optimal concordance of pairs is observed when $\cos\theta = \lambda_1$ and $\sin\theta = \lambda_2$ is of particular interest. Indeed, the λ parameters (probability for labels *IN* or *AC*) are related to the experimental protocol, which defines the proportions of labels *IN* and *AC*. In contrast, the angle θ characterizes the relationship between the observables, which become noncommutable if θ is different from zero (see the section **The Quantum-Like Formalism Applied to Benveniste's Experiments**). The probability for the experimenter to observe a high rate of concordant pairs is related to modification of its cognitive state described by the state vector in Hilbert space and summarized by changes of the angle θ. Therefore, it is tempting to link up the angle θ to a previous training and to information on experimental protocol.

It could be suggested that θ fluctuates randomly around zero; the more and more "favorable" values of θ would be progressively selected ("learned") by feedback according to the observed outcomes. In the Mach-Zehnder apparatus, this is equivalent to adjusting settings (e.g., fine-tuning for equal lengths of paths R and T) based on trial and error in order to get all photons in the detector D1 (all photons in phase) (Figure 2).

In summary, we propose that the outcomes of Benveniste's experiments were related to cognitive processes (i.e. establishment of relations between different cognitive states) and that the successive experimenters on Benveniste's team acquired skill by manipulating the biological systems and measurement devices (for example, by performing "classical" experiments).

Note also that a relatively large variation of $\sin^2\theta$ around λ_2^2 leads to "good" results with a high rate of concordant pairs observed by A (Figure 5). Thus, with λ_2^2 set at 0.35, values of $\sin^2\theta$ from 0.10 to 0.75 lead to $P_{II}(A_{CP}) > 0.90$.

Relevance of Quantum-Like Formalism for Describing Macroscopic Events

The conceptual framework of quantum theory is the logical consequence of some simple assumptions. Among them, the assumption of non-commutable observables plays a central role. In this framework, classical probabilities are only a special case of quantum probabilities, one for which all observables commute with each other. Contextuality is another central concept in quantum physics. Thus, according to the experimental device set up by the experimenter, a quantum object could appear as a particle or as a wave: With the use of a two-slit device (or a Mach-Zehnder apparatus), the decision to observe—or not—which path entered the quantum object has a chief consequence on the experiment outcome.

As we have seen, contextuality also had important consequences in Benveniste's experiments: The circumstances of blinding appeared to have crucial consequences. Since interest was focused on the local properties of water (the so-called "memory of water"), little attention was paid to the logical aspects of the experiments. Therefore, the different outcomes according to conditions of blinding were interpreted as difficulties in reproducibility related to "contaminations," "electromagnetic interferences," or other ad hoc explanations.

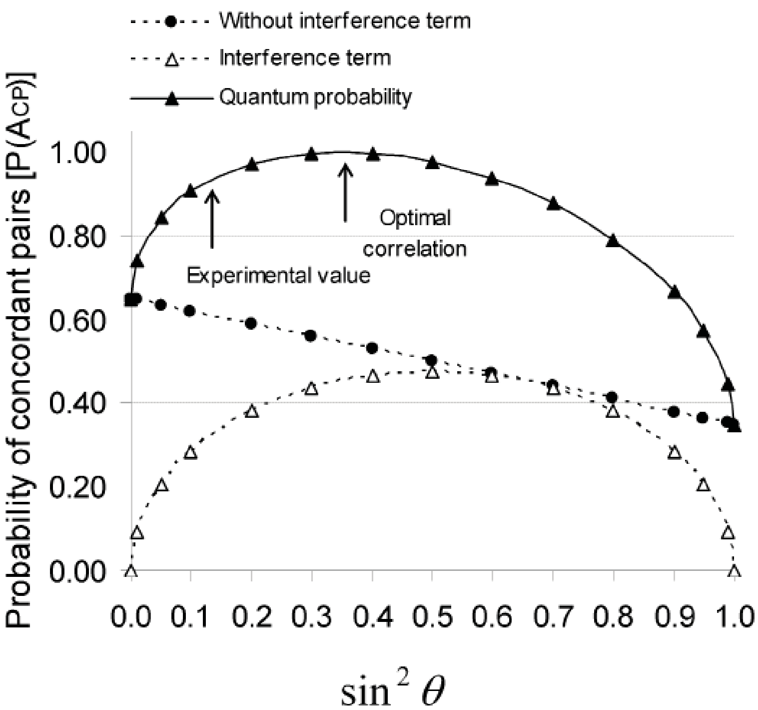

Figure 5. Probability of observing concordant pairs (*IN* with ↓ or *AC* with ↑) as a function of $\sin^2\theta$ (in this case λ_1^2 = 0.65 and λ_2^2 = 0.35).
Optimal theoretical value for probability of concordant pairs [$P(A_{CP}) = 1$] is obtained for $\sin^2\theta = \lambda_2^2$ (here for $\sin^2\theta = 0.35$; experimental value for $P(A_{CP})$ was 0.92 for $\sin^2\theta = 0.12$).

Opposite to this interpretation, we suggested that "successes" and "failures" during these puzzling experiments were the two faces of the same coin. The price to pay for this interpretation was to give up the idea that some modification in the water structure ("memory") was the cause of the biological outcomes observed with "high dilutions" or "digital biology." Note, however, that no convincing and reproducible physical modification of water structure able to induce specific biological phenomena has ever been reported; therefore, the price is not so high.

Faced with the description of Benveniste's experiments using quantum probabilities, different approaches are possible. It could be argued that the application of quantum concepts to these experiments is only metaphorical and that the analogy is simply ad hoc. Another approach—quite the opposite—is in the spirit of strict physicalism where everything can be reduced to physics. Since nothing can be left outside the field of physics, phenomena with a formal quantum description—as those described in this article—are thus quantum phenomena. However, a general acceptance of such an interpretation is generally hampered by the idea that the environment would rapidly destroy macroscopic superpositions.

A third way is possible, as suggested recently by some authors in different research areas. These authors proposed describing some specific parts of the world, whether physical or nonphysical, with a formalism isomorphic to that of standard quantum physics. This can be total isomorphism or more likely partial isomorphism, so that only certain special features of the quantum formalism are used for probability calculation of outcomes. A quantum-like formalism has thus been applied to human memory, information retrieval, decision making, opinion forming, personality psychology, etc. (Busemeyer, Wang, & Townsend 2006, Khrennikov 2006, 2009, Mogiliansky, Zamir, & Zwirn 2009, Pothos & Busemeyer 2009). These research areas have in common the description of cognition mechanisms and information processing in the brain, but this new approach does not rest on the hypothesis that there is something quantum mechanical about the physical brain. The quantum formalism is simply used as a source of alternative new tools (such as contextuality or entanglement) to address problems that remained unresolved in a classical framework. In these studies, the cognitive states of agents were characterized by state vectors in Hilbert space, and, in several experimental models, quantum probabilities had better predictive power than classical probabilities. Thus, some "paradoxical" statistical data, particularly in psychology and cognitive sciences, could be modeled (Atmanspacher, Filk, & Romer 2004, Conte, Todarello, Federici, Vitiello, Lopane, & Khrennikov, 2004, Khrennikov & Haven 2009, Mogiliansky, Zamir, & Zwirn 2009, Pothos & Busemeyer 2009).

In our model, the observables are nonphysical and therefore are not supposed to be exposed to the decoherence process. The first observable is labels, which have the meaning that the experimenter decides (all samples are physically equivalent). The other observable, pair concordance, also requires information processing for "interpretation." The cognitive process that we describe is not a causal action on the physical world, but it allows changing the "point of view" of the experimenter/observer, which is plunged into the world of possibilities described in the Hilbert space.

Finally, it has not escaped our notice that the present interpretation of Benveniste's experiments and the associated mathematical formalism that we propose could be extended to other experimental situations where an apparent "causal" relationship depends on contextual parameters.

Conclusion

The outcomes of the cognitive state of the experimenter were calculated for a series of Benveniste's experiments using a quantum-like statistical model (i.e. a model inspired by quantum physics and taking into consideration superposition of quantum states, non-commutable observables, and contextual-ity). Not only were the probabilities of "success" and "failure" of the experiments modeled according to their context, but the emergence of a signal from background also was taken into account. For the first time, a formal framework devoid of any reference to "memory of water" or "digital biology" describes all the characteristics of these disputed results. Particularly, the difficulties encountered by Benveniste (reproducibility of the experiments, disturbances after blinding) are simply explained in this model without additional ad hoc hypotheses. It is thus proposed that we see Benveniste's experiments as the result of quantum-like probability interferences of cognitive states.

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RESEARCH ARTICLE

Replication Attempt: Measuring Water Conductivity with Polarized Electrodes

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Abstract—We attempted to reproduce the results of experiments related to measuring the conductivity of water with deeply polarized electrodes. As proposed in the original works, the polarized electrodes are sensitive to a high-penetrating emission generated by objects of different origin. We demonstrate the experiment setup used and the results obtained in replication and control experiments. Based on the trials carried out, we judge the results of this replication to be positive.

Introduction

This study is based on previous research related to underwater communication by means of electric fields. This approach is inspired by weakly electric fish (von der Emde, Schwarz, Gomez, Budelli, & Grant 1998, Sim & Kim, 2011) that use different features of electric fields for navigation, sensing, and the coordination of collective activities. The equipment for the generation and sensing of electric fields is installed on small mobile underwater devices (Kernbach, Dipper, & Sutantyo 2011, Dipper, Gebhardt, Kernbach, & von der Emde 2011). The fields produced by different devices interact with each other and provide an account of the global properties of the underwater environment (Schmickl, Thenius, Moslinger, Timmis, Tyrell, et al., 2011). In several experiments, the modulation frequency of the electric field is very low (in the range of 0.01 to 0.001 Hz), which creates deeply polarized electrodes.

In carrying out these experiments on communication via electric fields, we noted two interesting effects. First, the results obtained are highly reproducible for relative values within one experiment. However, in the cases of deeply polarized electrodes, the results vary among experiments. The main factors identified, which influenced many of the results, included sensitivity to mechanical vibrations, the emissions of blue-light LEDs (used for navigation purposes), and the duration of the experiment.

Several works report sensitivity in polarized electrodes to laser and LED light, and ultrasonic waves (Bobrov 2006, 1998). These works are denoted as lying within the field of research related to "non-electromagnetic" (non-EM) fields. Despite controversial discussions, we also make use of this notion, because the original papers introduced it to explain the effects discovered in the electric double layer (EDL) (Muzalewskay & Bobrov 1988). The diffusion Gouy–Chapman layer in EDL is sensitive to, among other things, a spatial polarization of water dipoles, e.g., Lyklema (2005) and Belaya, Feigel'man, and Levadnyii (1987). Eliminating such factors as variation of temperature, EM fields, or vibrations, the authors Muzalewskay and Bobrov (1988) demonstrated that some active or passive objects can change the dielectric properties of EDL. These changes are detectable by measuring the current flowing through the water–electrode system. As noted in existing research, for instance in Bobrov (2006), experiments are carried out not only with non-biological but also with biological objects such as seeds or bacteria (Bobrov 1992). Thus, the deeply polarized electrodes in water might represent a detector, which is sensitive to possible non-EM fields.

In particular, we are interested in the following experiment: polarized electrodes in a small container with water, representing a detector. Several such detectors were placed inside a metal box, protected from EM fields and temperature changes. Electronic equipment measured the conductivity of water in each of the detectors and recorded its dynamics. An LED generator was prepared, consisting of 128 yellow-light super-bright LEDs. Another container of water was irradiated by this LED generator (Bobrov 2002). As stated in Bobrov (2009, 2006), the detectors demonstrated different dynamics in the presence of irradiated water, normal water, and control experiments. In other words, the impact of non-EM fields from the LED generator is measurable not only directly but also indirectly through irradiated water. Since mechanical, acoustic, optical, capacitive, temperature, and EM influences were excluded from these experiments, the polarization of water dipoles by the LED generator created a number of deep scientific questions related to the nature of this interaction.

We decided to replicate this experiment in the context of our research. Primarily, the goal was not only to confirm or refute the results of the experiment above, but also to estimate the value of a possible non-EM component and its use in the context of underwater communication. We changed the conditions of the experiments and compared the dynamics of the water conductivity (current flowing though the water at a constant voltage) in the presence of (a) an active LED generator, (b) water irradiated by the LED generator, (c) normal water, and (d) control experiments. Comparing the dynamics of (a) and (b) to (d) could provide an account of a possible non-EM field and comparing (b) to (c) an account of the degree of spatial polarization of the water dipoles. Since conductivity is measured by a small current, we paid close attention to technical issues of accurate measurement and experimental reproducibility.

This article is structured as follows: The **Methodology** section describes the methodology and measurement approach used. The experiment setup is described in **Appendix A**. We performed three experiment series: series "A"—calibration and preliminary experiments, as described in the section **Characterization of Sensors: Impact of Temperature, Vibration, and EM fields**; series "C"—measuring the conductivity of water under the influence of the LED generator and irradiated water, as described in the section **Experiment Series C**. Additionally, in series "B" we measured the conductivity of water related to non-EM fields of biological origin (as described for example in Bobrov (2006)); however, these experiments are excluded from this work. Finally, in the sections **Discussion of Results** and **Conclusion** we generalize from the experiments carried out and conclude this paper.

Methodology

The electric double layer (EDL) appears on the surface of an object placed into a liquid. Electrokinetic phenomena are described by the Gouy–Chapman–Stern model (Lyklema 2005). Corresponding to this model, EDL can be represented by two layers: the internal Helmholtz (absorption) layer and the outer Gouy–Chapman (diffuse) layer (Kornyshev 2007). As mentioned in Bobrov (2006), the diffuse layer is of interest. In a number of works, e.g., Stenschke (1985), Gruen and Marcelja (1983), and Belaya, Feigel'man, and Levadnyii (1987), dielectric behavior and properties of the Gouy–Chapman layer are investigated. In particular, the dielectric response of this layer depends on among other factors the temperature, ionic concentration, and spatial polarization of water dipoles. As proposed in the original works, e.g., Bobrov (2009), and confirmed by a large number of different experiments, some non-biological as well as biological objects are capable of influencing the spatial polarization of dipoles and thus change dielectric properties of the Gouy–Chapman layer. Despite the fact that the principles of such an influence are not definitively identified at the moment, the produced effects appeared in changing an electric current flowing through the water–electrode system and thus can be experimentally measured. The main methodology of those experiments consisted of removing such factors as variation of temperature and EM fields, acoustic impacts, and vibrations from influence on the results. For statistical analysis the measurements are done by several sensors in parallel and repeated to achieve statistical significance. This methodology is also adapted for our experiments.

We developed our own sensors by following the state of the art in conductometry. Conductometric analysis is a well-known approach that measures the conductivity of water. There are several different methods, using two or four electrodes, see Kirkham and Taylor (1949) or more recently Bristow, Kluitenberg, Goding, and Fitzgerald (2001), as well as inductive approaches. Generally, the results of measurements are influenced by (Orion Conductivity Theory no date):

- polarization of electrodes, that is appearance of EDL (Lyklema 2005);
- temperature;
- fringing effect of the electric field (Parker 2002);
- technical reasons, such as noise from the voltage generator, resistance of cables and connectors;
- contamination of electrode surfaces.

In the vast literature, the process leading to an appearance of EDL is denoted as polarization of electrodes (Lyklema 2005). For conductometric purposes, the electrode polarization leads to a measurement error and therefore is undesirable. To minimize this error, the conductometry with two and four electrodes is performed with an AC voltage of up to 10 kHz frequency, see for example Spillner (1957). When using EDL as a sensor, the polarization of electrodes is required and takes about 6–8 hrs. To underline the difference from a normal conductometric analysis, such electrodes are denoted as deeply polarized electrodes.

For our experiments, we prepared and used five setups, as described in **Appendix A** (see four setups in Figure 19). The difference between them lies in the material, placement, and number of electrodes. In the following, we denote each set of electrodes in containers with water as sensors. Three identical sensors are collected into one setup, controlled by one microcontroller. For experiments, we used setups 3, 4, and 5 with nine sensors in total. To counter the influence of EM fields and parasitic couplings, the water containers with electrodes were inserted into several grounded metal boxes lined with rubber matting and wool (see Figure 1). Finally, detectors and the container with irradiated water were placed into a closed metal cupboard (the LED generator was placed outside the cupboard). The purpose of such multiple EM and temperature shields is to minimize the impact of temperature variation and environmental EM fields.

All experiments were performed in two laboratories: the normal electronic laboratory on the second floor of a university building (denoted from now on as laboratory "A") and a laboratory placed in the basement

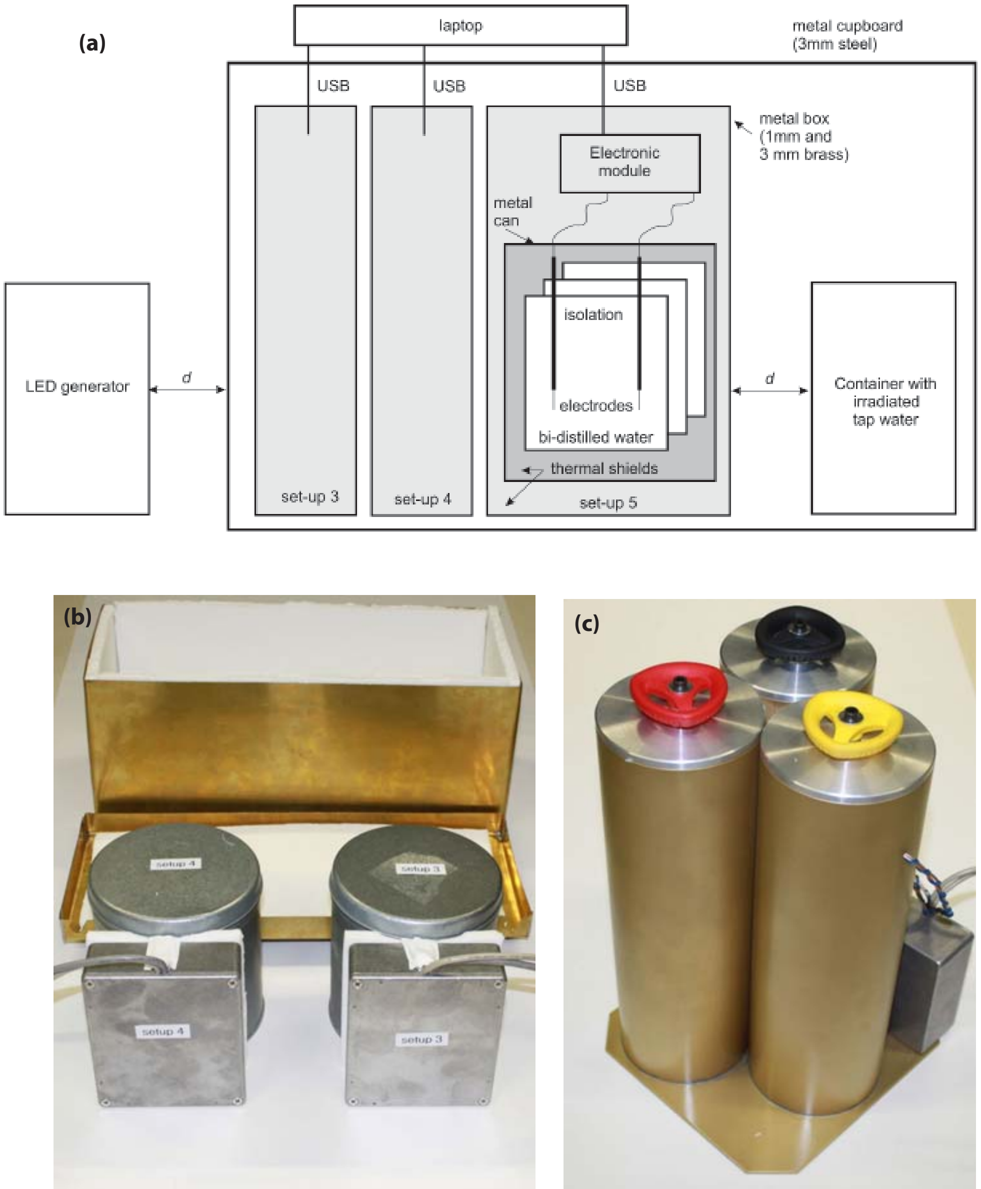


Figure 1. Experiment setup.
(a) General structure of the experimental setup
(b) Setups 3 and 4, each with three sensors in metal cans (with rubber matting and wool inside) and a metal box made of 1 mm brass
(c) Setup 5. Each 3 mm brass pipe has one sensor (see Appendix A for more detail)

of this building (with thick concrete walls without windows—denoted as laboratory "B"). For both laboratories, we measured spectra of EM fields and acoustic waves when the LED generator was switched off/on. Before the start of experiment, all detectors are characterized by their reaction to vibration, changes of temperature, and EM fields, as described in the next section **Characterization of Sensors: Impact of Temperature, Vibration, and EM Fields**.

The experiments were organized in the following way. All sensors ran one week and continuously recorded the current (from the 2× electrode scheme, all setups), voltage (from the 4× electrode scheme, only setup 3), temperature, vibrations, and the level of analog and digital power supply (to measure noise from a power supply). During weekends, the received data were archived and the data-collecting program on the laptop started anew. During the experiment, either the LED generator or a container with non-irradiated or irradiated water (the terminology of the original work) was placed in front of the detector, at distance *d*. As suggested in Bobrov (1992, 2006), the water was "irradiated" by turning the LED generator on for 5–30 minutes (90 seconds in the original experiments). To minimize the influence of the operator on the detector, the LED generator was autonomously turned on/off by a microcontroller at such time when nobody was present in the laboratory. In cases when this was not possible, for example when replacing water containers, an operator quickly left the laboratory after necessary manipulations.

As mentioned in the next section **Characterization of Sensors: Impact of Temperature, Vibration, and EM Fields** and shown during the preliminary experiments, the sensors are not sensitive all the time. Moreover, it is not possible to predict when a sensor will lose its sensitivity. Thus, we decided to use multiple sensors to record a single experiment in parallel. Each sensor was counted as a single trial, which can be positive or negative. The experiment was positive when at least two sensors demonstrated a positive causal reaction (that is, within the time of the experiment). We counted a number of trials and a number of independent experiments. Normally, the experiments were performed in the morning, because the sensors relaxed during the night hours and there was low environmental noise. However, if we observed high environmental noise in the two hours before the experiment, we postponed the experiment to the next day. Thus, we can perform, on average, only about three experiments a week.

We observed three typical reactions in the sensors. One, the value of the current rapidly jumps from one level to another, as shown in Figure 13. This is a typical kind of behavior observed when the sensor is in a stationary state. For this type of reaction, labeled as "T1", we measured the amplitude

TABLE 1
Parameters of Experiment C

N	Parameter	Description
1	Type of electrodes	Setup 3: 4-electrode scheme, chromium–stainless steel, 1 mm diameter (replaceable); Setup 4: 2-electrode scheme, first electrode chromium–stainless steel, 1 mm diameter (replaceable); Setup 5: 2-electrode scheme, platinum, 1 mm diameter (non-replaceable)
2	Distance between electrodes	10 mm–60 mm
3	Voltage level	DC, 0.9 V–4 V, changing of polarity is possible, noise level $\pm$10 mV
4	Current level in DAC circuit	3 μA–40 μA
5	EM (radio frequency and 50/60 Hz) and optical shield	All electrodes/electronics are placed inside several grounded metal boxes made of steel/brass. See Ott (1988) for more detail on EM production.
6	Temperature shield	Foam rubber and wool in each metal box
7	Elimination of parasitic DC couplings	Power via USB from a laptop, laptop in battery mode (in control experiments), LED generator powered by D-size batteries
8	Water used in sensors	Purified by osmosis (before Experiment C160) and bi-distilled (after Experiment C160), 50–150 ml in glass (setups 3 and 5) and stainless steel (setup 4) containers
9	Water used for irradiation	Normal tap water, rested for 7–24 hours before the irradiation, 500 ml in glass container
10	Water used in control experiments	Normal tap water, rested for 7–120 hours before the experiments, 500 ml in glass container
11	Type of sensor's reaction	Type 1, type 2, type 3
12	Exposure time of LED generator	20–40 minutes by 169 blue-light (470 nm), 11 cd LEDs
13	LED mode used in experiments	Oscillations 1 and 2, rotation CCW and CW
14	Exposure time of irradiated water	30–80 minutes
15	Duration of irradiation of water	5–30 minutes
16	Distance between detector and LED generator / irradiated water	5–13, 30 cm
17	Time between irradiation of water and start of experiment	Immediately before, to 72 hours before
18	Number of sensors recording in parallel	3, 6, 9

of the current changes over the average current for each of the sensors and wrote down only these values (without the label "T1") as shown in Tables 2, 3, and 4.

When current continuously increases or decreases (this behavior can take several days), the sensor either does not react at all, or changes its inclination (see Figure 12 and Figure 14). We did not observe a rapid change of current; this behavior is labeled as type 2 (or "T2"). Finally, when the current oscillates, it changes the amplitude or frequency of the modulation. This is the type 3 reaction, labeled as "T3". When the change of current was significant, we noted this change as well. The different parameters of the experiments performed are collected in Table 1.

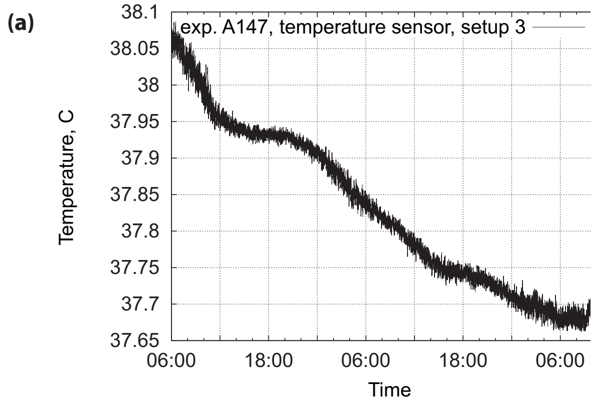
Characterization of Sensors: Impact of Temperature, Vibration, and EM Fields

Variation of Temperature

Despite thermal shields, it is impossible to maintain a constant temperature during experiments because of self-heating of electronic components and environmental changes. Thus, the temperature impact can represent an important factor influencing the results. To characterize the reaction of the sensors to temperature, we performed several measurements. The main methodology was to find a combination of temperature-isolating materials, the distance d, and the parameters of the LED generator (e.g., the voltage applied to LEDs) to observe a non-proportional or delayed response of temperature sensors in relation to a response of current sensors (see cases (2), (3), and (4) below).

(1) Control measurement. To characterize a non-influenced behavior of sensors, we performed the control measurement over 50 hours in laboratory B, as shown in Figure 2. The total variation of temperature was about 0.4 C; we observed a slowly increasing current $\Delta I = 0.5$ μA, which follows the changes of temperature. Thus $\Delta t = 0.1$ caused a change of current $\Delta I = 0.125$ μA in a long-term, slowly changing dynamic. However, this relation was nonlinear and depended on the previous dynamic (e.g., increasing or decreasing).

(2) Delayed response of temperature sensors. Laboratory A had a larger variation of temperature than laboratory B; this represents the worst-case dynamics of the current. In experiment C130 (see Figure 3), the temperature change in region I (3.5 hours, 3:30–7:00) was about 0.25 C, in region II about 0.025 C (1.5 hours, 7:00–8:30), in region III about 0.015 C (one hour, 8:40–9:40 when the LED generator was turned on), and about 0.22 C after the experiment in region IV (2 hours, 10:00–12:00).



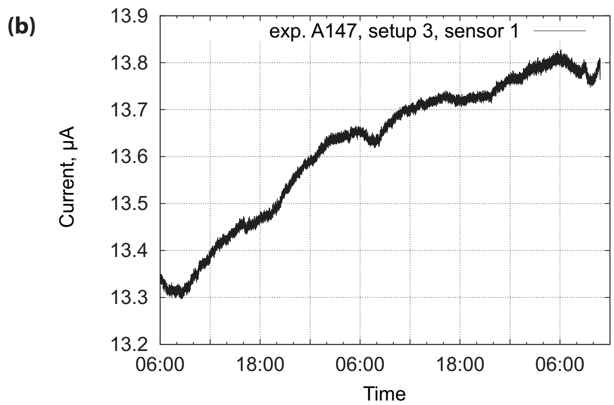
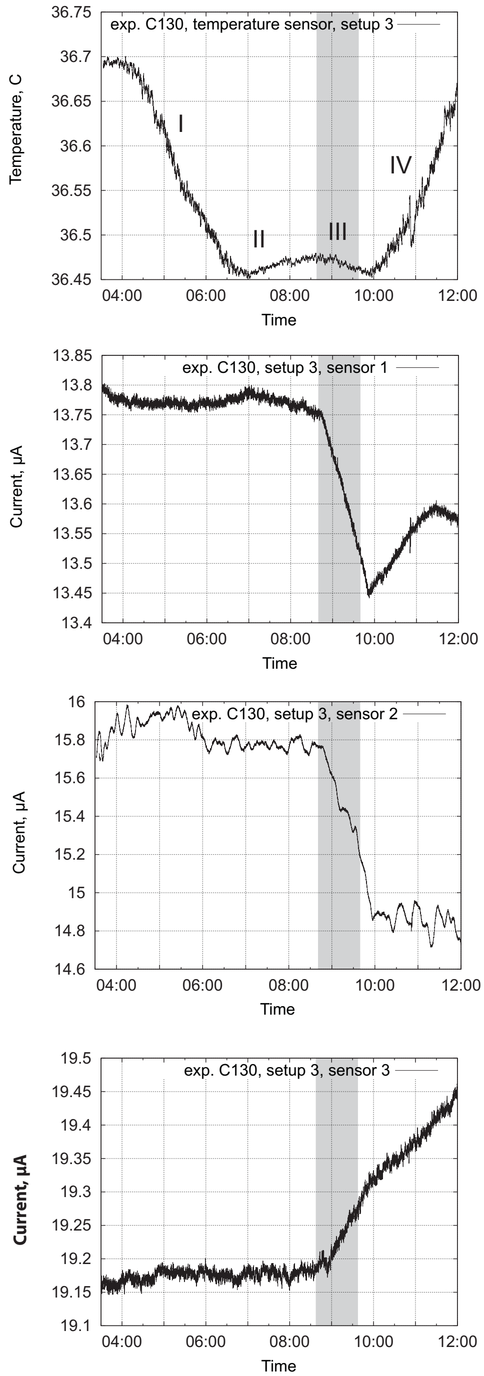


Figure 2. Control measurement taken over 50 hours in laboratory B.
(a) Setup 3, temperature sensor
(b) Setup 3, sensor 1 (all other sensors demonstrated similar dynamics)

Temperature changes during I–III were caused by the environment, and during IV mostly by heat from the LED generator. Corresponding changes of current during I and II were $\Delta I_1 = 0.05$ μA, $\Delta I_2 = 0.2$ μA, $\Delta I_3 = 0.03$ μA for all three sensors over 5 hours. Changes of current for region III were $\Delta I_1 = 0.15$ μA, $\Delta I_2 = 0.4$ μA, $\Delta I_3 = 0.08$ μA over 1 hour. Behavior in region IV was strongly influenced by the LED generator and was rather different for all three sensors. Thus, thermal and LED generator changes of current were quantitatively and qualifiedly different. Moreover, due to thermal shields, the heat from the LED generator reached the sensors 20 minutes after the LED generator was turned off.

(3) Comparisons of thermal impacts. In experiment C166 the LED

Figure 3. Impact of temperature on the sensors in laboratory A. (Gray area) represents the time when the LED generator was switched on. Variation of temperature 90 minutes before the experiment is about 0.025 C, and during the experiment 0.015 C. To demonstrate the delayed dynamics of temperature and make them more visible, we plot output from all three sensors.



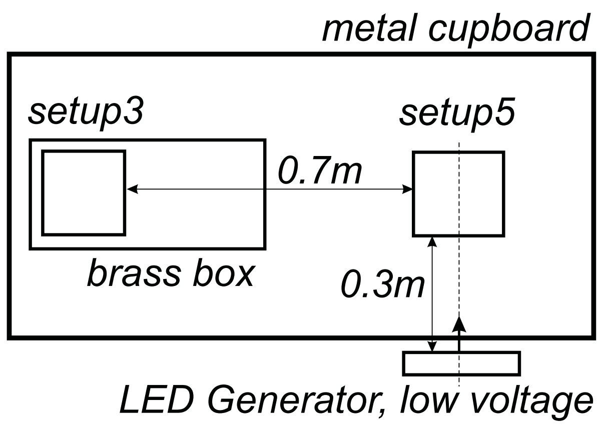


Figure 4. Scheme of experiments C166 and C167.

generator was powered by a low voltage of 2.5 V to minimize the self-heating and installed in front of setup 5 (outside the metal cupboard) at a distance of 30 cm. Setups 3 and 4 in a brass box were placed to the left so that the distance between setup 5 and setup 3 was about 70 cm (see Figure 4). In both setups Δt = 0.08 *C*; however, we observed a delayed increase in temperature in setup 3 due to additional thermal shields (this also resulted in a 2.5 C higher temperature inside the brass box) (see Figure 5). The current dynamics in each setup was different. In setup 5, which was placed on the main axis of the LED generator, we observed a fast increase of current and also a fast decay after the experiment. However, the current in setup 3 was slowly increasing following the changes of temperature. It was similar to the non-perturbed dynamics, shown in Figure 2. We repeated this experiment two days later in experiment C167 with similar results. Thus the dynamics of current in the LED generator and outside are completely different despite the fact that the changes in temperature were the same.

(4) Non-proportional (with respect to temperature) growth of current. In experiment A165 (see Figure 6), before the experiment we created a constant increase of environmental temperature of about Δt = 0.02 C every 2 hours. The corresponding constant change of current was about ΔI = 0.01 μA. During the experiment, the low-power LED generator additionally changed the temperature about Δt = 0.01 C, which however follows the previous trend of increasing the temperature. Thus, in total we did not observe any essential fluctuations of temperature in this experiment. Even so, the corresponding change of current was about ΔI = 0.09 μA. Thus, there was no essential fluctuation of temperature; however, there was an essential

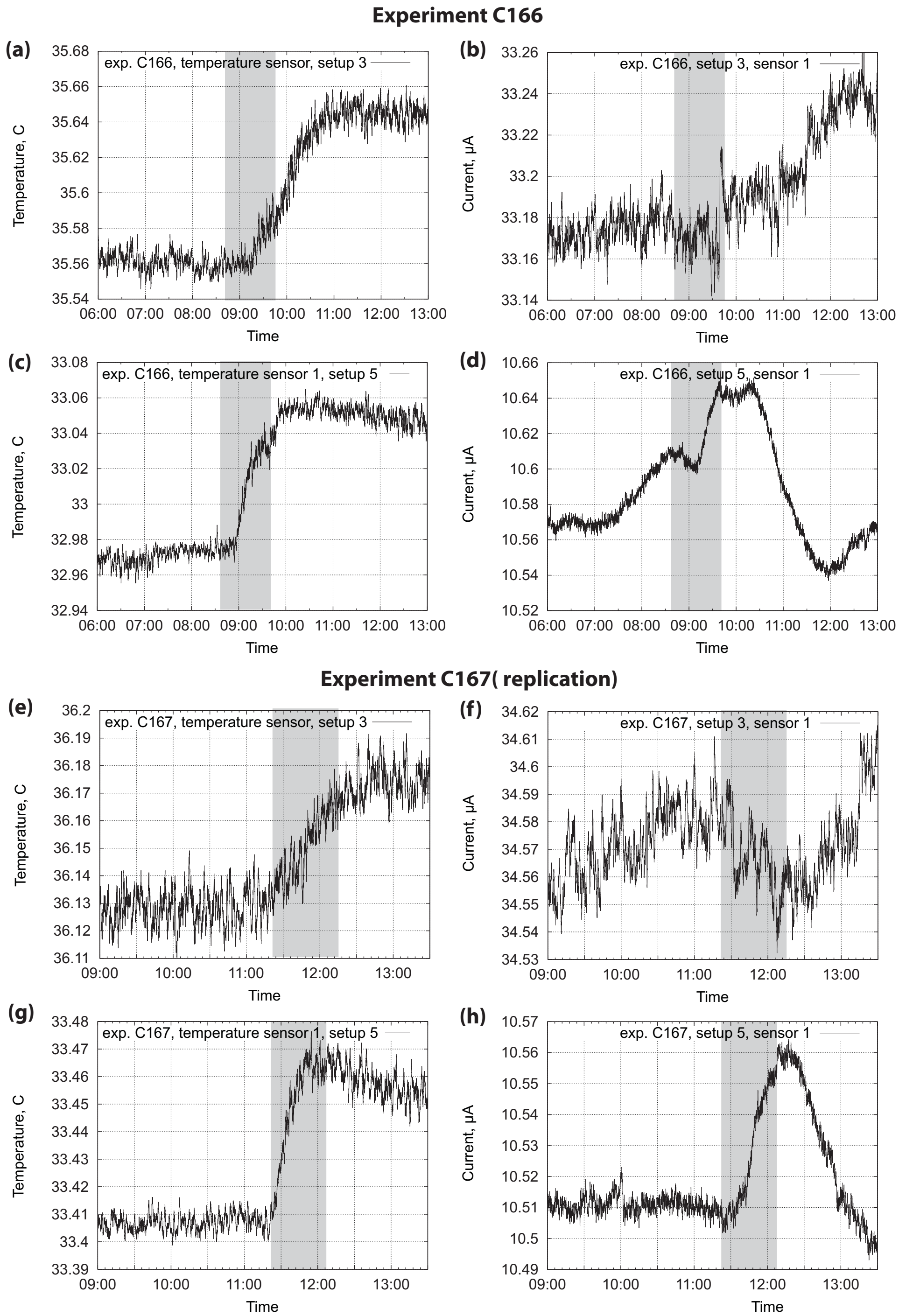


Figure 5. Experiment C166 and the repeated experiment C167 in laboratory B.
(Gray area) Represents the time when the LED generator was switched on.
(a, c, e, g) Data from the temperature sensors of setups 3 and 5
(b, d, f, h) Data from the current sensors of setups 3 and 5

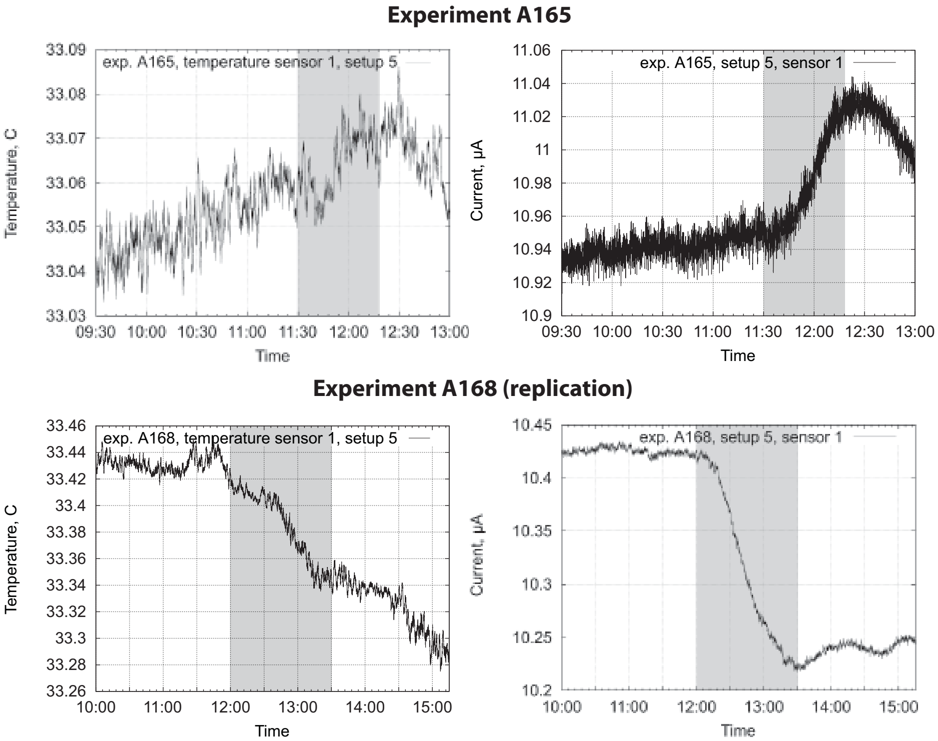


Figure 6. Impact of temperature variation on the sensors in laboratory B. (Gray area) represents the time when the LED generator was switched on. Variation of temperature 120 minutes before the experiment was about 0.02 C, and during the experiment 0.01 C.

fluctuation of current, i.e. there was a non-proportional (with respect to temperature) increase in current during the experiment. This behavior was replicated in experiment A168.

(5) Changed dynamics from irradiated water. Since containers with water do not produce any heat, a variation in environmental temperature is minimal in such experiments. For example, in experiment C162, the container with irradiated water was placed in front of the detector at 17:20 and removed at 18:00. Variation of temperature before the experiment was about 0.015 C, and during the experiment 0.03 C. The dynamics of the current was increasing, and we observed a deviation from this dynamic of 0.05 μA. The temperature started to change about 15 minutes after the beginning of the experiment. The current started to change immediately after the water container was placed close to the detector. We replicated this experiment several times, e.g., within A169 and C192 (with the recording of several other parameters) (see Figure 7).

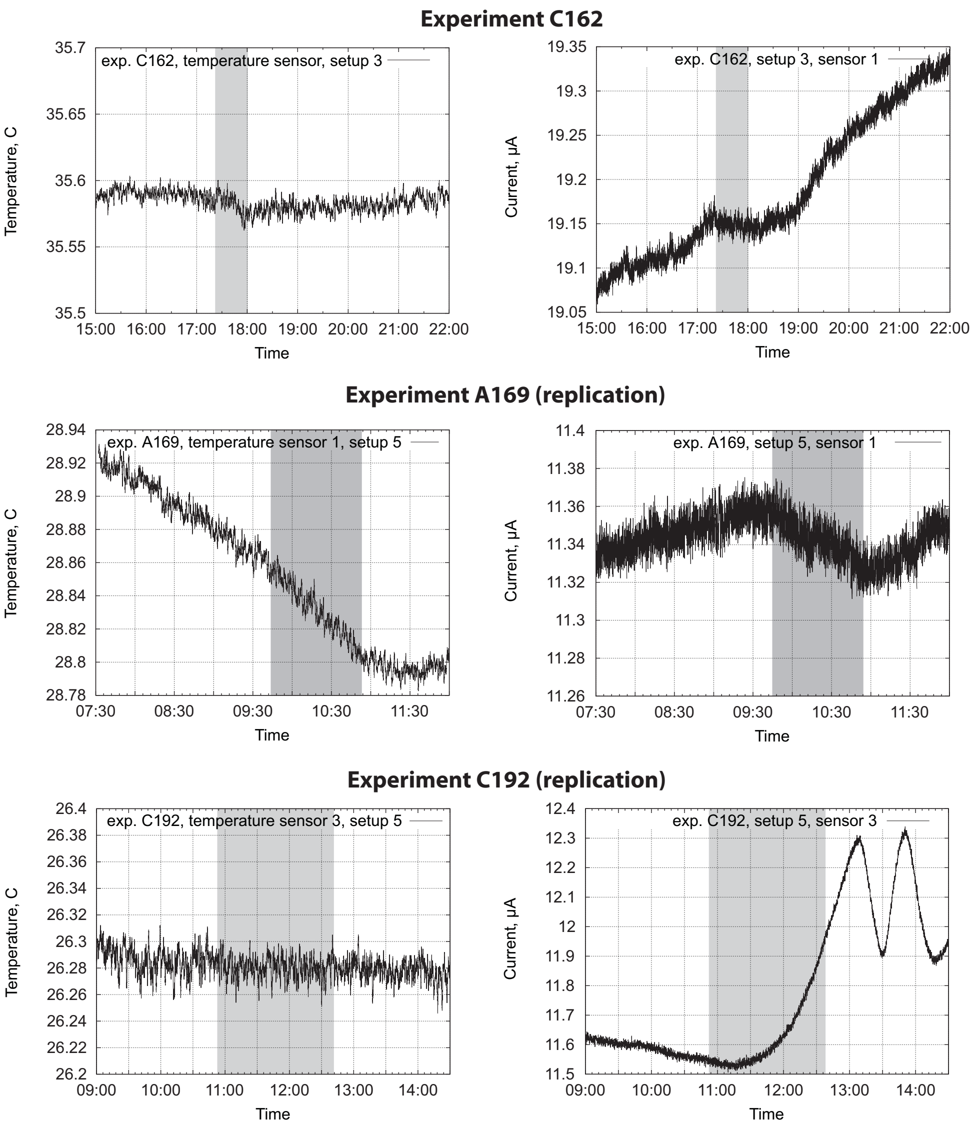


Figure 7. Impact of temperature variation on the sensors in laboratory B. (Gray area) Represents the time when the container with irradiated water was placed close to the detector.

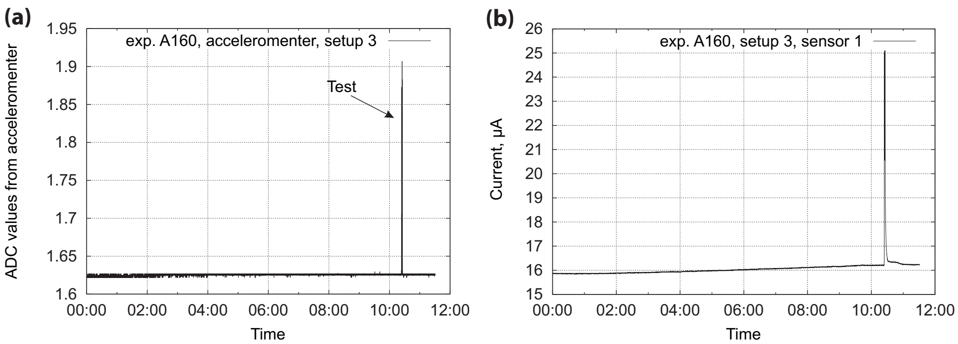


Figure 8. Impact of vibrations on the 3D accelerometer.
(a) Data from the 3D accelerometer (setup 3) recorded over experiment A160. To test the setup, we lightly touched the metal can at 10:30.
(b) Current dynamics (setup 3, sensor 1) recorded over experiment A160

Impact of Vibration, EM Fields, and Acoustic Waves on Sensors

Data from the accelerometer. To characterize the impact of vibrations and other mechanical perturbations, Figure 8 shows the data from the 3D accelerometer over 12 hours for experiment A160. To test the accelerometer, we lightly touched the metal can of the detector at 10:30; the consequent peak is readily visible. In Figure 8(b) we observe a corresponding change of current at 10:30, which was quickly normalized to a non-perturbed value. Thus, mechanical perturbations appear as readily visible peaks in the dynamics of the current and in this way qualitatively differ from the slowly changing dynamic observed in other experiments.

EM field. Measurements of the EM field were performed by the spectrum analyzer 9 KHz . . . 7 Ghz produced by Rohde & Schwarz. First we measured the spectrum of the background EM field in laboratories A and B. As is visible from Figure 9, laboratory A had frequencies occupied by WiFi, GSM, and FM radio. In laboratory B these frequencies were empty. In laboratory B we also measured the spectrum of the EM field close to the unshielded setup when: 1) the LED generator was on, and the detector was off; 2) the LED generator was on and the detector was on but the laptop was off; 3) the LED generator, the detector, and the laptop all were on (see Figure 10).

We did not discover any differences up to the level of −90 dBm, when the LED generator and the shielded detector were on or off. The unshielded detector generated a signal of −70 dBm in the area of about 40 MHz, which

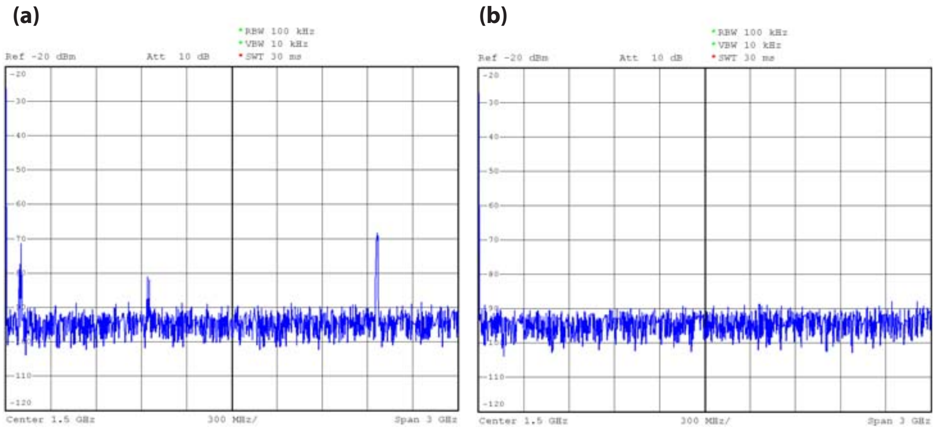
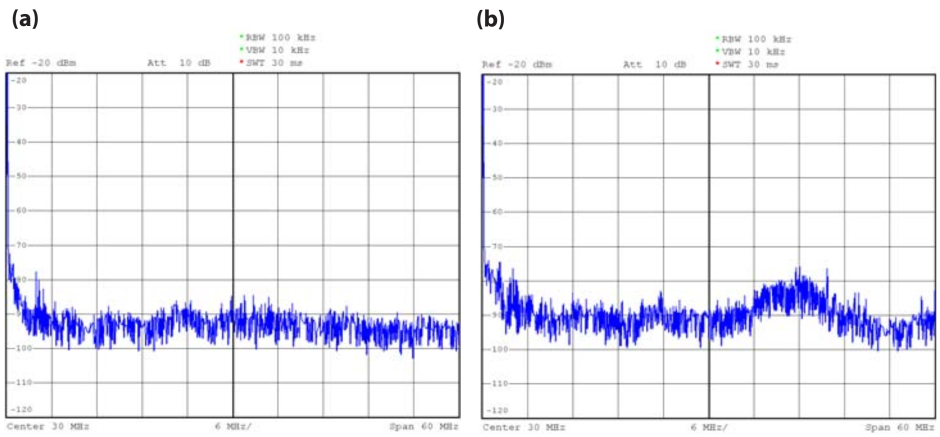


Figure 9. Spectrum of the EM field in laboratory A (a) and in laboratory B (b).

however decreased to −90 dBm when we moved the antenna 10 cm away from the detector. The laptop produced a number of different frequencies, as is visible in Figure 10(c).

Acoustic waves. The level of acoustic signals in laboratory B was measured with Metrel C-MI 6301 (20–10000 Hz, 30–130 dB) during the autonomous work of the LED generator. The signal remained below the minimal resolution of this device, i.e. <30 dB. Since no ultrasound emitters were installed in laboratory B and all surrounding laboratories, the level of acoustic waves over 20 kHz was not measured.

The main conclusion from these measurements is first of all, that the shielded LED generator and setups did not produce an EM field over −90 dBm. Thus, powering the LED generator did not influence the sensors, at least up to the level sensed by the spectrum analyzer. The laptop generated several frequencies in the area up to 100 MHz, which however remained unchanged before, during, and after experiments. Thus, the EM field produced by the laptop cannot be attributed to the changes of current during experiments. Frequencies occupied by WiFi, GSM, and FM radio are empty in laboratory B. The impact of vibration and perturbation of mechanical origin are well-distinguished by the characteristic peaks of current and by data from the 3D accelerometer. Thus, they also can be removed from consideration. We also monitored the level of voltage on the USB bus from the laptop, and no anomalous variations of the supply voltage were detected.



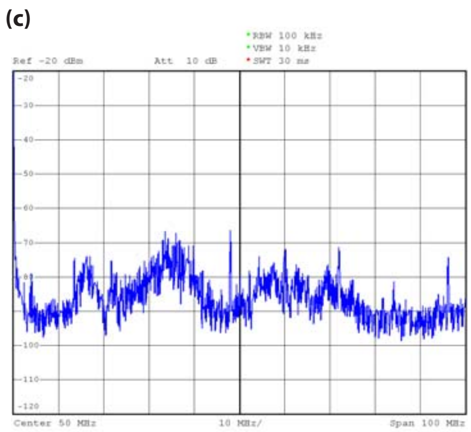


Figure 10. Spectrum of the EM field measured close to the unshielded detector and the LED generator.
(a) LED generator is on, detector is off
(b) LED generator is on, detector is on, no connection to the laptop
(c) LED generator is on, detector is on, laptop is on

Variations of temperature represent the hardest impact; they are in the best case $\Delta t = 0.01{-}0.03$ C, in the worst case $\Delta t = 0.05{-}0.2$ C. We cannot completely remove this influence from the experiments. There are several arguments for the hypothesis that changes of temperature do not represent the main factor for changes of current during the experiments. First, due to the thermal shields, we observed in many cases a delayed response of the temperature sensor—between 5 and 20 minutes after the current started to change (see Figure 3 and Figure 7). Second, we demonstrated that current increases disproportionately to temperature during several experiments (see Figure 6). Finally, after removing the influence, the dynamics of current changes its own slope or direction (from decreasing to increasing and vice versa), whereas the temperature is still increasing. This points to another factor (besides the variation in temperature) influencing the dynamics of the current.

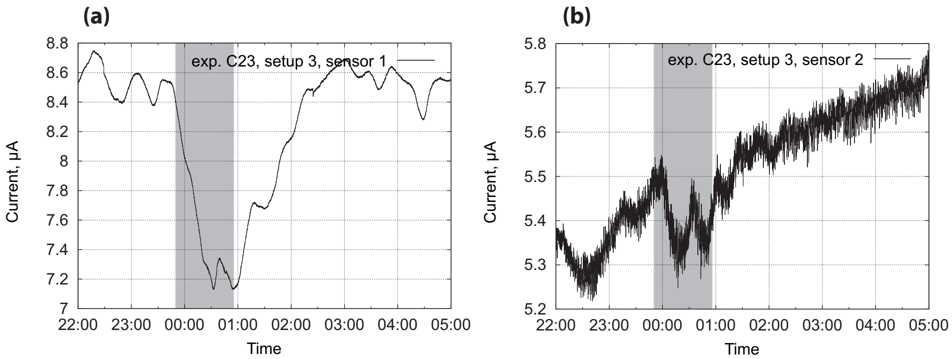


Figure 11. Experiment C23 with setup 3 of the detector and the LED generator, *d* = 15 cm, LED generator using the first waveform.
(Gray area) The LED generator was switched on between 23:47 and 00:57.
(a) Output of the first sensor
(b) Output of the second sensor

Experiment Series C

Experiments with the LED Generator

A typical run of such experiments is shown in Figure 11. Here, the LED generator was switched on for 70 minutes and we recorded the output of the sensors. It can be seen that both sensors demonstrated a type 1 reaction with a current change of 1.3 µA and 0.3 µA, respectively. Since the behavior of the current during the experiment and 2 hours before the experiment differed qualitatively, we decided the experiment was positive. Table 2 is an overview of the experiments using the LED generator. In some experiments, for example those shown in Figure 12, the current remained unchanged, but the behavior of the sensor was modulated, that is showing type 2 and type 3 dynamics.

We performed 72 trials (within 16 independent experiments; A165 and A168 are counted as trials but not as experiments) of the direct influence of the LED generator on the sensors. Fifty-eight indicated a visible change of behavior, 3 showed an appearance of T2/T3 dynamics, and in 11 trials we did not discover any changes in current during or after the LED generator was turned on. Several dynamics of the current recorded during these experiments are shown in Figure 12.

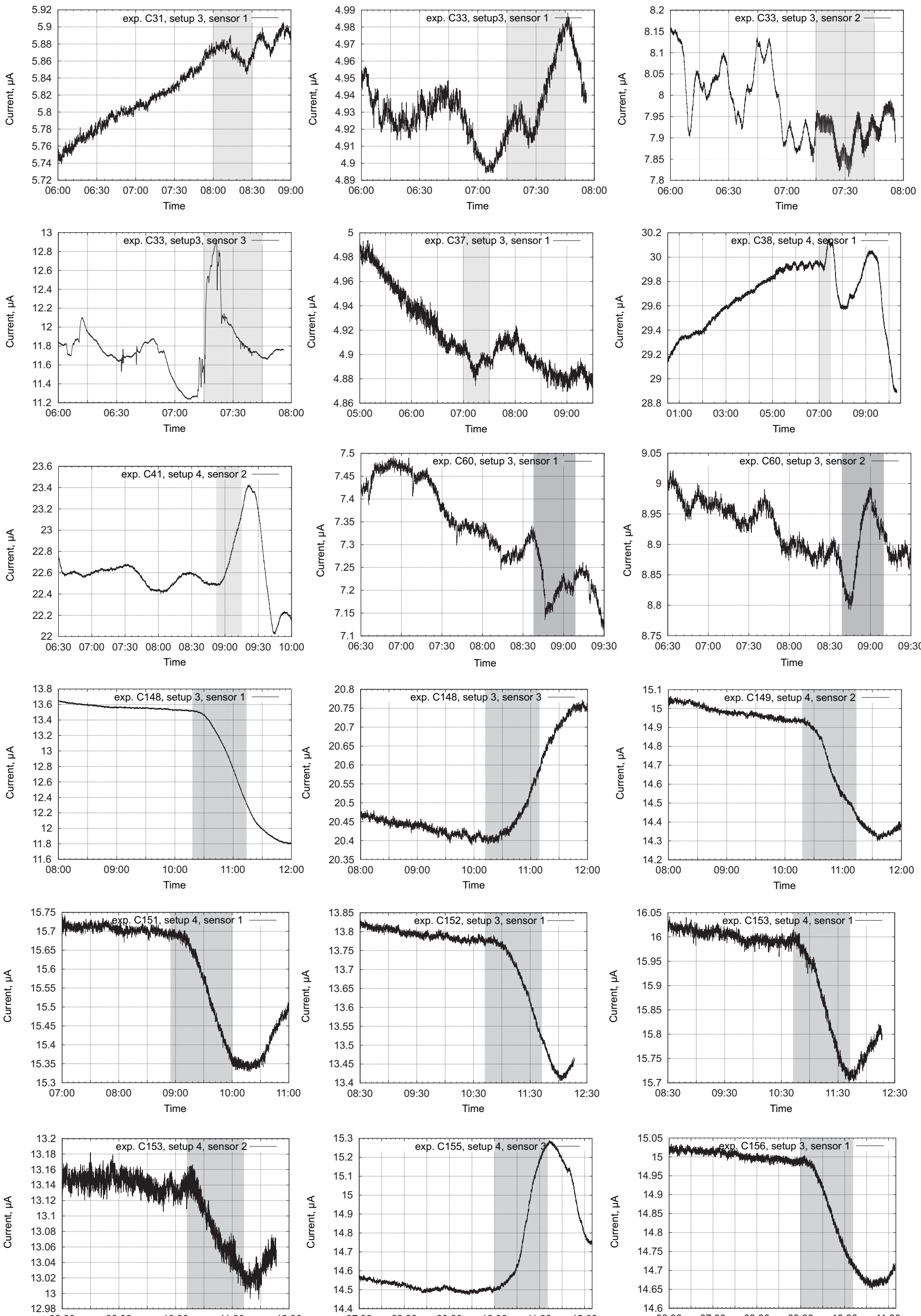


Figure 12. Several experiments with the LED generator.

TABLE 2
Overview of Experiment Series C with the LED Generator

N	Setup Number	Average Current $I\,S_{1,2,3}$ µA	$\Delta I\,S_{1,2,3}$ µA	Comments
C23	3	8.6, 5.5, 4.5	1.3, 0.3, none	d=15cm, LED=1, lab A
C27	3	11.3, 9.9, 7.6	0.15, 0.1, 0.05	d=15cm, LED=CCW, lab A
C29	3	12.5, 11.3, 8	0.05, 0.1, none	d=5cm, LED=1, lab A
C30	3	5.75, 9.1, 15.8	0.05, none, 0.4	d=5cm, LED=CCW, lab A
C33	3	4.9, 7.9, 11.4	0.04, T3, 1.2	d=5cm, LED=1, lab A
C37	3	4.9, 7.2, 7.2	0.02, 0.2, none	d=5cm, LED=CCW, lab A
C38	4	30.0, 22.0, 31.0	0.3, 0.5, T2	d=5cm, LED=CCW, lab A
C40	3	10.4, 39, 21.2	none, none, 0.6	d=5cm, LED=CW, lab A
C41	4	30.9, 22.6, 28.0	0.3, 0.6, none	d=5cm, LED=CW, lab A
C60	3	7.3, 8.9, 20.8	0.2, 0.2, none	d=15cm, LED=CCW, lab A
C61	4	32.3, 25.3, 29.4	T3, 0.4, none	d=5cm, LED=CCW, lab A
C130	3	13.75, 15.8, 19.2	0.3, 0.6, 0.1	d=35cm, LED=CCW, lab B
C148	3	13.5, 8.2, 20.4	1.1, 0.2, 0.2	d=25cm, LED=CCW, lab B
C149	4	16.4, 14.9, 14.4	0.3, 0.5, 0.05	d=25cm, LED=CCW, lab B
C150	3	13.6, 7.0, 20.27	0.5, 0.12, 0.05	d=25cm, LED=CCW, lab B
C151	4	15.7, 13.2, 14.05	0.35, 0.25, 0.05	d=25cm, LED=CCW, lab B
C152	3	13.77, 6.68, 20.46	0.2, none, none	d=25cm, LED=CCW, lab B
C153	4	16.0, 13.14, 14.28	0.35, 0.12, 0.04	d=25cm, LED=CCW, lab B
C154	3	14.65, 6.58, 21.9	0.3, 0.06, 0.05	d=25cm, LED=CCW, lab B
C155	4	16.15, 13.68, 14.5	0.35, 0.06, 0.7	d=25cm, LED=CCW, lab B
C156	3	15.00, 6.57, 22.82	0.3, 0.12, 0.08	d=25cm, LED=CCW, lab B
C157	4	17.6, 14.35, 15.25	0.5, 0.2, 0.3	d=25cm, LED=CCW, lab B
C166	5	10.6, –, –	0.05, –, –	d=30cm, LED=CCW, lab B
C166	3	33.18, –, –	0.01, –, –	d=76cm, LED=CCW, lab B
C167	5	10.51, –, –	0.05, –, –	d=30cm, LED=CCW, lab B
C167	3	34.58, –, –	0.02, –, –	d=76cm, LED=CCW, lab B
A165	5	10.94, –, –	0.08, –, –	d=30cm, LED=CCW, lab B
A168	5	10.42, –, –	0.2, –, –	d=30cm, LED=CCW, lab B

None means no qualitative changes.
T2, T3: See description in text.
Grayed rows indicate the experiments, with parallel recording by several sensors.

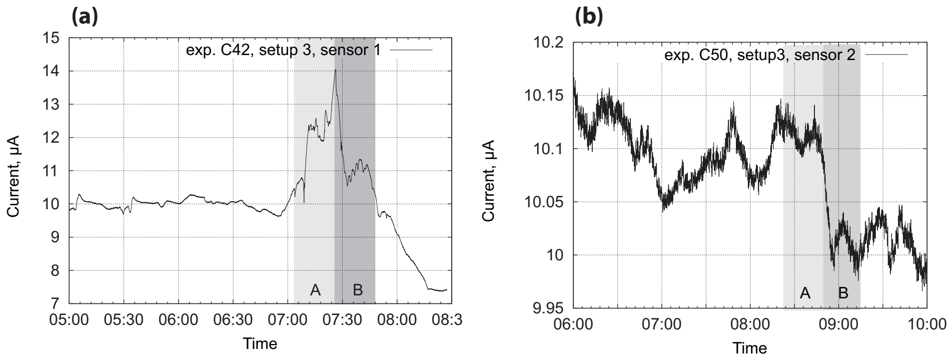


Figure 13. Experiments with irradiated water.
(a) Experiment C42; **(b)** Experiment C50.
(Gray area A) Container A is placed in front of the detector, d = 15 cm.
(Gray area B) Container B is placed in front of the detector, d = 15 cm.

Experiments with Irradiated Water

Experiments with irradiated water were performed in a similar way to those with the LED generator. A glass container with 500 ml of tap water was placed 5–15 cm from the LED generator. The generator was turned on for 5 minutes (20–30 minutes in C162–C194), then the container was stored separately from other containers. We irradiated the water several hours before the experiments (container "A") and immediately before the experiment (container "B"). An example of the behavior of the sensors is shown in Figure 13. We placed container A for 20 minutes and then replaced it with container B for another 20 minutes. This approach was used in experiments C42–C49, where we observed a stronger reaction with container B. In experiments C50–C55 we used water irradiated on previous days. Again, container B caused a stronger reaction. In experiments C54–C194 we used only one sample of A or B and observed monotonic changes of behavior to some extent (without the step-like change characteristic of containers A and B). The parameters of the experiments are collected in Table 3.

In several experiments we observed a reaction in the detector immediately after the container with irradiated water was removed from the box (for example, experiment C53). Experiments C44 and C45 contained noisy data, where reliable identification of a reaction was not possible. Despite an evident reaction in the sensors, we removed these data from evaluation as not plausible. Experiment A169 is counted as a single trial and not as an independent experiment. In total we performed 82 trials (16 independent experiments), in 24 cases of which we did not observe an evident reaction (especially for water irradiated several days before an experiment).

TABLE 3
Overview of Experiment Series C Using Irradiated Water

N	Setup Number	Average Current $I\ S_{1,2,3}$ μA	$\Delta I\ S_{1,2,3}$ μA	Comments
C42	3	10.0, 15.0, 22.6	4.0, 0.2, 0.5	A-7, B-0
C43	4	30.5, 25.5, 32.6	0.5, none, none	A-7, B-0
C46	3	9.8, 12.2, 22.3	0.1 (T3), 0.1, none	A-7, B-0
C47	4	29.6, 26.7, 31.0	none, 0.4, none	A-7, B-0
C48	3	8.6, 11.6, 22.3	none, T2, 0.1	A-7, B-0
C49	4	30.8, 27.2, 31.5	0.3, T2, none	A-7, B-0
C50	3	7.9, 10.1, 21.8	0.1 (T3), 0.1, 0.5 (T3)	A-36, B-24
C51	4	30.1, 25.6, 30.5	0.4 (T3), T2, 0.2	A-36, B-24
C52	3	7.8, 9.8, 21.4	0.25, 0.1, none	A-47, B-35
C54	3	7.6, 9.5, 21.4	0.2, 0.1, none	A-52, B-40
C55	4	31.0, 25.2, 29.5	0.4, T2, 0.5	A-52, B-40
C162	3	19.5, 9.82, –	0.5, 0.02, –	B-0
C163	4	18.6, 20.85, 17.8	0.04, 0.1, 0.04	B-0
C169	5	11.35, –, –	0.04, –, –	B-0
C175	3	12.8, 9.0, 13.45	none, 0.05, 0.3	B-0
C176	4	12.06, 12.96, 11.5	0.06, 0.08, none	B-0
C177	3	16.4, 11.6, 8.67	0.08, none, 0.12	B-0
C189	3	15.55, 14.38, 13.65	0.05 (T2), 0.04, T3	B-0
C192	4	9.06, 13.1, 6.9	0.05, 0.1, 0.1 (T3)	B-0
C192	5	–, –, 11.55	–, –, 0.05	B-0
C193	5	14.3, 6.5, 11.3	0.1, 0.05, 0.3	A-24
C194	3	6.1, 11.4, 10.55	none, T2, none	A-24
C194	4	8.9, 13.1, 6.12	0.2, T3, 0.04	A-24
C195	3	4.94, 11.2, 10.45	0.05, none, none	A-48
C195	4	6.1, 13.5, 6.02	none, 0.1, none	A-48
C195	5	13.92, 6.7, 11.1	0.04, 0.1, 0.1	A-48
C197	3	4.15, 11.1, 10.45	none, none, 0.03	A-72
A197	4	8.9, 12.6, 6.15	none, none, T3	A-72
A197	5	13.73, 6.7, 9.2	0.03, none, none	A-72

None means no qualitative changes. T2, T3: See description in text. Grayed rows mark the experiments, with parallel recording by several sensors. A-52 means that container A was irradiated 52 hours before the experiment.

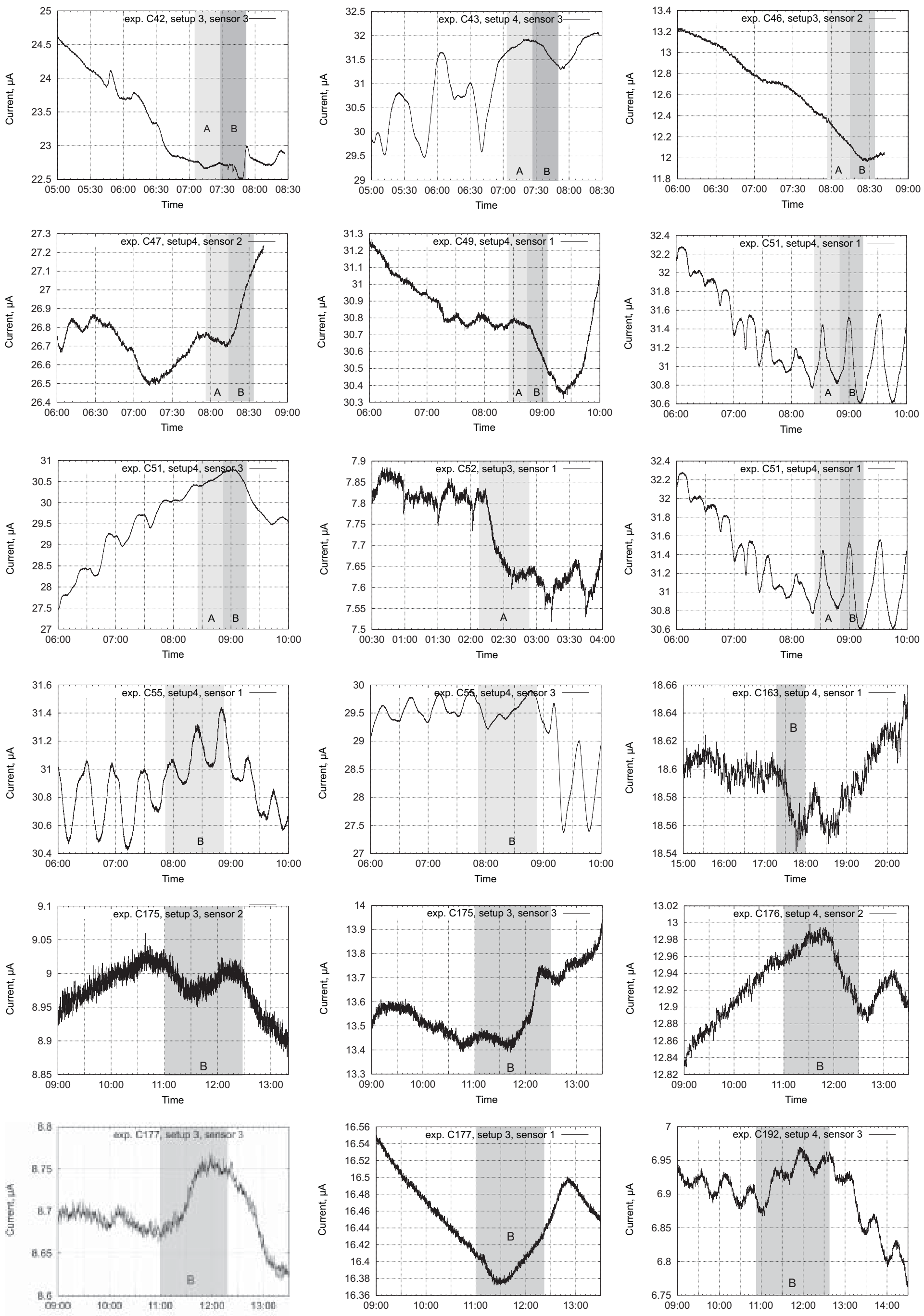


Figure 14. Several experiments with irradiated water.

Control Experiments

The control experiments were carried out under the same conditions as experiment series C; however, we used normal tap water. A container with water was rested 12 hours in the laboratory, then installed 15 cm in front of the detectors and exposed for between 30 minutes and 90 minutes. In several experiments (C62, C63, C199) the container remained in the laboratory for several days before an experiment. An overview of the control experiments is shown in Table 4. The reaction of the sensors in these experiments is significantly weaker than in the previous ones. For instance, we observed a type 1 reaction with step-like changes of current, as shown for example in Figure 13 and Figure 11. From 79 trials (of 14 independent experiments) we obtained 63 negative and only 8 positive reactions. It should be noted that in the original experiments with irradiated water (Bobrov 2009) the authors also obtained reactions in the sensors in control experiments that were weaker than the reactions with the irradiated water. The dynamics of the current in several positive cases are shown in Figure 15. Based on the criteria for previous experiments, we qualified several results from these experiments as positive. Within the boundaries of these experiments, we cannot identify whether these were caused by environmental noise or whether there were other pertinent factors, for instance a long resting time in the laboratory.

Discussion of Results

An overview of all experiments is given in Table 5. Since negative results are evaluated as results without any change in current, with a change of less than 0.01 μA, including where the parameters of the oscillations changed, there is doubt whether this was caused by irradiation or whether this oscillation has multiple frequencies. We excluded four datasets from the evaluation in which the sensors demonstrated a significant change of current before or after the experiment, or where the data were not plausible due to noise from the environment. If two or more sensors in a series C experiment were simultaneously positive, that experiment was counted as positive.

Evaluating the experiments we performed, we can conclude that all independent experiments with the LED generator and the irradiated water are positive. Most of the experiments with the tap water are negative (except those where the tap water was rested for several days in the laboratory). We performed several statistical tests for the trials. First of all, positive trials are coded as “1”, negative trials as “0” (i.e. trials are considered as experiments with binary output, T2/T3 results are not counted), and then we ran the chi-square goodness of fit test against the null hypothesis of the random character of the obtained data. Second, the results coded as “T1”-

TABLE 4
Overview of Control Experiments

N	Setup Number	Average Current $I\,S_{1,2,3}$ µA	$\Delta I\,S_{1,2,3}$ µA	Comments
C56	3	7.0, 8.3, 22.1	none, none, none	$t = 45$ min
C57	4	30.1, 24.2, 29.0	none, none, none	$t = 45$ min
C58	3	7.4, 8.9, 20.96	none, 0.1, none	$t = 80$ min
C59	4	31.0, 25.2, 28.5	T2, none, none	$t = 80$ min
C62	3	7.4, 8.9, 5.6	T2, 0.04, none	$t = 30$ min, 120 hr
C63	4	31.0, 25.5, 35.1	none, none, T2	$t = 30$ min
C64	3	7.1, 9.1, 4.8	none, none, none	$t = 40$ min
C65	4	34, 25.5, 35.0	none, T3, none	$t = 40$ min
C66	3	6.8, 8.9, 4.4	none, none, none	$t = 40$ min
C67	4	33.9, 26.5, 34.1	none, none, 0.05	$t = 40$ min
C68	3	13.1, 22.6, 17.7	none, T3, none	$t = 30$ min
C69	4	19.2, 15.5, 18.8	0.02, none, none	$t = 30$ min
C172	3	12.5, 25.5, 11.0	none, none, 2.0	$t = 30$ min
C172	5	10.2, –, –	none, –, –	$t = 30$ min
C173	3	11.95, 20.5, 10.65	none, none, none	$t = 60$ min
C174	4	10.7, 12.4, 11.7	none, none, none	$t = 60$ min
C179	3	10.5, 24.9, 11.22	none, T3, none	$t = 60$ min
C180	4	11.6, 12.05, 13.2	none, none, none	$t = 60$ min
C181	3	12.5, 23.9, 10.45	none, none, none	$t = 60$ min
C182	4	10.6, 12.78, 12.1	none, none, 0.03	$t = 60$ min
C185	4	11.35, 12.6, 10.1	none, none, none	$t = 90$ min
C186	4	12.75, 10.2, 12.5	T3, none, none	$t = 90$ min
C187	3	11.7, 20.5, 11.5	none, none, none	$t = 90$ min
C188	4	13.7, 14.78, 14.1	none, none, none	$t = 90$ min
C199	3	19.2, 11.1, 10.25	none, none, none	$t = 90$ min, 96 hr
C199	4	10.09, 13.6, 9.2	0.03, T2, none	$t = 90$ min
C199	5	13.42, 6.7, 10.62	none, 0.1, none	$t = 90$ min

None means no qualitative changes. T2, T3: See description in text. Grayed rows mark the experiments, with parallel recording by several sensors. $d = 15$, t is the exposition of time. A-52 means that container A was irradiated 52 hours before the experiment.

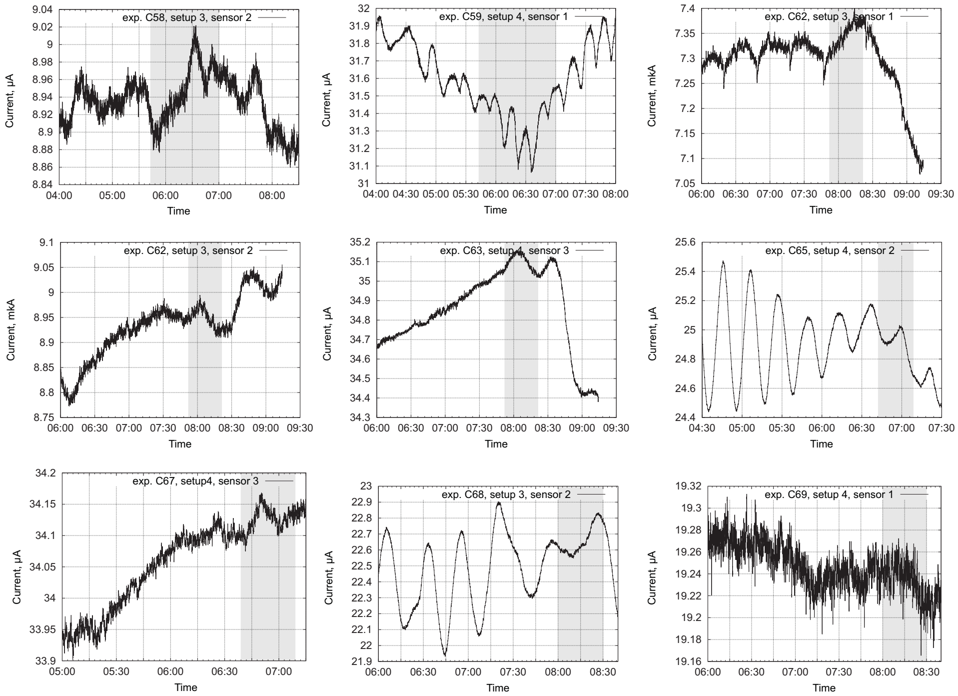


Figure 15. Several positive responses in control experiment series C with tap water.

1, "T2"-2, "T3"-3, and "N"-0 (negative) were tested by the Mann-Whitney U test for two groups of "LED generator vs. tap water" and "irradiated water vs. tap water" experiments (the null hypothesis is an identical distribution function of these groups). Finally, we considered the current in each trial (ignoring T2/T3 results) and calculated a one-sample *t*-test against the null hypothesis of "0" as the expected value. Results are shown in Table 5; based on obtained values we reject the null hypothesis for chi-square and Mann-Whitney U tests (significance level $\alpha \leq 0.005$, two-tailed). The null hypothesis for the *t*-test can be rejected for LED/irradiated water (the significance level $\alpha = 0.005$, two-tailed); however, it cannot be rejected for the tap water (significance level $\alpha = 0.240$, two-tailed).

From 154 evaluated trials of experiment C with the LED generator and irradiated water (32 individual experiments recorded in parallel by several sensors), 108 trials indicated a positive result and 35 trials indicated a negative result (see Figure 16).

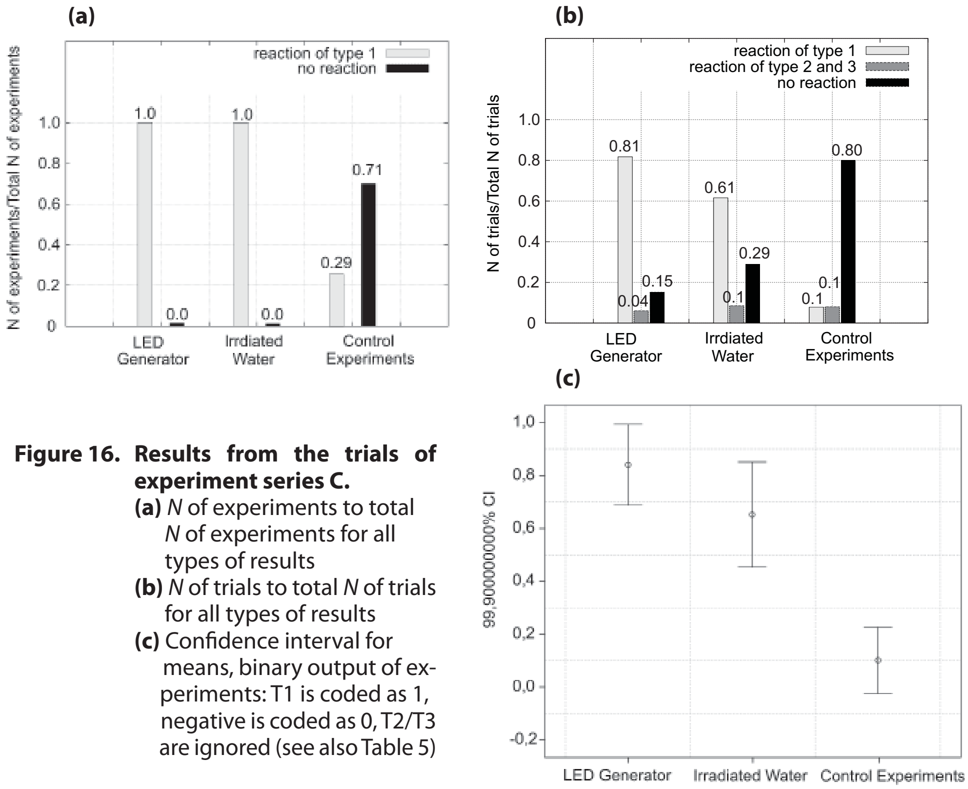


Figure 16. Results from the trials of experiment series C.
(a) *N* of experiments to total *N* of experiments for all types of results
(b) *N* of trials to total *N* of trials for all types of results
(c) Confidence interval for means, binary output of experiments: T1 is coded as 1, negative is coded as 0, T2/T3 are ignored (see also Table 5)

Comparison of the results from the LED generator and the irradiated water indicates that the irradiated water caused a weaker reaction in terms of (a) the number of responsive sensors, (b) the latent time, and (c) ΔI. The strongest impact occurred when the water was irradiated immediately before the experiment. The exposure time with the LED generator also had an impact: We observed a stronger reaction with a longer exposure time. We cannot unarguably say whether the reaction of the irradiated water can be related to a spin-based imprinting of water, as proposed in Bobrov (2009). Comparing the results with irradiated and normal water, we recorded almost no reactions from non-irradiated water. However, it is also unclear whether the several single responses from non-irradiated water were caused by environmental noise or some other factor.

In the experiments we did not observe changes of the EM field and acceleration (vibrations) above the minimal resolution of the sensors used.

TABLE 5
Overview of Experiment Series C and Its Statistical Evaluation*

	N of Trials	*N* of Exp.	RESULTS T1	T2/T3	Neg.	Mean, Std. Dev., Std. Err.	Chi-Square Test	M-W U-Test (*z*)	*t*-Test
LED gen.	72	–	58	3	11	0.84, 0.369, 0.044	32.014	−6.596	6.914
	–	16	16	–	0	–	–	–	–
Irr. water	82	–	50	8	24	0.68, 0.471, 0.055	9.135	−5.468	3.041
	–	16	16	–	0	–	–	–	–
Tap water	79	–	8	8	63	0.11, 0.318, 0.038	42.606	–	1.184
	–	14	4	–	10	–	–	–	–
Total	233	–	–	–	–	–	–	–	–
	–	46	–	–	–	–	–	–	–

* See description in text. Mean, Std. Deviation, and Std. Error are calculated for the binary output of experiments. T1 is coded as 1, negative is coded as 0, T2/T3 are ignored.

Containers of irradiated water are passive objects and do not emit any EM fields. However, we need to take into account high-frequency, stationary EM waves produced by external EM emitters (for example the laptop used in laboratory B or WiFi access points in laboratory A), which potentially can influence the behavior of the sensors. Any parasitic capacitive, optical, acoustic, and electric couplings were excluded from the experiments. Variation of temperature is the most difficult issue, because it cannot be completely removed from the experiments. Moreover, it requires the development of specific setups and experiment methodology—this should be taken into account when replicating these experiments. In several experiments we succeeded in demonstrating a non-proportional or delayed increase in temperature in relation to a response from current sensors. Thus, despite the temperature-impact sensors, its variation does not represent the main factor for the changing current during experiments.

We also identified environmental noise as one of the main difficulties. Comparing the behavior of sensors in an empty laboratory A during the day and at night, we noted more changes in the current occurring during the day. Original works point to an anthropogenic factor, e.g., ultraweak emission from the human body (Kobayashi, Kikuchi, & Okamura 2009) as the main source of such noise. For instance, the experiments described in Bobrov (2009) were performed not only in an empty room but in an empty building.

Thus, more robust experiments need to be performed in a special physical laboratory, where temperature, EM, and environmental and anthropogenic influences can be minimized by several orders of magnitude.

In brief, excluding EM, temperature, mechanical, optical, capacitive, and acoustic interactions, and any parasitic couplings, the publications cited mention two possible explanations for this behavior. First, Bobrov (2009, 2006) points to spin waves, which have been recently polemicized in the physics community. Experiments carried out with rotating objects, sources of radioactivity, and plant and animal cells support this theory to some extent and possibly explain interactions between biological and non-biological objects. Original publications suppose that spin waves generated by LEDs (or by the 630 nm helium–neon laser) are responsible for the spatial polarization of water dipoles in the Gouy–Chapman layer. Second, Zenin (2000) introduced stable macroclusters of water dipoles, which can exist for a long time (Zenin 2005). For instance, the behavior of V_{in}^{a} on voltage electrodes in setup 2 (see Figure 21) can be explained by the appearance of such macroclusters of dipoles. Since sensors contain water, macroclusters of dipoles can influence conductivity and thus the current. In turn, the combination of different weak signals can affect the behavior of macroclusters. However, as mentioned in the Introduction, the goal of this work is only to replicate the cited experiments without deep physical or chemical discussions explaining the behavior of the sensors.

Conclusion

In several experiments we observed a causal dependency between switching on the LED generator or installing the water container and the reactions of the sensors. Between two and six sensors simultaneously recorded such reactions. The recorded data were evaluated about two hours before and after the experiments. The active LED generator and passive irradiated water caused a similar impact on the detectors, whereas tap water and a normal state (night hours without any objects close by) indicated mostly a monotonic dynamic in the sensors. The impact of such influences as temperature, EM fields, and others was minimized up to the level sensed by the measuring devices. Based on these results, we evaluate the main part of the replication attempt as successful.

We offer several notes regarding these experiments. First, the measured level of current is about 1−50 μA with a resolution of 0.01 μA. Measuring such a small current is sensitive to many factors: resistance of cables, input impedance of operational amplifiers, temperature drift of electronic components, and electronic noise. Thus, while the setup has the advantage of compactness, it can be used only to detect changes. It is not intended for precise measurement of ionic processes appearing in the electric double layer. Second, the setup is protected from mechanical, optical, capacitive, temperature, and EM influences only up to the level allowed in a normal electronic laboratory. To improve the level of protection, for example when investigating the effect of very small influences, the experiments would have to be repeated in a special laboratory. We also did not investigate physico–chemical properties of water before and after experiments; these tests should be performed in such a laboratory. Third, we did not explore the impact of the geometrical placement of sensors and LED generator/water in all experiments, however it is assumed that such an impact exists and that the obtained results are influenced by it.

In the context of our research, the interaction between blue-light LEDs and deeply polarized electrodes introduces a new component in underwater communication. We observed this behavior in our original experiments. After performing experiment series C, this dependency became clearer. Since electric double layers are generally sensitive to mechanical and EM influences, it is possible that strong light or even the movement of mobile devices can be used to modulate an electric field. This suggests future work.

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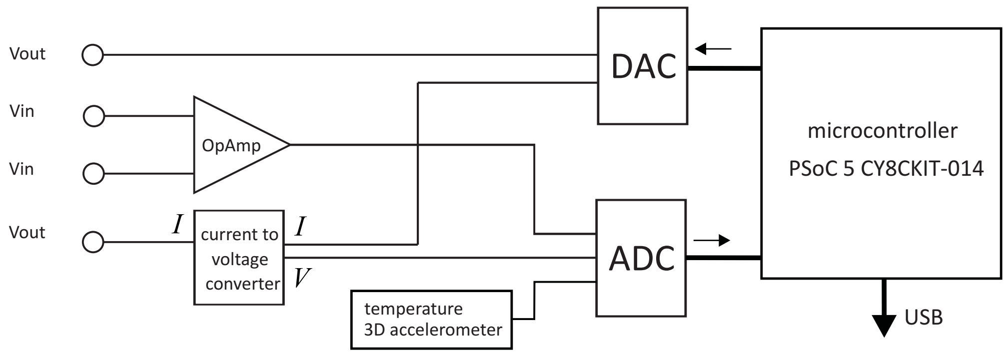

Figure 17. Structural scheme of the experiment setup. Voltage electrodes are installed only in setups 2 and 3.

Appendix A
Experiment Setup

The original experiments (Bobrov 2009) used DC voltage up to 30 V. For the replication experiments we used the well-known four-electrode scheme (see Figure 17). The electrodes V_{out} take their voltage from a digital–analog converter (DAC) in the range of ±5 V. The current flowing in the DAC circuit is converted to a voltage and digitalized by an analog–digital converter (ADC). The voltage from V_{in} electrodes is amplified by an operational amplifier and also digitalized by an ADC. Both the DAC and ADC are connected to the microcontroller. Data are transmitted via a USB interface to a laptop and sampled at a frequency of 1 Hz in real time (each measurement from the ADC is time-stamped). We used two versions of this equipment. The first version has an instrumental operational amplifier (AD8226), external DAC (AD5322), and ADC (AD7656), and provides an accuracy of current and voltage measurement of μA and mV ranges of 0.1%.

The second version uses a programmable system on chip (PSoC)

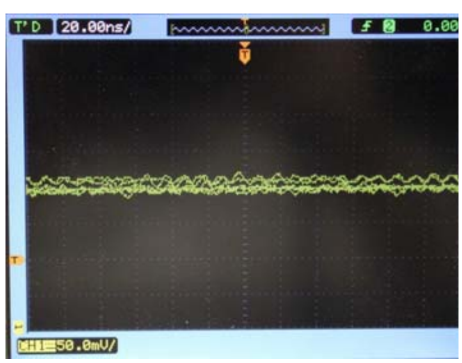

Figure 18. Noise produced by the DAC and power supply.

CY8C5588AXI 060 with internal amplifiers, DAC, and ADC. Accuracy of measurement is about 3%. The system is powered at 5 V through a USB interface from a laptop. The noise from the DAC and power supply is shown in Figure 18 and is in the range ±10 mV, about 0.3% of the voltage generated. To reduce the level of noise, a software filter averages values

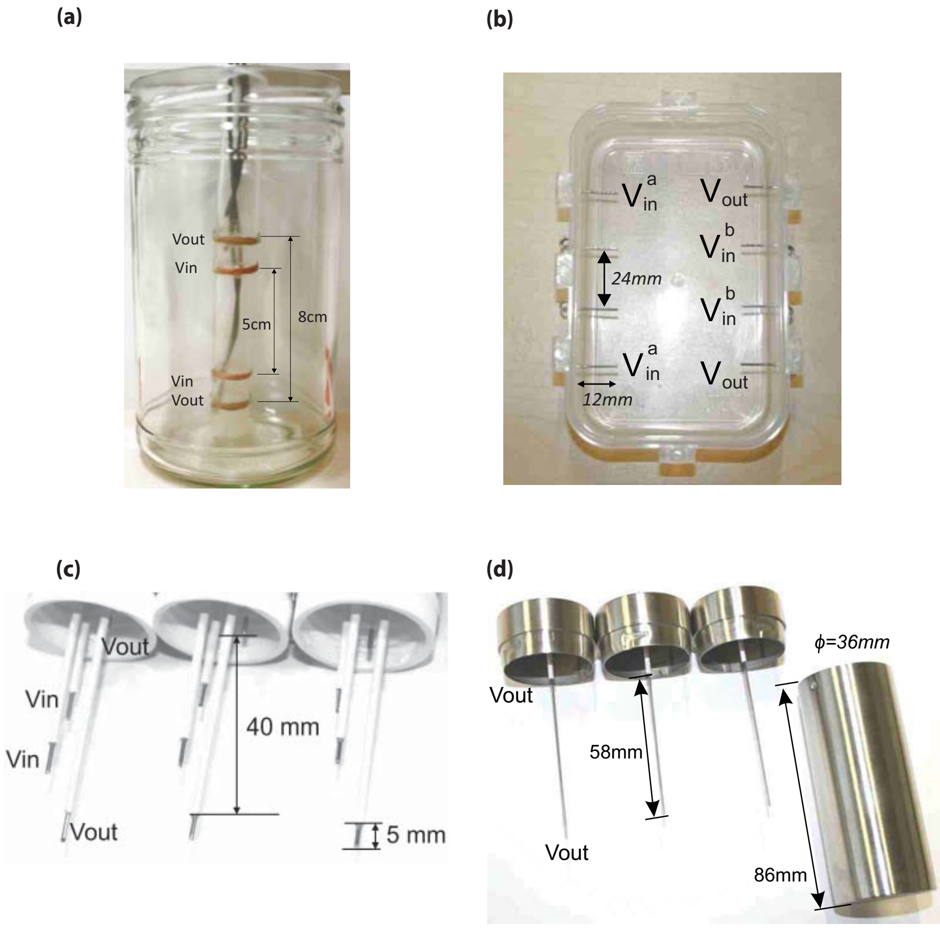

Figure 19. Description of the experiment setups.

(a) Setup 1, with a 1000-ml glass bottle containing a plastic tube of 3 cm diameter. The electrodes are made of copper (in the form of a ring) and installed on the plastic tube, distance between electrodes as shown. The glass bottle is closed by a plastic cover, with the electrodes inside.

(b) Setup 2 uses 300 ml water with 8 electrodes made from stainless steel of 1 mm diameter.

(c) Setup 3 has 3 independent sets of electrodes made from stainless steel of 1 mm diameter. Corresponding containers are glass jars of about 50 ml.

(d) Setup 4 has 3 independent sets of electrodes. The anode is a 1 mm wire, the cathode is a cylinder, both electrodes are made from stainless steel.

from the ADC (current and voltage) within a sliding frame of five steps.

The electronics also contain a 3D accelerometer KXSC7-2050 (sensitivity 660 mV/g) and three types of temperature sensors: NCP21XV103J03RA (installed on PCB), LM35AH, and AD592CNZ (installed in the containers with electrodes). The sensitivity of temperature measurement is below 0.01 C with a 20-bit ADC. Setups 3, 4, and 5 have two, two, and four temperature sensors, respectively. Data from temperature, acceleration sensors, and voltage of power supply were recorded during all the experiments to demonstrate the influence of these factors. For calibration and test measurements, an Agilent Technology oscilloscope DSO1014A and true RMS Multimeter 72-7730 for accurate DC current measurement in the range ± (0.1% + 15) were used.

All setups were shielded in a similar manner. All sensors from setups 3 and 4 were first inserted into metal cans made of 0.5 mm steel (three sensors in each) (see Figure 1), and then both cans were placed into a box made from 1 mm brass. Each sensor from setup 5 was inserted into a pipe made of 3 mm brass. All metal cans, boxes, and pipe were grounded. Temperature shields were experimentally selected from several materials. First we used boxes made of 80-mm–thick styrofoam; however, it seems this material is not suitable for experiments. Finally, all metal containers were lined with 10-mm rubber matting and empty space was filled up with wool.

Since the first setup remained from our previous experiments and had copper electrodes, it was removed from further tests. All other electrodes were made from chromium–nickel stainless steel (setups 2, 3, 4) and platinum (setup 5) of 1 mm diameter. Setup 5 is similar to setup 3, only the electrodes are made of platinum and they can be shifted inside a glass container. In all setups we used deionized water produced by Walter Schmidt Chemie GmbH (osmosis approach) and double-distilled water produced by AuxynHairol.

The LED generator is shown in Figure 20. It consists of 169 blue-light (470 nm) LEDs LC503FBL1-15Q-A3 placed in an area 120 × 120 mm^2. These have a luminous intensity of 11 cd and opening angle of 15 degrees. The LED generator has eight switchable fields and is controlled by an ATMega328 microcontroller. Powering of the digital part and LEDs is independent for each, and the voltage applied to LEDs can be varied within 2.5–6 V. In other experiments with deeply polarized electrodes, we applied up to 48 V to LEDs—we have experimental evidence that LEDs with a higher voltage impact more intensively on sensors. The microcontroller has a temperature sensor. For experiments without an operator, the microcontroller monitors the environmental temperature and can autonomously start an experiment when the variation of temperature is

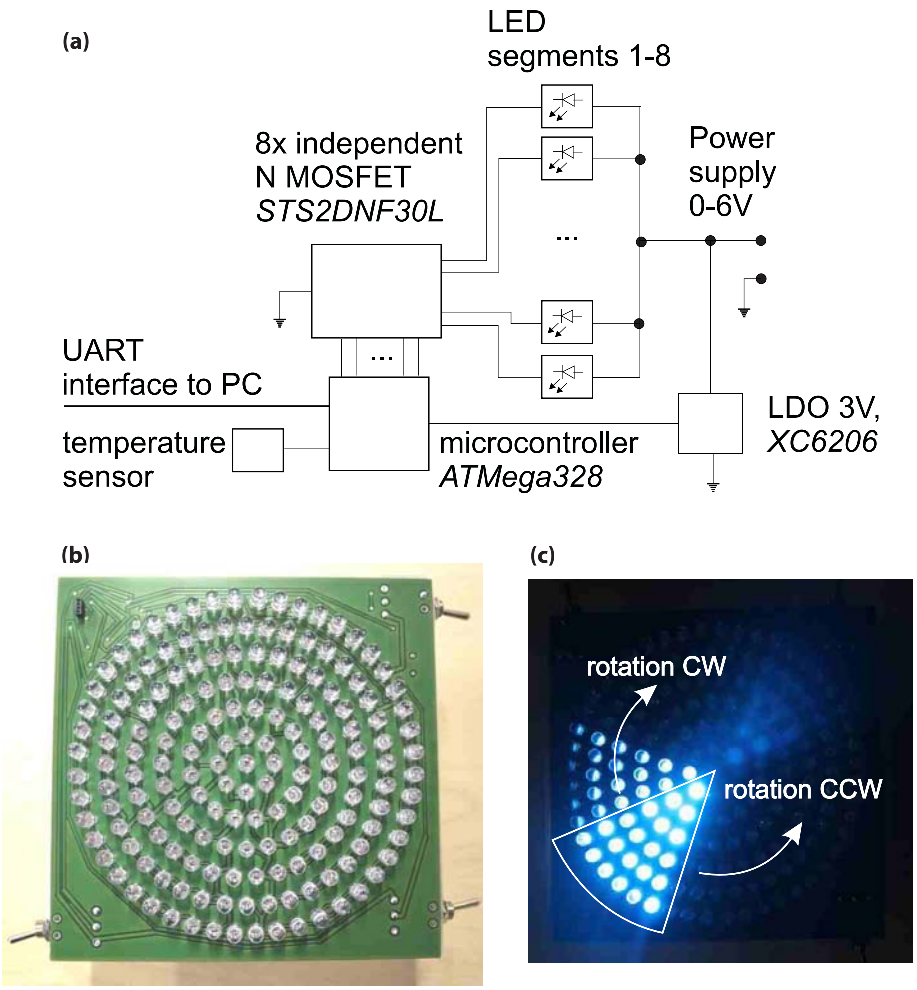

Figure 20. The LED generator.
(a) Structure of the LED generator
(b) LED generator made with 169 blue-light (470 nm) LEDs
(c) LED generator: rotation of sectors CCW and CW

low (there is a log of all activities of the LED generator). We programmed four modes of operation: oscillation 1 (LED = 1, "on" pulse—1 µs, "off"—333 µs), oscillation 2 (LED = 2, "on" pulse—100 ms, "off"—100 ms), and rotation of sectors CCW and CW (LED = CCW, CW, "on" pulse—100 ms, "off"—100 ms) (see Figure 20(c)). To avoid parasitic coupling, the LED generator ran on two, three, or four D-size batteries.

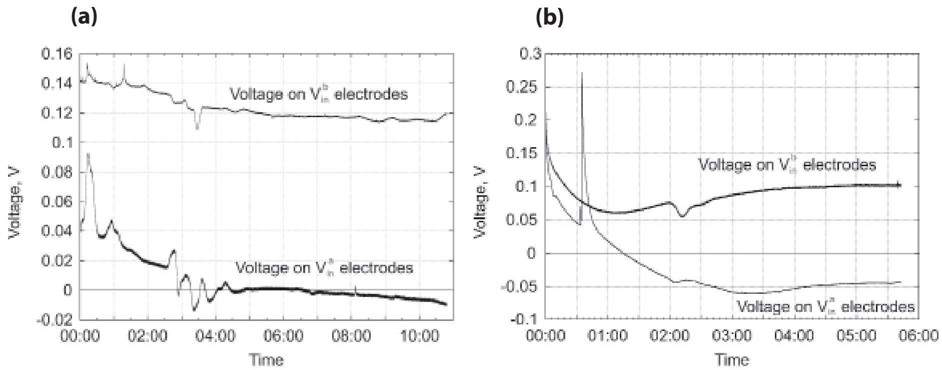

Figure 21. "Current position" vs. "field position".
(a) Experiment A10
(b) Experiment A15

Loss of sensitivity. After a while, sensors lose their sensitivity. This loss shows when the current quickly stabilizes to a particular value and does not indicate even normal environmental noise. This effect was confirmed by A.V. Bobrov for his sensors. The timeframe within which a sensor loses sensitivity varies between a few days and a few weeks. We surmised that contamination of the electrodes was the reason for this effect; when it appeared, we replaced all the electrodes and changed the water. Sensors are also less sensitive when they relax after disturbances or after powering the systems. Usually, we waited for 6–8 hours after powering before operating the sensors. After disturbances, the relaxation time is between 3 and 12 hours. Loss of sensitivity could be one of the reasons why, in several experiments, some sensors did not record any changes of current, whereas other sensors did.

Voltage in V_{in}^{a} and V_{in}^{b} electrodes. In the second and third setups we used V_{in}^{a} and V_{in}^{b} electrodes to test the hypothesis that macroclusters of water dipoles were created (Zenin 2000). The idea is inspired by Orion Conductivity Theory (no date), in which a "current position" (V_{in}^{b}) and a "field position" (V_{in}^{a}) in the four-electrode scheme are distinguished. Analyzing the state of the art in this area, we discovered a number of works (Vegiri & Schevkunov 2001, Rai, Kulkarni, Gejji, & Pathak 2008) concerning the clustering of water in electric fields as well as the simulation of clustering behavior (Kumar & Skinner 2008). Based on these works, we anticipated different potential dynamics of V_{in}^{a} and V_{in}^{b} electrodes. This was experimentally confirmed, for instance in Figure 21 we plot V_{in}^{a} and V_{in}^{b} in two particular experiments. It can be seen that the potential for V_{in}^{a} can change its polarity (the polarity of V_{out} remained constant); artefacts on

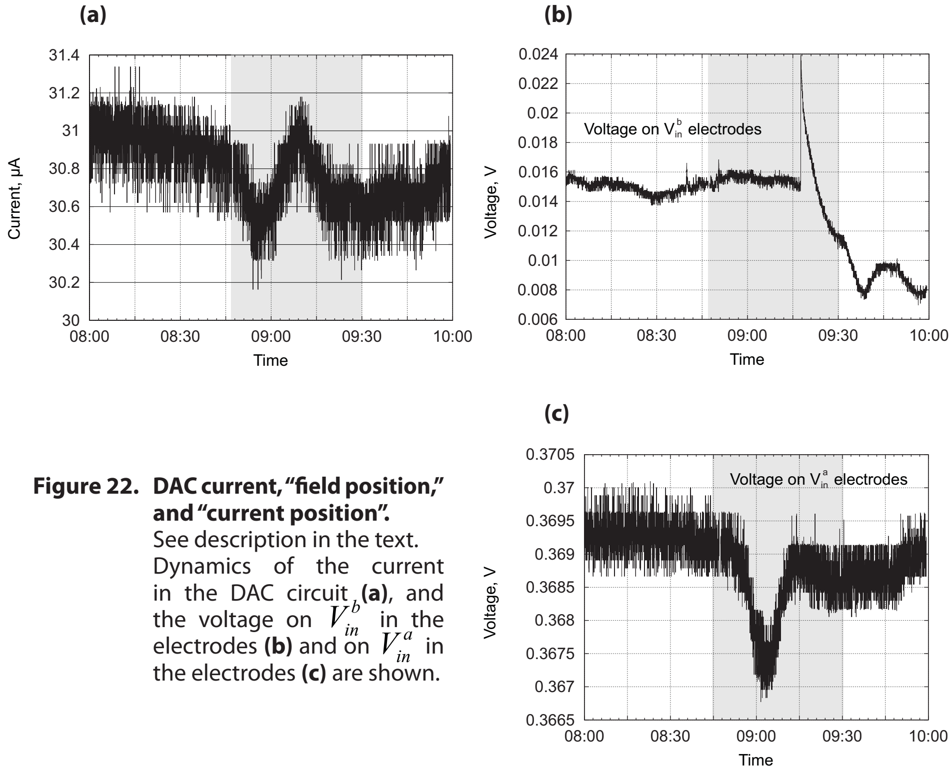

Figure 22. DAC current, "field position," and "current position". See description in the text. Dynamics of the current in the DAC circuit **(a)**, and the voltage on V_{in}^{b} in the electrodes **(b)** and on V_{in}^{a} in the electrodes **(c)** are shown.

one set of electrodes were not observed on other sets of electrodes. In the preliminary experiments with the LED generator (discussed in the section **Experiment Series C**) we recorded the dynamics V_{in}^{a} and V_{in}^{b} and the current in the DAC circuit, as shown in Figure 22.

We can see that V_{in}^{a} demonstrates similar dynamics on the current, that is the voltage of the four- and six-electrode schemes are also sensitive to non-EM fields. Since these experiments move away from the original approach (Bobrov 2006), we decided to postpone them and, for this work, concentrate on current detection. In setups 3, 4, and 5 we analyzed only the current flowing through the DAC circuit.

Auto-oscillation mode. As mentioned in Bobrov (2009, 2006), sensors can enter a so-called "auto-oscillation" mode. The period of these oscillations varies between a few minutes and a few hours. We are unable to estimate the current range in which the auto-oscillation can appear. We observed the spontaneous start of oscillations between 4 and 40 μA, that is, in the whole range of current measurement. Bobrov (2006) suggested that this mode is more sensitive to non-EM fields.

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COMMENTARY

The Influence of Reichenbach's Concept of Od

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Michael Nahm's (2012) recent article about Reichenbach and his concept of Od, in *JSE 26*:2, Summer 2012, reminds us of important work done in the past that has been forgotten by many current students of psychic phenomena and related topics. I find particularly interesting how the concept of Od influenced a variety of conceptual developments, something I would like to briefly discuss in this Commentary. While Nahm is aware of this, and addresses the issue briefly, he appropriately in my view did not make this the focus of this paper because his purpose was a general overview of Reichenbach for the modern reader.

One of the main influences of Reichenbach was how his work was used by others to develop and support the development of unorthodox concepts of force in relation to psychic phenomena, a model that existed before in the mesmeric movement and in other contexts. A prominent example of this was how Reichenbach's Od was one of the inspiring factors behind the development of ideas of forces to explain physical manifestations such as those associated with mediums during the beginnings of spiritualism. This is clearly seen in American books published during the 1850s in which various authors speculated on the powers of the living medium to explain various mediumistic manifestations. A prominent one was Edward C. Rogers' *Philosophy of Mysterious Agents, Human and Mundane* (1853). Reichenbach's work was used by Rogers repeatedly throughout the book to justify his acceptance of the existence of a new force associated with the nervous system. This was basically a biophysical force coming from the bodies of mediums and others, an idea that allowed him to apply the concept of a non-spirit–based agency to explain spiritualistic manifestations such as movement of objects, raps, and luminous effects. He wrote in his Introduction:

> In our researches with regard to the phenomena treated in the following pages, we have found so many of the characteristics of an agent differing so

> essentially from those of Electricity and Magnetism proper, and bearing so many of the characteristics of identity with the Odyle of Reichenbach, that we feel forced to admit this identity. (Rogers 1853:20–21)

Rogers also used Reichenbach as a guide to analyze several cases, such as the so-called electrical girl Angélique Cottin, a once-well-known poltergeist case, in whose presence objects were reported to move (Owen 1864, Tanchou 1846). In his view the attraction and repulsion phenomena of Cottin followed Reichenbach's ideas of polarity. He also related Od to the phenomena of the divining rod (p. 272).

The concept of Od was also important to support the notion of forces responsible for mediumistic phenomena in other writings (e.g., Guppy 1863, Mahan 1855). An interesting example of the centrality of Od in speculations of this sort were the ideas of physician B. W. Richmond presented in his book with Samuel B. Brittan *A Discussion of the Facts and Philosophy of Ancient and Modern Spiritualism* (Brittan & Richmond 1853).

> The Od-force of Reichenbach [wrote Richmond] comes to our aid in the "modern mysteries." It is an imponderable fluid . . . . The human body having it in abundance transmits it to inanimate matter—the human will having control over it—as easily grasps and impels it, when chairs and tables have been charged with it, as when a muscle or a nerve has been charged with it. (Brittan & Richmond 1853:70)

Such influences were not limited to the United States. Od made its way to the writings of individuals in other countries. Examples in England and in Germany were Herbert Mayo (1851) and Carl du Prel (1899/1907), respectively. The latter, who was discussing the subject in the later part of the nineteenth century, believed that: "The key of magic is in animal magnetism, what Reichenbach has designated with the name of *od*. It is the *physics of magic* . . ." (du Prel 1899/1907:13). In his view Od accounted for psychic phenomena and other manifestations.

The influence of Reichenbach was also evident in France in the work of Albert de Rochas. In fact, de Rochas greatly popularized Reichenbach's work in France in several books (de Rochas 1887:45–50, 1891, 1895:2–5, 189–190). A presentation of Reichenbach's lectures was preceded by various essays authored by de Rochas introducing the Baron and presenting a discussion of those who conducted work relevant to his.

Like Reichenbach, de Rochas believed Od was a vital principle related to the human body. He wrote in *Le Fluide des Magnétiseurs*:

> The odic movement, called a *current*, comes mainly from the brain, descends down through the nerves of the face and goes to its corresponding branches. Finally, it is exhaled in the air, rendered sensible by impressions of heat and cold that it causes on the sensitives, is made visible in the form of effluvia in plain day, and as lights in darkness. The whole body seems bright; the head seems to have an aureole; the hands, the fingers, and the toes throw long streams of odic light. (de Rochas 1891:104)

Reichenbach's Od was an important part of the context in which de Rochas (1895) conducted his well-known studies of the exteriorization of sensibility. In a later work entitled *Les Frontières de la Science*, de Rochas (1902) discussed the concept of a psychic force and Reichenbach's work and referred to him as "the man who is, without any possible comparison, the one who has studied the issue with the most care and talent" (de Rochas 1902:29). He organized the discussion of psychic forces in sections about the precursors of Reichenbach, Reichenbach's work, and Reichenbach's successors. Interestingly, de Rochas also related Od to the astral double (de Rochas 1906).

Spiritualists received the gospel of Od in different ways. Some were not convinced that it had the explanatory power to account for spiritualistic manifestations in the way Rogers, Mahan, and others suggested, as was the case of Samuel B. Brittan (Brittan & Richmond 1853). Others had different views. One of them stated:

> Mediumship seems to depend upon the presence in, and evolution from, the persons of its subjects, of a very subtle fluid—that which the German Von Reichenbach calls "human-od." When this "od" is electrical or negative, the party becomes a rapper or "physical medium." When it is positive or magnetic, the subject is a trance or mental medium of some sort . . . (Randolph 1860:27)

John W. Edmonds wrote about the existence of an electric body, a principle bridging the soul and the physical body. According to Edmonds: "In the earth-life its presence is manifested by that odic light of which Reichenbach speaks . . ." (Edmonds 1874:119).

In later years French spiritist Gabriel Delanne (1900) discussed Reichenbach's Od. He implied that such a principle was consistent with spiritist teachings. In particular he referred to the idea that spirits of the dead cannot act on matter without taking the "necessary force from a living being" (Delanne 1900:641). This force, Delanne said, was visible to "the *sensitives* discovered by Baron Reichenbach, and by some magnetic or hypnotic subjects . . . ." (Delanne 1900:641).

Reichenbach's work served yet another function. A small group of people utilized Reichenbach's work to argue for the power of the mind to create illusory phenomena, a topic discussed before in relation to mesmerism and other phenomena. The individuals in question saw the performances of Reichenbach's sensitives as the product of suggestion and expectation. This was the case of James Braid in *The Power of the Mind Over the Body* (1846). As he wrote:

> But it is an undoubted fact that with many individuals, and especially of the highly nervous, and imaginative, and abstractive classes, a strong direction of inward consciousness to any part of the body, especially if attended with the expectation or belief of something being about to happen, is quite sufficient to *change the physical action of the part, and to produce such impressions from this cause alone, as Baron Reichenbach attributes to his new force*. Thus every variety of feeling may be excited from an *internal* or *mental* cause . . . . (Braid 1846:6)

Later writers—Thomas Laycock (1851:389), William A. Hammond (1870:240), and Willliam B. Carpenter (1877:31), among others—presented variations of Braid's ideas assuming the influence of suggestion and expectation. Henri Beaunis (1884:197) stated that Reichenbach's tests showed the "influence of imagination, or, better, of a dominant idea in the production of sensations . . . ." However, the discussions presented by those men were less detailed than Braid's.

In summary, like the theoretical construct of animal magnetism, Reichenbach's concept of Od had many influences and uses. It provided the basis for speculation about a physical force to account for spiritualistic phenomena, and it inspired many to develop the idea of imagination to account for the unexplained perceptions of luminosities. The discussions and reactions to the concept of Od affected the conceptual development of spiritualism and psychical research.

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OBITUARY

Archie E. Roy Dies at 88

Archie was Emeritus Professor of Astronomy at the University of Glasgow. An asteroid is named after him in recognition of his contribution to Astronomy: Asteroid 5806 Archieroy. He was a prolific author, and not just on academic topics; in fact, six of his twenty books were novels. Archie's other scientific passion was investigating the paranormal. This interest began when he was a student at the University of Glasgow, when one day while browsing through the University library he came upon books on paranormal topics by noted scientists. His first reaction was "What's all this rubbish doing in a University library?" As he later said to me, at least he had the grace to read some of them. However, he was impressed by much of what he read, and he resolved to pursue the topic further. I met Archie in 1987, and thereafter we embarked on a series of experiments with mediums and healers. Three of our published papers are in the *Journal of the Society for Psychical Research*, and they document a five-year rigorous practical study of mediumistic information transfer in conditions up to triple blind.

Archie was a Fellow of the Royal Astronomical Society and the British Interplanetary Society. He was Past President of the Society for Psychical Research London and the Founding President of the Scottish Society for Psychical Research. Other positions included Patron of the Churches Fellowship, Scotland, for Psychical and Spiritual Studies, and membership in the Scientific and Medical Network. Archie lectured on both of his passions throughout the world. For many years he gave 10-week evening classes in Psychical Research at the University of Glasgow, and seven years ago I collaborated with him to expand this to a 20-week course.

Archie was in every sense a gentleman who was liked and respected by everyone, and whose personality and dry sense of humor will be sorely missed. His favorite expression was:

If when I die I find that I have not survived, I will be very surprised.

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LETTER TO THE EDITOR

Registering Parapsychological Experiments

A webpage for registering parapsychological experiments has been implemented by the Koestler Parapsychology Unit (KPU) at the University of Edinburgh, with Jim Kennedy advising on the development of the registry. The value of study registration for a controversial area such as parapsychology has been mentioned many times over the years. Prospective registration of experiments provides a database for research synthesis that is not subject to possible reporting or publication biases. Registration also increases confidence by providing clear evidence that the key hypotheses and analyses were planned prior to conducting the experiment. Among other benefits, registration should be of value to editors and reviewers during the publication process. Registration of experiments is a well-established practice in medical research and enhances the credibility of a study. The KPU registry webpage is at:

http://www.koestler-parapsychology.psy.ed.ac.uk/TrialRegistry.html

We expect that other options for study registration will become available in the future. Our intention is for the KPU registry webpage to also serve as a resource that provides information about other registration options as they become available.

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LETTER TO THE EDITOR

Magnetic Anomalies and the Paranormal

In his article "Magnetic Anomalies and the Paranormal" in *JSE 26*:4 (Ralphs 2012), John D. Ralphs notes that correlations have been found between geomagnetic fluctuations and hallucinatory visions, poltergeists, PK phenomena, and ESP. Ralphs argues that ". . . it is a distinct possibility—indeed, a definite probability—that the active agent in most such cases is NOT the magnetic fluctuations themselves, but the cosmic rays that cause them." Ralphs describes the nature of these cosmic rays emitted from the Sun: He asserts that a Coronal Mass Ejection (CME) is a stream of cosmic rays that "can be imagined in terms of a gigantic volcanic eruption ejecting millions of tons of this electrically charged 'dust' at speeds in excess of four million miles per hour." Ralphs further asserts that these cosmic rays create the Northern Lights (Aurora Borealis).

The Sun does indeed emit cosmic rays, which can travel at the speeds approaching the speed of light, but these are not the constituents of a CME, and they are not responsible for geomagnetic fluctuations. Geomagnetic fluctuations, and the Aurora Borealis, are caused by a plasma of low-energy particles emitted from the Sun, traveling at between 50 and 1200 km/sec (Kivelson & Russel 1995, Campbell 2003, Kallenrode 2004).

Correlations between mental phenomena and geomagnetic fluctuations cannot be due to cosmic rays, as the latter arrive at the Earth one to three days before the plasma responsible for the fluctuations.

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LETTER TO THE EDITOR

Response to Adrian Ryan

Thank you very much indeed for your helpful correction to my *JSE* article "Magnetic Anomalies and the Paranormal" (Ralphs 2012). I am not surprised at the error, as my last serious consideration of cosmic rays was seventy years ago (I qualified in 1943). But I hope that you agree that it does not affect the point I hoped to make, that the simulation of magnetic anomaly effects using magnetic fields only cannot claim to be complete or authoritative.

Can I take this opportunity of asking your opinion on an associated matter on which I have failed to find any authoritative information? I maintain that study of the actual generation of action potentials in a neural axon of the brain establishes that the movement of electrical charges is almost entirely radial to the axon, so that the external magnetic fields generated at either end of a diameter will be opposed, and tend to cancel out. I suggest that the very low level of magnetoencephalography fields detected is because they are received only from epiphenomenal divergencies from the mathematical model (such as bends or junctions). This would account for the very low level of magnetic changes detectable external to the skull.

Again, thank you for your comment.

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Ralphs, J. D. (2012). Magnetic anomalies and the paranormal. *Journal of Scientific Exploration*, *26*(4), 781–790.

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BOOK REVIEW

The Risks of Prescription Drugs, edited by Donald W. Light. Columbia University Press, 2010. 184 pp. $45 (hardcover), $15 (paperback). ISBN 978-0231146937.

The traditional concern of anomalistics has been to demonstrate that its interests are substantive and worth attending to, within a wider social context that has treated mainstream science as a touchstone of an authenticity that anomalistics still has to earn. Increasingly over the last few decades, however, mainstream science has become less and less trustworthy as a result of excessive competition and concomitant dogmatism (Bauer 2012a). This has happened quite markedly in medical science and practice, and *The Risks of Prescription Drugs* describes this retreat from reliability.

Complementary and alternative medicine have traditionally been decried by mainstreamers and their groupies on two related grounds: that any claimed successes of alternative treatments can be ascribed to the placebo effect rather than to the treatment, and that eschewing mainstream treatment robs patients of health-safeguarding, possibly life-saving benefits.

The Risks of Prescription Drugs takes the wind out of both sails. Eschewing rather than accepting drug-based mainstream treatment can safeguard health and save lives. Moreover, the ability to summon the placebo effect is no mean feat and brings tangible benefits (Brody & Brody 2000).

The Risks of Prescription Drugs covers much ground succinctly but comprehensively and with full documentation. The subject is of concern for everyone, because prescription drugs have become so widely used. The context for this pandemic of promiscuous prescribing includes:

➢ **Vastly expanded definition of illness**: 1) Doctors used to be consulted when there was obviously something wrong, when one felt ill. Nowadays, by contrast, illness is defined by biomarkers, surrogates for clinical condition: blood pressure, blood sugar, EKG, etc. Departures from population averages for such numbers are defined as illness or potential illness even when the "patient" seems in perfectly good symptom-free health, and drugs are prescribed to bring about population-average numbers for the individual. 2) Normal conditions are redefined as illness, in particular natural changes with age. Menopause is defined as something whose effects should be eliminated, for example.

➢ **Drugs for all seasons**: Infectious diseases have been treated effectively with drugs, antibiotics that kill intruding bacteria or parasites selectively enough that host tissues remain relatively unharmed. The major virus-borne diseases have been stymied effectively through vaccination. Now that infectious diseases are largely controlled, drugs and vaccines are being applied against non-infectious conditions that have been defined as illness even though they are normal accompaniments of living and aging (Bauer 2012b).

➢ **Symptoms and not causes are being treated**: Blood pressure and other such measures are *markers*, not ailments. Yet deviations from population-average numbers are defined as ill health or potential ill health and markers are treated as though they were causes.

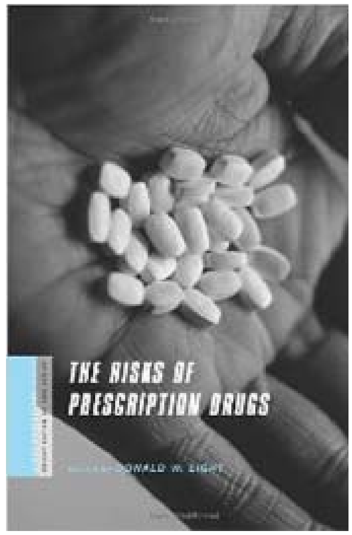
One of the authors (Howard Brody) of *The Risks of Prescription Drugs* is an MD, the others are sociologists specializing in economic or medical issues. With copious citing of the research and review literature, they describe the parlous state of contemporary medical practice: dominated by the prescribing of drugs that are often unsafe, often not as good as those they replace, and exorbitantly and unwarrantedly expensive. Regulation is quite inadequate, in part because the pertinent agencies are specifically hobbled by laws and by gross underfunding.

Those circumstances are even less excusable since many books over the last decade or so have described various aspects of this state of affairs—books by well-informed people: editors of medical journals and physicians both in academe and in medical practice as well as trustworthy science writers and journalists (Bauer no date). But these comprehensive critiques have so far had no discernable effect. Pharmaceutical companies have been able to bend Congress and federal agencies to their own benefit. Sooner or later this dysfunctional bubble must burst, under pressure both from economics and from increasing recognition of the harm done by "side" effects of drugs that should never have been prescribed, some of which should never have been approved in the first place.

Here are some of the salient points:

➢ Senator Estes Kefauver held hearings in the 1950s that revealed deficiencies and dangers that have not been reduced let alone rectified since then (pp. 47–48.). Retroactive evaluation revealed that hundreds of drugs in common use were not effective; they had been approved on the basis of unsound submissions by the manufacturers (pp. 50–51).

➢ Political interference has emasculated drug evaluation (p. 51 ff.). The definition of normal conditions as illness and associated advertising has meant that ~80% of adult Americans and ~50% of children take at least one prescription drug (p. 24). About 20% of seniors take ten or more.



— Menopause was declared a treatable disorder many decades ago. Over time it turned out that the supposed cure could be worse than the supposed disorder (Chapter 5).

— Diagnoses of Attention Deficit Hyperactivity Disorder have increased from less than 1% two decades ago to nearly 8% by 2003. Two million children take stimulants to treat this condition (p. 93).

— About 10% of teenagers are diagnosed as having a Major Depressive Disorder and are chiefly treated with drugs (p. 94). But the efficacy of antidepressants is minimal, e.g., 65% vs. 58% for placebo, among pre-teen children (p. 95).

— Prescriptions of psychotropic drugs increased seven-fold during the 1990s (p. 100). "Social anxiety disorder" is said to affect six times as many people as a decade earlier (pp. 103–104).

➢ Drugs are prescribed not only for manifest illness, but also for people said to be at risk of illness for such reasons as elevated blood pressure or genetic predisposition. If that trend continues, the results could be disastrous (pp. 111–112).

➢ Clinical trials that form the basis for drug approval are typically too small and too short to properly test safety and efficacy (p. 7). Trials are also readily biased in many ways (pp. 15–16). That they are biased in actual practice is demonstrated by the fact that trials funded by a given company almost always yield a result favorable to that company's drug (p. 81).

— The Food and Drug Administration often approves drugs championed by manufacturers even against the advice of its own in-house experts. Well-known drugs bearing serious risks or little advantage over others include Avandia, Bextra, Celebrex, Crestor, Lamisil, Levitra, Singulair (p. 6).

— Statins do not have the claimed benefit of reducing risk of cardiovascular disease by lowering cholesterol levels. They do have seriously damaging other effects (Chapter 3).

— Most new drugs are no better than the ones they replace. Often they are not as good, as well as often more toxic, but always they are more expensive. Only 2–3% are significant advances, and only ~10% represent any improvement at all (pp. 5, 11).

➢ The frequency of adverse events caused by prescription drugs was ten times greater in 2005 than in 1985 (p. 3). The number of serious adverse events reported to the Food and Drug Administration is on the order of only 1% of all the occurring serious adverse events (p. 3).

— After toxic effects become known, warnings on labels are slow to appear, for reasons both of bureaucratic inertia and influence exerted by drug companies (p. 11). Drugs continue to be marketed even after serious toxicity has become known to manufacturers and the FDA, for example with streptomycin or Vioxx (pp. 46–47).

— One estimate claims the lifetime risk of severe injury from a prescription drug to be 26 in 100. By comparison, the risk from an auto accident is 2 in 100 (p. 54).

➢ Most doctors get most of their information from drug companies, through visits from sales representatives, and from advertisements in medical journals (p. 46). Many doctors do not recognize well-established side effects of drugs, for instance muscle aches and cognitive impairment associated with statins; patients remain uninformed and at serious risk (p. 10).

➢ Drug companies spend far more on marketing than on research (p. 5). They break laws against marketing off-label use (p. 22) and continue to pay enormous fines—hundreds of millions of dollars—because their profits from such marketing are much greater (p. 6).

➢ The national costs of prescription drugs are enormous, owing in part to Congressional deference to pharmaceutical companies (p. 26 ff.). Sales of psychotropic drugs increased tenfold in a decade to ~$6.7 billion by 2001 (p. 110).

The weakest part of *The Risks of Prescription Drugs* is the Epilogue, which suggests strategies to correct the present dysfunctions in a striking table (p. 159) that contrasts present-day practices with what would serve the public good. The suggested strategies amount to eliminating conflicts of interest, relying on independent evaluations of drugs, and funding federal agencies well enough to make that possible. It seems highly unlikely that the political will for this can be summoned in the foreseeable future. Nothing will be done until the influence exerted by the pharmaceutical industry is curbed, but that industry has swamped Congress with lobbyists and campaign contributions. It is hardly an exaggeration to say that Pharma has the best Congress that it could buy.

In addition to its important specific substance, this book also illustrates the general applicability of the aphorism that war is too important to be left to the generals. On any matter of public policy, the specialists should not be the decision makers. They see trees but not the landscape, and they suffer inevitable conflicts of interest, intellectual as much as material. Historians and sociologists have discerned fatal problems with medical practices that remain unacknowledged by professional medical associations and official agencies. Some individual specialists do try to raise the alarm over inappropriate mainstream professional practices, of course, but these minority voices are generally ignored in this era of professional dogmatism (Bauer 2012a).

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BOOK REVIEW

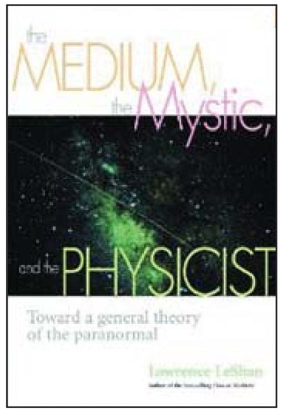
The Medium, the Mystic, and the Physicist: Toward a General Theory of the Paranormal by Lawrence LeShan. Allworth Press, 2003. 320 pp. $22.94. ISBN 978-1581152739.

The Medium, the Mystic, and the Physicist, first published in 1974, reads as if it were published this week. The premise of this classic remains relevant to contemporary parapsychologists and to those of other disciplines taking part in the study of consciousness. LeShan describes his book as, "a story of adventure, as a search for the meaning of impossible events," giving examples of anomalous perceptions from laboratory experiments, spontaneous cases, mediumistic cases, and from the psychotherapy setting which beg for a theory to explain them. This book describes his theory of the paranormal and the adventure of shaping this theory.

LeShan interviewed serious sensitives who had a high frequency of paranormal events in their lives. During the years of LeShan's inquiry, it became clear that at the moment when paranormal information was acquired (when "telepathy" or "clairvoyance" or "precognition" was happening), sensitives were reacting to the world as if it were constructed and "worked" differently than what we normally believe it to be. At those moments they used a different metaphysical structure of the world than our ordinary, everyday, metaphysical structure.

LeShan identified two other groups, mystics and Einsteinian physicists, who reached the identical conclusion: that there are two ways of being in the world, which LeShan calls the Sensory Reality for our usual way of knowing, and the Clairvoyant Reality, independently described by these groups where information is gained other than through the senses, with a unity of subject and object and an experiential quality of deep wholeness. We get a chance to see this, almost intimately, by reading Eileen Garrett's sessions with LeShan and in quotes from other sensitives, mystics, and physicists, bringing the Clairvoyant Reality within our grasp.

Out of these findings a theory of the paranormal emerged. This is based on the idea that each metaphysical system permits certain activities and events (which are "normal" when in the system) and does not permit other activities and events (which are therefore "paranormal" when you're in it). In the everyday metaphysical system (the "Sensory Reality"), ESP is "paranormal." In the other system (the Clairvoyant Reality"), ESP is "normal." ESP-type events occur when an individual is relating to the world as if its metaphysical structure were that of the Clairvoyant Reality. LeShan



sees this theory accounting for most of the data that we have in parapsychology.

LeShan chose to use "psychic healing" in a practical test of his theory of two "realities," by learning and teaching the psychic healing ability in order to access the Clairvoyant Reality. He categorized different types of healing based on the behaviors that healers felt were related to the healing effect and used one particular type coined *Type 1*.

Type 1 healing is a process where the healer goes into an altered state of consciousness in which s/he experiences herself or himself and the healee as one entity. There is no attempt to "do anything" to the healee, but simply to meet her or him, to be one with, to unite with. The healer is focused by love, by caring, by *caritas*, on the healee: This is an essential factor, and at this moment of intense knowing, of being one with the healee, at this "ideal organismic condition," both healer and healee exist at home in the universe in such a way that the healee's self-repair system functions with greater efficiency. Under these conditions, there are sometimes positive biological changes.

He chose a series of meditations and exercises that are used to facilitate the shift in consciousness to the Clairvoyant Reality. They are designed to strengthen the structure of the ego, then loosen the individual's usual concepts of dealing with space, time, the location of the self, etc., and to make her or him emotionally aware of alternative valid ways of conceptualizing in these areas. The final step is to allow students to move in a step-by-step progression until one arrives at the altered state of consciousness theoretically associated with psychic healing.

LeShan also identified a Transpsychic Reality and specifies differences across the Sensory, Clairvoyant, and Transpersonal Realities. *The Medium, the Mystic, and the Physicist* ends with a discussion on *"a new note on a work in progress"* and 101 pages in the appendices of parapsychological literature relevant to his thesis.

At the present time, Type 1 healing is trained and practiced in five-day introductory seminars and three-day advanced seminars. Several double-blind studies have been done on Type 1 healing with promising results. These further support LeShan's theory and belief in parapsychology's value today as we learn more about human potential.

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BOOK REVIEW

Controversies in Archaeology by Alice Beck Kehoe. Walnut Creek, CA: Left Coast Press, 2008. 256 pp. $29.95. ISBN 978-1598740615.

Anthropologist/archaeologist Alice B. Kehoe not only has a solid record of empirical research and scholarly publication but is also known for her critiques of American archaeology and archaeologists (notably, Kehoe 1998, in which she does not shrink from calling spades spades). Further, certain of her interests and ideas have involved some of the most contentious topics in the field. Thus, she is a "natural" as author of a book on *Controversies in Archaeology*. This particular volume is aimed at college-level courses but is also informative for a broader audience. It tells much more than what is or has been controversial; it gives extended glimpses of many general changes that have occurred in anthropology's subfield of archaeology over the past half-century or more. Numerous case studies are provided. A major point is that differing values and presuppositions among archaeologists and others commonly lead to differing and often conflicting interpretations of the data and even to divergent opinions as to what data are looked for and at.

In her opening chapter, "The Past Is Today," she states, "The fun part of science is finding unexpected data and proposing an explanation that startles people" (p. 11). Although not true for conventional scholars who stodgily confine themselves to mainline, non-controversial investigation, the excitement of unusual data and paradigm-shaking interpretations does provide spice for many more adventurous scholars (a few of whom, it must be acknowledged, have overreached). Such novel data and interpretations also inevitably generate argument, because they call the accepted into question.

Professor Kehoe points out that some former core ideas in the field of archaeology have included concepts that have been more or less discredited in recent times. These include 1) Late Pleistocene Siberian hunters following megafaunal-game animals across the then-dry Bering Strait and subsequently moving southward into interior America via an ice-free corridor between the glaciated Rocky Mountains and the retreating Laurentide ice sheet of eastern Canada; 2) a lack of development of true pre-1492 Native American civilizations in what is now Anglo-America; 3) the risen Holocene oceans' having caused a total isolation of the pre-Columbian New World from post-Pleistocene interaction with the Old World until (and then, insignificantly) the eleventh century A.D.; and 4) a lack of noteworthy interaction between pre-1492 North America north of Mexico and Meso- and South America.

She asserts that many even-earlier core concepts were unconsciously based on Anglo-American racism—for example, that Native Americans were incapable of constructing the impressive erections of the "Mound-builders."

Archaeology/prehistory has highly significant political and economic implications and uses.

> Known pasts are used to draw tourists, often making up a substantial part of the economy. . . . Modern nations assert their inalienable right to their homeland by exhibiting archaeological finds from the territory, proving that people did live there for millennia. . . . [But W]ere the ancient people [really] the forbears of the present population? (p. 23)

Chapter 1 also raises knotty questions concerning who "owns the past" in the form of artifacts. Is it the countries in which the objects originated, no matter how corrupt, unstable, or war-torn, or is it (or should it be) responsible, state-of-the-art museums in safe, relatively stable countries? Should prime treasures—the patrimony of the past—be commoditized by poor local people endeavoring to make a living through illegally (and content-destroying) unearthing and selling the leavings of their predecessors, by acquisitions on the part of art and antiquities dealers, and by sales to private and public collectors?

Other issues Kehoe raises are: the question of artifactual fakes (which are legion); the authenticity of the restorations of certain ancient monuments; and the reliability of, and even the competition between, the interpretations of the pasts involved (e.g., Native American vs. Anglo-American vs. African-American perceptions of sites' pasts). Then, there is the problem of deterioration of sites by excessive visitation, plus the question of access afforded to sacred sites for believers and the perceived profanation of such sites by tourism development (and, one might add, by New Agers' rituals and the like).

During recent decades, ethical issues regarding archaeological digs have engendered burgeoning discussion followed by changes in approach. *Cui bono* issues arise. Site excavations provide short-term paid employment for local people but also involve removal of ancestral treasures, which might be seen either as a local loss of patrimony or, instead, loss of a saleable resource; in fact, a professional dig may inspire the natives to excavate illicitly for their own economic benefit. Local site museums, intended to keep materials in the communities where they were excavated as well as to spur tourist income, are becoming increasingly common.

Chapter 2 is an excursion into "Scientific Method." Kehoe makes the point that because scientists cannot examine all of the infinity of data out there, they must be selective in what they look at; selection is based on what seems likely to be informative in answering questions deemed important. Therefore, theory guides investigation.

Real science, explains Kehoe, relies exclusively on empirical observation, including measurement. It attempts to describe, define, and categorize with precise language and, when appropriate, to utilize visual images to communicate, providing "virtual witnessing." One builds a "chain of signification," from naming through classification to interpretation. Scientific archaeology strives to employ "inference to best explanation." From observed data collected, the archaeologist induces an explanatory hypothesis, which is then tested by means of new observations in order to ascertain the consistency of the hypothesis.

During the 1960s and 1970s, Processual Archaeology enjoyed a vogue. However, its hypothetico–deductive approach did not work, because it began with an explanatory hypothesis and from that hypothesis deduced what data to seek. If the information found fit the hypothesis, that was considered a validation. There was no testing of alternative working hypotheses.

Properly, in interpretation one applies the criterion of probability, based on frequency of occurrence of a phenomenon in other instances. The explanatory hypothesis is revisable in light of new data. "Still, improbable is not impossible. . . . Anything that is physically possible is scientifically possible, whatever the odds" (p. 44). One might add that there is a high probability of *some* improbable occurrences, that Ockham's razor is suggestive not definitive.

The author discusses the process of paradigm shifts. She upholds the necessity of pre-publication peer-review of research to uphold quality but also sees the danger that conventional, conformist thinking on the part of reviewers may stifle dissemination of innovative ideas.

In Chapter 3, "Popular Archaeology," Dr. Kehoe tackles widely credited notions such as the landing of a saucer full of extraterrestrials near Roswell, New Mexico, in 1947. An archaeologist working in the area at the time remembers no unusual occurrences, she says. She may be referring to Herbert W. Dick, who denied knowledge of any landing—a denial termed false by some UFOlogists (Bragalia 2010). (Postscript: 2002 geophysical prospection and archaeological testing of supposed extraterrestrial-spacecraft landing sites on the Foster Ranch, intended to evaluate the sites' potentials, yielded nothing startling. Of the two kinds of physical evidence reported by eyewitnesses, no clear signs of one, an unnatural furrow, were discovered—although after 55 years, geomorphic processes could have obliterated it. Regarding metallic debris, nothing out of the ordinary was found. An old weather balloon was discovered, but its age was estimated to be only about a decade (McAvennie 2004; analysis of a few puzzling specimens had not yet taken place at the time of this publication). For a recent book regarding credible UFO sightings, see Kean (2010). Pollard (2011) asserted that secret Cold War–era highly maneuverable unmanned aircraft built and tested in the Roswell area can account for local UFO sightings.)

Erich von Däniken comes in for negative attention as a consequence of his attributing to spacemen certain Mayan imagery as well as medicine wheels of western North America.

> The problem for archaeologists is that von Däniken and his imitators appropriate actual sites and antiquities, denying credit to the peoples such as the Maya who did create them. In a sense, von Däniken stole the achievements of [Palenque King] Pacal's Maya citizens and the ancestors of the Saskatchewan Indians who scientifically mapped in stone the positions of six astronomical bodies at solstice. (p. 61)

Another topic treated is the fraudulent artifacts whose putative provenience is "Burrow's Cave" near Olney in southern Illinois (which Kehoe does not specifically name), alleged to be the burial site of Alexander the Great and the Ptolemys—all of which she labels "flim-flan" [sic] (in 1992, Russell E. Burrows and Fred Rydholm penned a book, reminiscent of the novels of Edgar Rice Burroughs, about the alleged site). She also discusses the flamboyant University of New Mexico archaeologist Frank Hibben, whose claim that he found Sandia spear points in association with extinct fauna has never been verified by others.

"Pyramid power" is another target of Kehoe's myth-exploding effort. Atlantis earns her skepticism, as well; Plato's story of the sunken city wasn't intended to be history, she explains, but was a parable illustrating the principle that natural disasters can overturn even the finest works of humans. But legends die hard; on the basis of his belief that Tiwanaku, Bolivia, was Atlantis, within the past decade Col. John Blashford-Snell of Dorset, England, built a bulrush raft and traveled down the Amazon tributaries with the ultimate intent of reaching the Atlantic and sailing on to the Red Sea and the Persian Gulf (Blashford-Snell & Snailham 2002).

The "Aryan" master-race fantasy, "creation science," and the claim of the contemporaneity of humans and dinosaurs provide additional butts.

During the 1970s, the practice of "psychic archaeology" gained a few professional adherents. Certain self-proclaimed psychic individuals purported to be able to apply extrasensory perception (ESP) to archaeological questions, as in reconstructing what ancient life was like at sites. Such persons employed psi, defined by Kehoe as "a tenuous ability to perceive more than most other people can," acknowledging "certain persons' unusually acute perception and recognition" (p. 67). However, psychics'

> pronouncements fall outside the range of science's ability to confirm or debunk . . . . [P]sychic time travel, extraterrestrials, pyramid power, secret codes and lost cities [like Atlantis] defy reasoned efforts to explain how much we do know about the human past. (pp. 68–69)

(Readers of the *Journal of Scientific Exploration* no doubt have a different perception of the scientific testability of some of these phenomena.)

All in all, observes Kehoe, many popular books and television specials falsely promote controversies in their expositions, controversies that do not really exist *within* the field of archaeology.

"America's First Nations" is the subject of Chapter 4. A "First Nation," for the author, is "one of the nations first in a territory, before European invasions." (She presumably does not mean the usually unknowable very first inhabitants but, rather, the historically identifiable pre-European denizens—descendants of whom may still survive.) "The most serious legitimate controversy in contemporary archaeology is the question of whose country America is" (p. 79). She describes the complex issues respecting scientific versus indigenous values, First Nation sovereignty, control of sites and artifacts, and competing interpretations. As an example, she cites the clash between scientists and Native Americans over disposition of the remains of the Late Pleistocene Kennewick-man skeleton unearthed in Washington State. Despite the challenges, Kehoe lauds the usefulness of archaeologists' studying the local ethnography as well as the archaeology, and the growing collaboration between Indian nations and archaeologists (and there are now numbers of Native American archaeologists); many Indian Nations hire archaeologists for purposes of heritage-preservation.

As an example of the synthesizing of Anglo-American written history, Native American oral history, and archaeology, she cites the historical reconstruction of what took place at the 1876 Battle of the Little Bighorn ("Custer's Last Stand"). Other instances of cooperation mentioned include Hopis and archaeologists cooperating to identify prehistoric Hopi sites in southeastern Arizona's San Pedro Valley, which carries implications for artifact-repatriation under the Native American Graves Protection and Repatriation Act (NAGPRA); and archaeology providing a basis for Blackfoot land claims to mountain hunting zones under the 1946 Indian Land Claims Act.

Missing in this discussion of controversies are the many cases of overlapping land claims among the various Indian "tribes" during the land-claims cases of the latter half of the twentieth century, the most notable of which involved the conflicting claims between the Hopi and the Navajo of northeastern Arizona (see, e.g., Brugge 1994). To the different claimants and to the courts, ranks of archaeologists often offered conflicting interpretations.

Kehoe discusses the various regrettable errors and exploitations that occurred under U.S. Indian policies of the nineteenth and twentieth centuries. She also treats archaeologists' former blasé attitudes concerning exhumation, examination, and display of Native burials and grave goods on the often-false assumptions that traditional culture had disappeared upon the settling of the Indians on reservations and that the deculturated Natives no longer cared about their primitive, pagan forebears. The 1990 passage of NAGPRA reflected changing perceptions and required non-Native holders of sacred and funerary objects—such as museums—to repatriate them to identified Indian nations or individuals who had legitimate claims to them. The process of repatriation is an ongoing one, fraught with controversies. Some Indian Nations have accessioned the objects and human remains awarded to them, others have elected to allow museums to curate them safely and respectfully, the Indians taking them out, if at all, only periodically for ceremonies.

For the acquisition of indigenous information, mutual trust must be established between scholar and informant.

> Those of us who work with First Nations take time to establish a clearly reciprocal relationship, fulfilling requests from them and sharing data and records. (p. 95)

Oral history can be very useful for its potential factual content but must be approached with cautious regard for possible distortions and omissions reflecting religious beliefs, personal or group agendas, and memory lapses.

Chapter 5 introduces us to "Finding Diversity." Beginning in the 1960s, archaeology experienced a rising consciousness and appreciation of human diversity and individual human agency reflected in the archaeological record, in part as a reaction to the Processualists' sanguine belief in the discoverability of deterministic universal laws of cultural evolution. This development paralleled the emergence of "alternative lifestyles" in wider society as well as increased recognition and appreciation of, and focus on, societies' diversity in terms of ethnicity, age, gender, abilities, wealth, roles and statuses, and so forth. Kehoe contrasts this multifaceted, subcultural, and individual-agency view, which she associates with wider social movements for reduced sociological and political constraints as well as tolerance for differences and for individual idiosyncrasy, with the universal-laws approach of the Processualists, to whom she attributes a fascist yearning for a tightly regulated and controlled society based on universal principles.

This general atmosphere, plus research conducted in connection with the post–World War II Indian Land Claims litigation, plus the rise of cultural-resource-management (CRM) salvage archaeology, fostered the emergence of *ethnohistory*, which involves the synthesis of written history, oral history, and archaeology in the service of reconstructing the pasts of formerly non-literate societies, with much more emphasis than previously on the Native point of view—and, often, under the aegis of the Indian nations themselves.

Beginning with a 1989 Chacmool conference at the University of Calgary, gender archaeology has become prominent, in which evidence for differing gender roles is explicitly searched for in the archaeological record in contrast to simply making assumptions about universal gender roles.

Another development of the past few decades regarding diversity, and one fueled particularly by class-struggle–oriented Marxist archaeologists, is interest in the archaeology of formerly often-ignored non-elite sectors of past societies, humble often marginalized sectors such as the poor, prostitutes, slaves, and the like. This shift in traditional emphasis also reflects a shift in the field from museum-supported, spectacular-object–oriented, institution-directed digs to grant-supported, academic examination of past ways of life and to CRM salvage archaeology, which looks for everything that archaeology can tell us.

Chapter 6 is "Religion and Archaeology." There, various topics are covered:

- Archaeology has shown Stonehenge to be clearly pre-Druidic, contrary to the suppositions of neo-pagans.
- Later archaeologists, such as Cynthia Eller, have shown the suppositions of the (unsought) New Age followers of the late archaeologist Marija Gimbutas who worship the "Goddess" who supposedly reigned in pre-patriarchal Neolithic Çatal Höyük, Turkey, are based on no convincing evidence (see review by Jett 2011b).
- The term *shamanism* has been stretched to the breaking point. True shamans are community priests who go on drum-induced trance journeys to the land of the spirits and who are confined to Siberia and northern North America. Shamanism cannot be used as a global explanation for rock art, as some have endeavored to do especially during the drug-using 1960s and 1970s. Different rock art reflects

different purposes and reflects different mental states, not just the ecstatic visions of religious leaders.
- The *Book of Mormon* is a religious but not a scientific source for belief.

With the rise of evolutionary theory beginning in the mid-nineteenth century, scholars have increasingly questioned aspects of the *Holy Bible*. As more and more finds by biblical archaeologists—who initially intended to unearth artifactual confirmation of the scriptures—proved to be at odds with that book, numbers of these researchers tried to become more objective and renamed themselves Syro–Palestinian archaeologists. Kehoe mentions questions that have been raised concerning the destruction of Jericho, the numbers of people involved in the Hebrews' Babylonian Captivity, and the existence and nature of kings Saul and Solomon. She cites evidence of a pre-Captivity association between Yahweh and the Canaanite deities Baal and Asherah, even, sometimes, with the latter goddess being seen as Yahweh's consort.

Kehoe concludes that science and religion are not at war but are different realms: Science deals only with the natural and observable, whereas religion is concerned with the supernatural and hidden. She does not say what happens when the two make conflicting assertions, such as creationism versus scientific evolutionism.

Chapter 7 involves "'Diffusion' versus Independent Invention." Scholars have been remarkably resistant to the idea that humans could have crossed the oceans before A.D. 1492 (or A.D. 1000) and could thus have been in a position to influence folks on opposite shores. Kehoe terms this a conflict between "dogmatic orthodoxy and common sense," feeling "that the model of intermittent contacts best explains the cited features of indigenous American cultures" (p. 140).

She gives examples of good candidates for contacts. One is for Polynesians reaching the Pacific coast of the Americas. Evidence includes Oceanian-looking sewn-plank watercraft—the *dalca* in Chile and the *tomol* in Southern California. The name for the latter appears to be derived from the Polynesian language. And in southern Chile, late-Pre-Columbian bones of Western Polynesian chickens have been excavated. Too, the sweet potato, a South American domesticate, appears in the pre-European–contact archaeology of Polynesia and carries a South American name, *kumara*. (Since the appearance of Kehoe's book, an important compendium volume on this topic has been published: Jones et al. 2011.)

Historically, people of European, particularly Northwest European, origin have felt that they were racially superior and mentally and culturally the most evolved of humans, but this view has increasingly been repudiated, and non-European archaeologists have been making their abilities manifest. Too, technological ascendency has characterized different societies over time; until the eighteenth century, China led the West in this realm. And in ancient times, it was northern Europeans who were relatively "backward" and who traveled to, traded with, and took goods and ideas home from, the Mediterranean civilizations.

Joseph Needham's magisterial, multivolume *Science and Civilisation in China* and his other publications provide multitudinous examples of cultural diffusion within Eurasia and beyond. Needham and his collaborator Lu Gwei-djen set forth criteria for assessing the probability of diffusion explaining any particular case of distant occurrences of culture traits. The first criterion was "collocation," that is the numbers or complexities of traits or trait complexes found in the two areas; the more numerous and/or elaborate, the greater the likelihood of diffusion rather than independent development having taken place. The second criterion was geographic distance; but great, difficult-to-traverse distances separating occurrences can be evidentially countered by demonstrating that travel actually happened or that the means of travel—e.g., ships, camel caravan—were present. The third criterion was chronological congruences versus gaps; but time gaps in the record are not definitive in proving independence because they may reflect as-yet-undiscovered evidence and/or the tradition having been carried on in perishable materials that have not survived to the present for archaeologists to find. "[I]n archaeology, *lack* of evidence does not *prove* a phenomenon never existed" (p. 147).

Kehoe presents the example of the Chinese practice of block-printing texts onto paper having stimulated the fifteenth-century German Johann Gutenberg to rework this concept and to develop printing with movable type. She observes that nomotheticists (universal-law seekers) have difficulty in dealing with the complicated particulars of this kind of diffusion of trait complexes. Further, nomotheticists' search for regularities in cultural evolution is confounded by the fact that, globally, different peoples have devised divergent solutions to comparable survival challenges; different histories yield different outcomes.

Sometimes, new data have required revision of diffusionist hypotheses. An example is John E. Clark's re-examination of James Ford's earlier conclusion that the mound-erecting, incised-potterymaking American Formative cultures, ranging from Louisiana to Peru, represented waves of diffusion as a complex that commenced some 5,000 years ago. On the basis of better dates and other considerations, Clark instead later concluded for multiple movements of various groups and traits at different times. Debated, too, is whether there even was a single Mesoamerican-civilization "mother culture"—the Olmec of the San Lorenzo region of Mexico's Gulf coast (Olmán)—or whether Olmec was just one of several interacting polities of the time; whether Olmec-style ceramics found widely in Mesoamerica represent exports from Olmán or, rather, local emulation of the Olmec style (many of the extra-Olmán sherds are made with Olmán clays, a fact that favors the export claim).

Much cultural and biological evidence for early transoceanic contacts has been forwarded. Among the former, are: the bark-papermaking complex and ritual cutouts made of the paper, wheeled figurines, and royal-purple shellfish dyes (for better evidence on dyestuffs, see Jett 1998). I would add my own work on the distribution of the blowgun complex (Jett 1970, 1991) and of resist-dyeing methods (Jett 1999). Other traits include the jade complex, tiered pyramids whose levels represent the layers of heaven, Hindu-style postures and hand positions, and, especially, a complicated calendar system (Kelley's 1981 manuscript on calendar comparisons, cited by Kehoe, is currently in press, in *Pre-Columbiana*). Some Asia–America indigenous cultural sharings may reflect not pre-Columbian contacts but, rather, post-1500 transpacific links such as the Manila galleons that plied between Mexico and the Philippines.

Drawing upon an earlier book of hers (see review by Jett 2011a), Professor Kehoe summarizes the new evidence that the much-reviled, supposedly fraudulent, Kensington, Minnesota, Norse runestone, dated 1362, is in fact genuine.

Even more compelling than cultural commonalities is biological evidence. This is in the form of organisms—cultivated plants, intestinal parasites, and so forth—that were shared between the hemispheres in pre-Columbian times but which are very unlikely to have been able to travel across oceans or via the Arctic. An astounding compendium of such evidence assembled by anthropologist John L. Sorenson and geographer Carl L. Johannessen (2009) is the latest and greatest of the works that have appeared on this topic (Kehoe cites an earlier version). Of the plant evidence, some of the most striking is the finding of residues of nicotine and cocaine in Egyptian and other mummies, implying access to the American domesticates tobacco and coca (see also Jett 2003–2004).

Psychologically, scholars who grew up being instructed that Columbus was first are like small children—or adults—who vehemently object when sung a variant version of a song or told a variant of a story. Kehoe writes,

> Most archaeologists don't want to think about this evidence; it makes them uncomfortable to discuss data drawn from research areas outside their own studies, and it's hard to overturn the reigning paradigm that "primitive people" couldn't cross water "barriers." (p. 159)

Although she predicts a future paradigm shift in favor of contacts and diffusion, Kehoe observes that

> Not many archaeologists were experienced sailors and Western culture tends to see water travel as more dangerous than travel by land. Sailors, on the other hand, will maintain that open sea is less hazardous, though coastal waters, they do admit, are often perilous. (p. 153)

Characteristically, most archaeologists have been only vaguely aware of the capabilities of the three major non-Western traditions of watercraft—of which the author gives brief descriptions. To demonstrate the possibility of transoceanic contacts, she provides (on pp. 155–157) a table of some experimental voyages (by Thor Heyerdahl and by Tim Severin, described on pages 154, 157–158) of replica ancient watercraft as well as of numerous modern, small-boat ocean crossings, many solo and in tiny, flimsy craft.

Kehoe grants that some diffusionists have gone overboard, as it were, with poorly supported but dramatic claims, mentioning as examples the early twentieth century anatomist/cultural historian G. Eliot Smith (who is currently undergoing intellectual reassessment; see Smith 2011 and Crook 2011) and the late-twentieth century's avocational epigrapher Barry Fell.

Chapter 8 of the book treats "What People before Us Could Do: Earlier Technology." In the 1960s when monuments like Stonehenge were first seriously attributed astronomical-observation functions, there was wide and sometimes derisive objection; it was perceived that the ancients could not have possessed such sophistication. In the meantime, however, archaeoastronomy has become a recognized, mainstream aspect of archaeology. Kehoe expostulates on a site that she and her late husband Tom Kehoe studied, southern Saskatchewan's Moose Mountain "medicine wheel," which dates to the mid-first millennium B.C. It exhibits five sightlines to bright stars and another to the sun at solstice—perhaps to signal the moment for the annual rendezvous of the tribes.

Kehoe goes on to describe pyramids in Egypt, Mexico, and Illinois, pointing out solar alignments as well as mentioning the still-debated means of lifting massive stones up the rising Egyptian edifices during construction, the provision of adequate drainage, and so forth.

She also touches on Old and New World earthen mounds, including giant geometric Hopewell enclosures with lunar alignments, lengthy avenues, and big burial mounds in Ohio, and notes those people living in simple structures and settlements despite the sophistication of their geometric and earthwork-building skills and their access to raw materials from as far away as Yellowstone, the Gulf of Mexico, and the Southern Appalachians.

The author includes a box on the "mysteries" of Easter Island, which become less mysterious as one looks at them in context. The settlement of this remote speck of land was part of a Pacific-wide Polynesian colonization thrust, and the giant statues can be transported overland and set upright using simple mechanical means.

The sources, timing, and routes of the initial peopling of the Americas have long been among anthropology's most contentious areas of investigation. The long-dominant notion that the first arrivals were the ancestors of the creators of the Clovis spearpoints who followed big-game animals across Beringia into North America has recently been dealt a seeming deathblow by the excavation of several pre-Clovis American sites, by the recognition that the "ice-free corridor" was impassible at the time, and by the ascendency of the boat-borne, Pacific-coastal–migration concept. Kehoe looks at these issues and also discusses the particularly controversial hypothesis that Clovis ancestors arrived not from Siberia but from Solutrean-occupied areas in Iberia, migrating along the edge of the Late Pleistocene North Atlantic ice pack (see Jett 2012; a major book on the subject, Stanford & Bradley 2012, has appeared since *Controversies* was published). Kehoe finds the idea attractive but remains bothered by the current large chronological gap between Solutrean and Clovis.

Regarding possible pre-Clovis sites, Kehoe has reservations about the dating of the Meadowcroft site, in Pennsylvania, but accepts Wisconsin's Chesrow complex, South Carolina's Topper site, Virginia's Cactus Hill, and Texas's Gault site as manifesting occupation a millennium or so before Clovis (Texas's pre-Clovis Buttermilk Creek Complex (Waters et al. 2011) had not yet been reported by the present book's publication date). She seems also to accept the Monte Verde site in Chile as pre-Clovis, while noting that a number of discrepancies appear in the published report.

The book's ninth chapter is "Neandertals, Farmers, Warriors, and Cannibals: Bringing in Biological Data." The forebears of Neandertal and anatomically modern humans (AMH) split off from their ancestral species, *Homo erectus*, some 400,000 years ago (as *H. heidelbergensis*), with modern humans differentiating about 160,000 years ago (nearer to 200,000 years ago, according to more recent estimates, and 300,000 years ago for Neandertals). The AMH and the European and Near East's Neandertalers have been considered to be separate species, but it is not known whether interbreeding took place and resulted in fertile offspring, leading to the eventual genetic swamping of Neandertalers by modern humans. (More recent work has identified Neandertal genes in some contemporary Eurasian human populations (Gibbons 2010), which would suggest that the two lines were but subspecies.) These two species/subspecies long shared

Middle Paleolithic technology and subsistence strategies, but Neandertalers may have lacked well-developed language as well as art (Kehoe feels that Neandertalers' burial of their dead bespeaks abstract thought and language, and some archaeologists do attribute art to Neandertalers (Balter 2012)); also debated is whether more-sophisticated Upper Paleolithic technology was exclusively the product of modern humans or whether some Neandertalers also learned it, from *Homo sapiens*. Kehoe seems to favor technological distinctiveness, at least with the inception of *H. sapiens*'s Aurignacian toolkit.

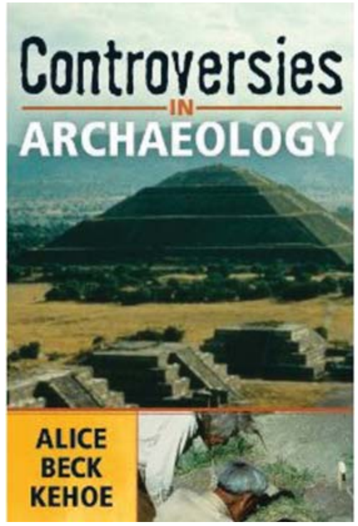
Another topic that has been argued is whether the spread of farming across Europe was a matter of group-to-group transfer, involving little migration (contagious diffusion), or whether Near Easterners carried the technology of food production with them while physically moving into and across the continent (demic diffusion), also carrying Indo-European speech with them. (Genetics now indicates that demic diffusion was, indeed, involved, predominantly by males, who often married local women (Haak et al. 2010).)

Farmers, Kehoe contends, had continuously to acquire and develop more farmland, because rapid population growth and the need to support non-farming occupational specialists required it. However, during Neolithic times, cultivators lacked armies to accomplish this. Further, Indo-European languages share little in the way of horticultural vocabulary, and other words that *are* held in common indicate an origin for the family in the steppes to the north of the Black Sea (for a competing, Anatolian-origin Indo-European theory, see Renfrew 1987). She concludes that farming, conquest, and language-spread were not fully congruent here, and that attributing major language spread simply to the spread of farming (à la Peter Bellwood 2004) is ill-founded. Expansion by force, she concludes, despite a lack of armies, is a stronger explanation.

Attributing warfare to peoples such as Marija Gimbutas's allegedly pacific, goddess-oriented, Neolithic, Middle Easterners or to the ancestral Hopi—"the peaceful people"—of Arizona by authors such as Lawrence H. Keeley has generated much resistance. This was particularly the case when Christy Turner gave evidence that the Hopi had even occasionally practiced cannibalism.

Although acknowledging that commonly evoked simple explanations for warfare, such as scarcity engendered by climate change or by population growth, are probably sometimes correct, Kehoe cautions that these are not necessary conditions.

She forwards the case of the large Cahokia mound complex in southern Illinois, where excavation of a small mound revealed 266 seemingly



sacrificial bodies and rich grave goods; the victims—war captives?—were from distant areas. Kehoe sees Cahokia resemblances to historical Osage practice and even to aspects of southern Mexico's Mixtec culture. She also comments on Chaco Canyon, New Mexico's, connections with the south, involving exchange of turquoise for macaws, ceremonial knowledge, etc., tentatively identifying Cahokia's and Chaco's trading partner as Central Mexico's late-pre-Columbian Toltec culture. Cahokia may have exported foodstuffs, deer hides, and slaves.

Alice Kehoe's final chapter, 10, is on "Competing Theories of Cultural Development." Over time, the general question of the processes of cultural elaboration has probably been the most controversial one in the field, producing dramatically opposed answers. In fact, as in the case of a number of other disciplines, archaeology has been plagued in contemporary times by extreme internecine conflict, disdain for dissenting colleagues, and other forms of divisiveness.

She speaks of the nineteenth-century idea of cultures being superorganisms, entities greater than the sum of their members, with intrinsic tendencies toward inception, florescence, decline, and extinction, observing that this concept is no longer taken literally but only as a metaphor. (Certainly, individuals are in considerable degree conditioned by the cultures/societies into which they were born and in which they exist, which are in that sense superorganic.)

The rise of the Darwinian concept of organic natural selection inspired the notion of *cultural* evolution, a vision that, although waxing and waning and transmogrifying over the years, remains a strong tradition today.

Kehoe points out that it is an error to assume, as many do, that "evolution" necessarily equates with "progress." And she writes of the widely accepted "racist" cultural evolutionary theories of the pioneering nineteenth-century American avocational anthropologist Lewis Henry Morgan ("savagery," "barbarism," and "civilization"), which ultimately fell into disrepute, particularly owing to the meticulous and broad-minded empirical and interpretative work of the German-American Columbia University anthropologist Franz Boas. She also mentions the impact of Karl

Marx's materialist thinking on the influential earlier–twentieth-century Australo–British archaeologist V. Gordon Childe.

The socialist University of Michigan anthropologist Leslie White and his colleague Elman Service rejected Boas's historical particularism and revised Morgan's old ideas to produce the concept of unilinear cultural evolution, which, in turn, set the stage for the 1960s rise of the "New," or Processual, Archaeology. The latter drew upon the environmental sciences—something that was very productive but which, at the same time, led to excessive ecological determinism. Practitioners also tried to be scientific by applying the hypothetico–deductive method, but this proved to be an inappropriate approach to the study of the pasts of cultures. "Their commitment to natural history rather than cultural histories persuaded them that human societies *must* follow evolutionary pathways" (p. 225), when in fact societies tend to *diverge* culturally according to the *particular* influences to which they are subjected.

The New archaeologists ignored Julian Steward's 1950 idea of *multilineal* evolution, a concept more in accord with (largely divergent) biological evolution. Steward felt that different environment types were fundamental in conditioning the basic lifeways of peoples and the sociopolitical natures of their societies but that secondary accretions could come from emulation of practices of other societies and also from internal innovation; Kehoe stresses that humans have always been inveterate travelers and that intersocietal exchanges have always been important for cultural evolution.

Although Processual concepts have been recently repackaged as "evolutionary archaeology," reaction against Processualism was one source of "Post-Processualism," in which the extreme relativism of philosophical Postmodernism included the ideology that reality is subjective, that "science" is impossible, and that all points of view are equally valid—a stance antithetical to any objective, fact-based, best-explanation interpretation.

These theoretical approaches relate to the question of how most human lifeways developed from those relatively simple, sparse-population ones based on the collecting of wild foods to those depending on agriculture and producing populous and elaborate societies and sophisticated technologies. Julian Steward followed a Marx-influenced historian (Karl Wittfogel; not named by Kehoe) who hypothesized that the rise of "hydraulic civilizations" required despotism to manage their critical irrigation systems. However, later empirical investigation of the facts and chronologies showed that this scheme didn't hold up. Subsequent Processual Archaeology tended to perceive the pressure of population growth as the engine for the development of farming, whose expansion, in turn, required an overall coordinating authority and led to state-formation, which involved specialized occupations, including in religion and in military affairs.

No state, the Processualists contended, could arise in the absence of abundant resources, most fundamentally plenty of good farmland. But these notions fail to account for empires established by non-farming Asian pastoralists or for rich farming areas that did not give rise to states. "Ecological determinism fell short as a universal explanation" (p. 230).

Further, the gradually-increasing-complexity model of band > tribe > chiefdom > state, with advancing hierarchies, has been challenged. Mesopotamia's development of writing, standardization, laws, administrative structures, and the like seems to have been in the service of *simplification* of increasingly unwieldy ramifying but poorly coordinated activities, and it took place over a period of only a few centuries, circa 3500 B.C. Further, along the Niger River in Mali, loose settlement and economic heterarchy rather than hierarchy seem to have predominated, manifested as village clusters rather than as nucleated cities, each village with its own production specialty but sharing power within the cluster and the whole operating as an economic and political system in a fashion similar to the functioning of a city.

Kehoe's final paragraph includes the following observation: "Controversies will continue, but there seems to be a heartening reaffirmation of the importance of empirical data. There was a real past out there," which we are endeavoring to understand as actualities and not to just theorize about (p. 235).

One could cavil concerning a handful of her factual statements, but Dr. Kehoe is remarkably knowledgeable and very largely quite accurate in this book. Her prose is aimed at undergraduates and will seem somewhat casual and inelegant to the more mature reader. But the work contains a great deal to ponder, on a great variety of intriguing, debated topics.

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BOOK REVIEW

The Pseudoscience Wars: Immanuel Velikovsky and the Birth of the Modern Fringe by Michael D. Gordin. University of Chicago Press, 2012. 291 pp. $29 (hardcover). ISBN 978-0226304427.

Everyone interested in pseudoscience, fringe science, anomalistics, is likely to benefit from the material in this work. The book has much to say about the social and political context in which heterodox claims about matters of science have flourished and been argued over since the middle of the 20th century. Creationism and Lysenkoism as well as Velikovsky are discussed quite comprehensively and informatively. Attempts within unorthodoxies to maintain a monolithic paradigm are illustrated and analyzed to good purpose.

The Pseudoscience Wars uses the Velikovsky episode as entrée to examine how scientists and society behave when drastically unorthodox claims about matters of science are ventured by non-scientists; the Velikovsky affair "was about science in the postwar public sphere" (p. 22); "an abiding anxiety about science's relation to the 'public'" (p. 47) was central in the reaction of the scientific community.

Much of the material is drawn from the Velikovsky Archives and some of it is likely to be new to most readers; in other ways as well the book illustrates the wide-ranging familiarity with pertinent literature that historians somehow manage to command, enabling them to recapture comprehensively the ambience of past eras.

I should disclose that I published a book about the Velikovsky Affair nearly 30 years ago, and that I'm cited at many places in this book; but on those matters Gordin does not quarrel with what I wrote nor do I quarrel with his takes on those issues—my book was concerned with how scientists ought to have addressed Velikovsky's substantive propositions, whereas Gordin explicitly disavows concern with the correctness or otherwise of Velikovsky's claims. His "goal is historical: to chronicle what happened, to explain when possible why, and to reveal the passions excited by calling something 'science' across this temporal period" (p. 18). This approach, agnostic about the substantive claims, is at once a strength but also a weakness. The strength lies in the elucidation of the influence of social context, which is too often ignored by unorthodox thinkers and their critics, who all imagine their task to be purely intellectual, focusing on the substantive claims. The weakness lies in the fact that how society reacts to unorthodox claims ought surely to vary according to the plausibility or legitimacy of those claims, so ignoring that aspect could distort some conclusions. But no book can do everything, and Gordin has done a major service by addressing important factors that have not before been discussed adequately.

The book begins with the unequivocal assertion that pseudoscience is an empty concept since there exist no viable demarcation criteria by which science can be distinguished from non-science, be it called pseudoscience or something else. Indeed, the very definition of pseudoscience as something that "resembles or mimics" science, "has the trappings but not the essence of science" (p. 202) means that there could not be a definitive way of distinguishing science from its Doppelgänger, pseudoscience. Pseudoscience is just a pejorative term employed when scientists or their groupies feel the enterprise of science to be threatened. There is no commonality among all the matters that have at various times been labeled pseudoscience, other than that they have been abhorrent to some number of scientists or their fans or some part of the scientific establishment. Gordin is also spot on in pointing out Martin Gardner's role in turning "discussions of alleged pseudoscience into debunking crusades" (p. 12).

Immediately one might ask why scientists should ever feel threatened by claims from outsiders, given that science and scientists enjoy high social prestige and that their opinions are granted almost universal deference. Here Gordin provides welcome insights based on the social environment in which Velikovsky caused such a brouhaha in 1950 with the publication of *Worlds in Collision*—claiming that literary sources reveal that Venus was once a comet that induced such cataclysmic events on Earth as the parting of the Red Sea and the falling of the walls of Jericho. Among the important contextual factors were:

- Science had only recently attained its current high status, perhaps chiefly as a result of the World-War-II–ending, atom-bomb development as well as the work on radar, penicillin, and other technological feats that brought much of science out of its traditional ivory tower. (Critical aspects of this fundamental change in scientific activity are summarized in Ziman (1994).)
- Anxiety over keeping the recently gained high status and the generous funding for science that accompanied it was exacerbated by political circumstances: rabid anti-Communism by the House Un-American Activities Committee and Senator Joe McCarthy had harassed quite a few prominent scientists.

Under those circumstances, some scientists over-reacted: threatening the publisher of *Worlds in Collision*, castigating the book while proclaiming they had not read it. The fuss gave Velikovsky much greater publicity than if the book had just been ignored by official science.

In the 1960s and 1970s, some social scientists and some student groups seized on Velikovsky's work as a tool to promote postmodernist, relativist attitudes and anti-Establishment activities. Velikovsky himself never set out to battle with science, he wanted acceptance, and was drawn rather unwillingly into acting as an anti-Establishment guru; however, Gordin suggests,

> Velikovsky served as a middle ground for people of all political persuasions. He was an underdog in an age that had ceased to trust scientists (capturing the Left), but he also promoted deeper study of the Bible (seducing the Right) in a decade whose best-selling work was Hal Lindsey's *Late Great Planet Earth* (1970), an application of biblical eschatology to Cold War geopolitics. (p. 169)

Disparate others also sought to benefit from Velikovsky's coat-tails: a conscientious objector on non-religious grounds (pp. 174–175), a Native American activist (pp. 175–176).

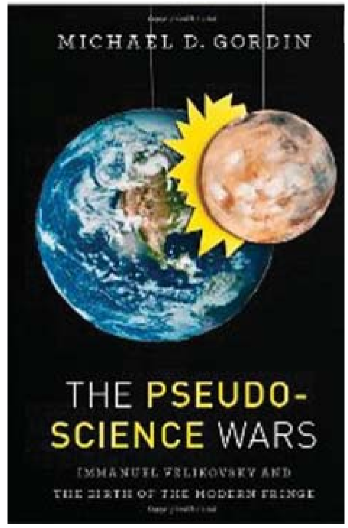
Given the appeal of science fiction to contemporary youth, what did authors of science fiction think of Velikovsky? "Among the most persistent and hostile critics . . . were the luminaries of science fiction" (p. 170). Gordin seems to find this rather surprising, but I do not: Authors of science fiction such as Asimov or Crichton tend to be very knowledgeable about science and good friends of honest science.

A novel and illuminating feature of this book is the comparing of the Velikovsky matter with several other topics, Lysenkoism and scientific creationism in particular. At roughly the same time as *Worlds in Collision* was published, Western scientists had been surprised and disturbed that political machinations and control had led to biology in the Soviet Union being taken over by a pseudoscientific doctrine, Lysenkoism, which rejected genetic theory and claimed to be able to modify heredity directly and deliberately. Gordin doesn't mention it, but in the same era Soviet ideologists had also declared the "idealistic" theories of chemical bonding and quantum mechanics incompatible with Marxist materialist principles, so chemists and physicists as well as biologists were aghast at what damage could result when outsiders were enabled to interfere with science. Thus when Velikovsky came along at the same time as American politicians were harassing supposed Communists in the scientific community, some scientists became perhaps overly concerned that the public might take him seriously.

Gordin's recounting of the Lysenko affair and its impact on American scientists is well worth reading just for its own sake. Not only had World War II brought scientists unprecedented status, it had stimulated them to seek to influence public policy. One outcome was a journal, the *Bulletin of the Atomic Scientists*, which had articles about the Lysenko affair as well as such matters closer to home as atomic bombs and nuclear power stations. The geneticist Dobzhansky clearly recognized that the importance science had assumed "in the lives of individuals and of nations" meant that science would "need popular support and will have to submit to social control" (p. 96). So even a populist like Velikovsky could be seen as a threat to science if he managed to achieve public credibility.

Chapter 4 of *The Pseudoscience Wars* discusses the history of eugenics as showing that something once labeled pseudoscience can rehabilitate itself, segueing into Velikovsky's attempts at legitimation by cultivating interactions with well-established scientists, Einstein in particular. Like all dissenters from mainstream doctrines, "Velikovsky found himself torn between becoming popularized and becoming vulgarized" (p. 162). Charismatic individuals like Velikovsky appeal to people who have a genuine interest in matters scientific and who long for *understandable* science by contrast to the impenetrable abstractions and jargon that permeate modern research; but popularizing morphs easily into, or leads to, unbridled superficial speculation.

Chapter 5 has much of importance for and about people maligned as pseudoscientists, using as a prime example scientific creationism, which also connects substantively to the Velikovsky story at a number of points. The attempt to promulgate alternatives to mainstream science is always fraught with the difficulty of maintaining a common front. Freud's problems with his disciples are well-known. Ufology and parapsychology and cryptozoology have all experienced infighting and schisms. Velikovsky was frequently unhappy with efforts made by people who thought they were supporting his views even as they differed in some respects and in ways that were not congenial to him. Scientific creationism, the brainchild of Henry Morris, experienced similar episodes of self-styled supporters unwelcome to Morris. Creationism and Velikovsky could not avoid all contact because both found support for their views in heterodox interpretations of geology and fossils—albeit their interpretations were totally distinct; Velikovsky was often at pains to distance himself from religious fundamentalism, and Morris tried to hide that some of his citations were the same as Velikovsky's (e.g., p. 145): "If Velikovsky was too 'pseudo' for Morris, creationism was the same for Velikovsky" (p. 153). Velikovsky was also anxious to distance himself from Erich von Däniken (pp. 176–178).



The perpetual threat of schisms is illustrated by the case of Donald Patten (p. 146 ff.) whose idiosyncratic chronology and creationist theory offended both Velikovsky and Morris. Velikovsky was also unhappy with attempts to link his work to that of Wilhelm Reich (p. 158 ff.). In wanting to suppress dissent, Gordin points out, Velikovsky and his ilk can resort to the same tactics that the mainstream deploys against them; thus Velikovsky himself pronounced Patten's book as worthless while acknowledging that he had not read it himself (p. 153).

Gordin's emphasis on social context is also illuminating in pointing to the temporal proximity of on the one hand federal involvement in training scientists as part of the Cold War and on the other hand the drive by creationists to influence science curricula (p. 144). That continuing drive, now under the guise of "intelligent design," will have stimulated the scientific community to be perhaps overly sensitive to any incipient pseudoscience or pseudoscientist.

I recommend this book unreservedly, while noting here a few points on which more deserves to be said. To begin with a perhaps trivial quibble: Jacques Barzun, who happens to be a great hero of mine, did not make a "positive comment" (p. 155) about Velikovsky's work, he merely decried the ad hominem tactics directed at the man.

I think the book has a few non-trivial flaws. The "war" metaphor seems forced in places and didn't really help to illuminate anything for me. There seems an inconsistency between acknowledging pseudoscience to be an empty concept and referring to *the* pseudoscience wars (e.g., p. 158) or even a "coherent conflict of the pseudoscience wars" (p. 4), when there is really no commonality let alone coherence to be found in the controversies over the multitude of things that have been prominently called pseudoscience since the middle of the 20th century: UFOs, Loch Ness Monster, parapsychology, cold fusion, *Chariots of the Gods*, Bermuda Triangle, homeopathy, etc. Though the Velikovsky business does afford a useful entrée into considering reactions to such claims, it hardly foreshadowed or set the stage for those other things, as Gordin seems to suggest in some places; I found no evidence to support the view that "Velikovsky's lived presence—even if only on the printed page—had always been crucial to the waging of the pseudoscience wars” (p. 195). Martin Gardner’s classic enumeration in *Fads and Fallacies in the Name of Science* claims no coherence among all the mentioned topics, nor does it recognize any primacy for Velikovsky. I think any coherence among all those disparate topics arises not from anything inherent in them but from the fact that they all played out within the contextual factors that Gordin describes so convincingly.

Gordin suggests that the Velikovsky Affair might have proceeded differently had contemporary historians focused on Velikovsky’s chronological unorthodoxies instead of treating it as a scientific dispute (p. 74); but the historians who first commented were historians *of science*, no doubt because scientists had jumped into the fray first. I’m also hesitant to accept that “one of the chief activities of the mainstream scientific community is *the process of demarcation itself*” (p. 202); if so, I would opine that this is a relatively recent development as a corollary of science moving into the halls of political power.

I wish fervently that Gordin had eschewed, in the last chapter, “Pseudoscience in Our Time,” the suggestion that threats to science nowadays come not from outsiders but from those members of the scientific community who question the mainstream consensus and who have been declared, by the official mainstream, to be “denialists” (p. 206). He is right to the extent that they are *perceived* as a threat, but his quotations indicate that he accepts that the denialists are not only substantively wrong but wrong even for wrong reasons. For a deconstruction of the use of the term *denialist*, see Furedi (2007). As a denialist myself, I dispute that we “have a common discourse, are funded by a specific set of industries, and are affiliated with particular think tanks with a common (strongly conservative) political ideology” (p. 207). On this—unlike in his comprehensive coverage of the pertinent literature in the rest of the book—Gordin cites just a few partisan sources (including the journalistic rant from Mooney (2005) and the shoddy book by Specter (2009)). We HIV/AIDS denialists exist in schismatic sects, are not funded by anyone, and represent the range of political persuasions from very Green to quite conservative–reactionary, including libertarians of several stripes (I relished sitting between two self-styled libertarians who had diametrically opposite views about Obamacare). Moreover, on issues where “denialism” is shouted, even more than regarding what is labeled pseudoscience, the validity of the evidence for and against the mainstream consensus cannot be ignored, it’s the central point. As I’ve shown elsewhere, if the evidence is respected one must conclude that it is far from settled science that HIV causes AIDS (Bauer 2007) or that human activities have appreciably added to global warming (Bauer 2012).

I hope these caveats will be seen as information for readers of the book, not as detracting in any way from the book's value. After all, it is a high compliment that a book invites and warrants discussion. Gordin's treatment of many important matters is thoroughly scholarly and highly informative, especially as to social context which has typically been given too short shrift in discourse about pseudoscience. That Gordin may not have everything right is hardly a serious criticism, especially since he gets so much so insightfully right.

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BOOK REVIEW

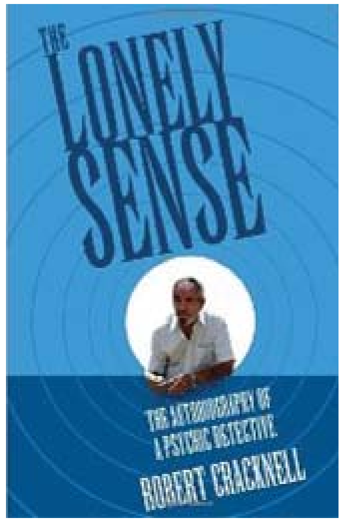
The Lonely Sense: The Autobiography of a Psychic Detective by Robert Cracknell and Colin Wilson. Anomalist Books, 2011. 330 pp. $16.95. ISBN 978-1933665511.

Robert Cracknell is a British psychic who won fame in the 1970s and 1980s as a psychic detective. *The Lonely Sense* is a record of his life, originally published as *Clues to the Unknown* in 1981, here expanded and updated. The book makes interesting reading. Cracknell comes across, as author Colin Wilson accurately remarks in a Foreword, as "totally down-to-earth, blunt, aggressive, and impatient," also "intelligent, honest, and obsessively, almost self-destructively, devoted to his own vision of the truth."

The first chapters describe a difficult working-class childhood, first as a foster child suffering deprivation and then being brought up by his mother and stepfather, and then service in the British Royal Air Force. Early experiences of isolation, hunger, and occasional brutality turn him into a loner, but he also discovers within himself an unexpectedly deep sensitivity toward others, to which he attributes the first stirrings of his psychic awareness. There follows a failed marriage and a stint as a student nurse in a psychiatric hospital.

This is not the story of seeing dead people, commonly described by spirit mediums—there are rather fewer such incidents than one usually finds in such books—but rather of developing and learning to trust his inner intuitions. In fact, after an initial period training as a medium Cracknell develops a marked antipathy to spiritualism, with its focus on afterlife and spirit guides. He is adamant that anyone and everyone is psychic to some degree, and is scornful of the mystique surrounding mediumship—a theme he returns to throughout the book.

There are several exhibitions of his psychic ability. In one striking episode he agrees to try to identify a person who will be sitting in a particular chair at a meeting that will take place several weeks in the future, the venue yet to be decided. He tries to visualize the meeting and tapes himself describing his impressions, then hands the tape to the organizers, who keep it in a safe until the meeting, at which time he stands on the stage as the tape is played. Cracknell feels anxious that he has bitten off more than he can chew, and minutes before the meeting he is convinced that he has failed, as the person sitting in the target chair does not at all match his taped description. But



by the time the event begins, more rows of chairs have been added, changing the target chair, which now to his relief is occupied by the person he visualized. As the tape plays, various other predictions about audience members and their circumstances are all triumphantly vindicated.

Cracknell is reluctant to profit directly from his gift, but eventually finds a use for it in his job as a financial fraud buster, first as an employee of an agency and then running his own business. Later chapters describe how he also uses it to help solve high-profile police cases, which brings him to the attention of the national media. One of the most striking cases takes place in Italy, when Cracknell is persuaded by the father of a kidnapped girl to go out to his Lake Como residence to help the police search for her. He starts with the conviction that it will end with the girl's safe return, and eventually declares that it will be next Friday, five days away, giving time for the girl's somewhat venal father to sell the story of her homecoming as an exclusive to a British Sunday tabloid. The girl is duly recovered on the Friday, although apparently without any direct help from Cracknell himself.

There's a curious encounter with Uri Geller in New York, where he is disillusioned to find his fellow-psychic more interested in fame and money than in using his gifts for people's benefit, for instance in healing. In a street meeting in front of news photographers, Geller does his key-bending trick, which he finds impressive. Cracknell suggests they try a joint public event, he in the UK repeating his chair trick for a future event to be held in New York while Geller in the U.S. attempts to interfere with a computer in London. Geller seems keen. But after the media has been hooked on the idea, he suddenly pulls out without any explanation.

First-person accounts by psychics are always suspect to a degree, in the sense that the reader has no way of determining how accurate the descriptions of successful cases actually are, and what details may have been tweaked or massaged—whether consciously or unconsciously—to make the outcome look more impressive than actually was the case. Self-proclaimed psychic detectives are a particular target for skeptics. In this instance, readers who acknowledge the genuineness of psychic functioning, either from experience or from responsible research, may be willing to acknowledge that Cracknell is a psychic of uncommon ability. It's true that his descriptions show the ambiguities and complexities involved in detection work, for instance having to persuade skeptical policemen to follow up apparently nonsensical hunches and often coming up with predictions that prove to be accurate but that however do not necessarily contribute directly to a resolution. However, in these and other ways the book provides valuable insights into a psychic's inner development and the realities of life in the public eye.

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BOOK REVIEW

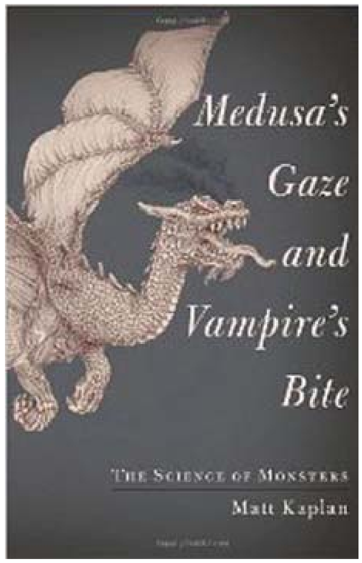
Medusa's Gaze and Vampire's Bite: The Science of Monsters by Matt Kaplan. New York: Scribner, 2012. 244 pp. $26.00 (hardcover). ISBN 978-1451667981.

The Dark Ages lie deep in the past, the isolated folk community has grown almost as rare as the unicorn. Nevertheless it is amid this modern age of technology and enlightenment that we live in the golden age of monsters. They no longer crouch under the bed at night but leap out from the big screen in 3-D. Turn on the TV, pick up a popular novel, and you risk attack by vampires, zombies, dinosaurs, or aliens—while abundant videogames offer an opportunity to fight back. Monsters have stayed with us throughout human history but their persistent and insistent intrusion in modern times poses a phenomenon in need of scholarly attention, and such attention is now very much at hand. A subject that was once beneath academic dignity as mere fashion in lowbrow entertainment or superstitious survivals from the childhood of the species has risen to prominence across multiple disciplines. The grounds of that interest underlie not so much the monsters themselves as a realization that if monsters saturate modern culture, that fact tells us something about ourselves; and even if we no longer need to hunt the primeval forest for our quarry or venture beyond where the map leaves off, understanding the monstrous is no less important, and perhaps all the greater because the source lurks so close to home.

Why do we love our monsters so? Why do we even have monsters, of all things? Don't we know better? The issues inspired by the universal presence of big, ugly, dangerous, and disturbing creatures breaking into the order of the everyday world have given rise to an impressive scholarly literature. Anthropologists inventory the prolific array of monsters recognized by peoples around the world, and consider the social functions these creatures serve. Folklorists and psychologists have pondered the monster as a personification of otherness, an expression of deep-seated fears, or a response to the uncertainties of modern life. Literary scholars long preoccupied with the role of the hero are now giving his adversary its due, while books, articles, and conferences devoted to cinematic treatments of vampires and TV series like *Buffy the Vampire Slayer* have mined the rich representation of these creatures as romantic anti-heroes and outsiders at once alluring and terrifying. Zombies derive their popularity as versatile metaphors for a consumer society wherein everyone performs only mindless routines, while all too many of us join the ranks of the cell-phone variety to stumble around in semi-conscious oblivion. For scholars of religion the monster of ancient as well as modern texts exemplifies the forces of chaos ever biding, always threatening the divine order. Postmodernists look ahead to see our monsters portrayed more and more as human inventions. For example, the monster movies of the patriotic 1950s cast scientists and the military as heroes saving the world from aliens and creatures from the deep, while the countercultural 1960s began a turnaround that transformed the former heroes into the villains creating robots, hybrids, and viruses that threatened the world.

How monsters originate has also been the subject of extensive scholarship. Psychoanalytic theory with its attention to dreams and psychic conflicts traces monstrous imagery to fantasy processes. Folklorist David Hufford, in his seminal book *The Terror That Comes in the Night* (1982), proposed an experience-centered approach to understanding bedroom attacks by malevolent, suffocating creatures, the original bearers of the term "nightmare." He concluded that this universal phenomenon could be explained in part by the physiological process of sleep paralysis and thus, contrary to standard academic wisdom, some seemingly supernatural encounters were not imaginary but in fact had an experiential basis. A growing literature honors the legitimacy of experience in making monsters, among them Paul Barber in *Vampires, Burial, and Death* (1988), who argues that the physical phenomena of death and decay provided the imagery for vampire accounts; and Adrienne Mayor in *The First Fossil Hunters* (2000), who attributes the origin of many monsters to the discovery of fossil skeletons by ancient peoples. Some of the most complex and compelling theory on the origin of monsters comes from cognitive psychology, which takes into account the evolutionary history of the human species and certain predispositions that became hard-wired into human thought through processes of natural selection. As prey for giant predators and often in competition with members of their own kind, early humans and proto-humans underwent a long mental process that shaped certain responses still with us today, such as fear of the dark, fear of snakes, and xenophobia. In theory, many aspects of social behavior, religion, imagination—and its monsters—have this evolutionary basis.

A new addition to the literature of monsters is Matt Kaplan's *Medusa's Gaze and Vampire's Bite*. Kaplan, a science journalist published in many leading magazines, subtitles his book "The Science of Monsters," a phrase that keynotes an attempt to find rational origins for the monsters of myth, legend, and storytelling past and present, and to draw out some understanding of why they fascinate us. He defines monsters in a broad, informal sense as creatures that are horrible to behold and threatening in some way, though even these basic characteristics turn inside out as he explores the evolution of the monstrous and sees, for example, the vampire transform into screen heartthrob or the giant ape King Kong become sympathetic and his exploiters the villains. An important sub-theme of the book is that human fears have changed over time as human circumstances have changed, yet our monsters have evolved in parallel to stay with us, adapting as our fears change and renewing themselves as relevant embodiments of those fears.



Kaplan's plan is to showcase a certain type of monster in each chapter, provide notable examples, consider possible natural sources, and follow up with appearances of the type in such modern contexts as the movies. He starts with creatures made monstrous by unusual size and ferocity. Examples include the Nemean Lion and Calydonian Boar from Greek mythology, the Rukh (or Rok) from Persian folktales, and the modern King Kong. Another step upward in complexity arrives at the monster of mingled parts. The Chimera has the head of a lion and tail of a snake with a goat's body in between, the Sphinx has the head of a human and the body of a lion. What makes these creatures monstrous is their disturbing, unnatural mixture of parts, an unsettling property exploited by H. G. Wells in *The Island of Dr. Moreau* as the scientist surgically transformed animals into semi-humans. Some monsters like the Minotaur and Medusa abide beneath the earth, or like Leviathan and the shark in *Jaws* belong to the depths of the sea. The dragon deserves a chapter of its own as one of the most widespread and versatile monsters. It draws on the inherent fearfulness of the serpent and adds the ability to fly as well as the fabulous quality of breathing fire. As the form of the elder gods dragons threaten to destroy the world, as subterranean guardians of treasure they imperil heroes from Beowulf to Harry Potter, yet in China they trade off much of their terror to appear as godlike agents of benevolence, fertility, and good fortune.

Another class of monster belongs to the realm of the supernatural. Some are disembodied spirits like ghosts and the demons that can assume physical form to attack people in their sleep, often to rob them of sexual or life energies. Others prey on the living, among them humans that transform into ravenous werewolves, the undead vampires that suck human blood for sustenance, and zombies that are animated corpses with an appetite for human flesh. In these cases the monsters are predatory, malevolent, merciless, possessed of superhuman powers, often cunning and intrusive in their attacks so that the usual places of safety like home or bedroom are vulnerable—all in all a potent combination for evoking terror. A third class of monster originates in human creation. The Golem, Frankenstein's monster, and robots begin with good intentions but eventually, inevitably, run amok. The same can be said of the dinosaurs resurrected by science for *Jurassic Park*. These monsters tell a straightforward morality tale of human hubris usurping a power that belongs to God alone, and the punishment that always ensues. Kaplan closes with aliens as the modern restoration of endless opportunity for monstrousness, since in the vastness of space there is no danger of running out of room on the map and all fears become possible once again.

So where do these striking creations of the human imagination originate? They provoke fear by being horrible to look at, terrifying in their behaviors, or dangerous by placing us in the position of prey or victims. Fear itself holds an appeal for humans. Whether the feeling is relief that comes from escape or the adrenaline-pumping excitement that comes from the presence of danger, fear represents one of the strongest emotions a human can sense and we have sought it through the ages in our actions and our storytelling. For many of us an occasional taste of fear is a good thing; for some of us it is a drug, even an addiction.

No doubt about it, an underlying psychological predilection creates a receptive audience for stories of monsters. Even so, Kaplan is not content to explain them as purely psychological phenomena and settle for imaginary products of the psyche. He puts his faith in concrete origins and searches for the experiences that might reasonably occasion the monsters he catalogues. For him monster stories are accounts of reality and not just tales, however many misunderstandings and distortions intervene between experience and the story we read today. He promises that while his solutions will be speculative, they will stand on informed scientific foundations.

Some of his explanations sound thoroughly plausible. A story of depredations by an unusually large lion or boar requires nothing more mysterious than human contact with uncommon wildlife or an exceptional specimen of an indigenous species. Geological causes offer phenomenology with a striking similarity to some activities attributed to monsters. Take, for example, the rumbling sounds of an earthquake that might be mistaken for the subterranean bellows of the Minotaur, and the heaving ground for evidence that he stirred just beneath the surface. A mass of bones left by several animal species killed in a flood could be mistaken for one animal of many parts, while ancient peoples puzzling over the gigantic bones left by extinct reptiles and mammals might well have imagined serpentine monsters, bird monsters, monsters with enormous teeth and grotesque form. His answer for elements of the dragon story is particularly convincing. The dragon of Beowulf guards underground treasure, breathes fire, and spews poison. The burial of treasure in caves and underground tombs placed these objects in an environment where flammable gases like methane might collect, where a grave robber with a torch might set off a fiery explosion that, combined with the roaring noise of the fire and the noxiousness of the gases, could persuade a survivor or onlooker that a deadly monster was punishing the intruder.

Naturalistic explanations continue to work for some supernatural monsters: Sleep paralysis and the hallucinations that accompany it have surely contributed much to demonology, while many attributes of vampires mimic the phenomenology of bodily decay, plague, and rabies too closely to doubt a connection. When passing into the realm of man-made monsters Kaplan has to give up natural sources and rely on fears of technology mingled, in the case of movie portrayals of cloned female monsters, with the threat of a sexually alluring creature made dangerous by a lack of humanity. To find the (acceptable) science in aliens, he talks only about the prospects for extraterrestrial life, the fear of colonization played on by invasion stories like *The War of the Worlds*, and human reaction to the parasitic and predatory monsters of the *Alien* series.

Kaplan stays true to his goal of seeking out the scientific issues related to monsters, and this approach is fine as far as it goes. He emphasizes an experiential basis for many famous examples of the breed, and though he consistently reduces the experience to mistakes and misinterpretations of natural phenomena, at least he does not resort to facile dismissals of everything anomalous as mere imagination. Kaplan's fascination with the scientific implications of monsters is infectious, though he sometimes becomes digressive. Some readers may tire of the lengths he goes to, for example, in arguing the many reasons that giant animals are not likely to be genetic mutations, or that the La Brea tar pits in Los Angeles are admirable preservers of prehistoric animals, but the absence of such pits in Greece means no bone beds of this type could have influenced the Greeks. A detour into the scientific realities of parasitism seems unnecessary for understanding the terror response to the *Alien* creature exploding out of the chest of an infected crewman. The movie's visuals were quite sufficient for a good scare. While his explanations are never completely impossible, Kaplan sometimes stretches them into implausibility, as in his argument that the ancient Greeks got the idea for Medusa turning people to stone from observations of bones petrified by fossilization.

One shortcoming in Kaplan's argument is his reliance on natural science to the near-exclusion of anthropological, sociological, psychological, and humanistic contributions to the subject. Natural science can answer many questions, but cognitive psychology offers some of the most exciting current pathways to understanding the nature and persistence of monsters, and he devotes only passing attention to these findings. The symbolic and metaphoric functions of the monster as an agent of chaos or expression of the Other holds as important a place in explaining the cultural hold of such ideas as natural origins or even the evolutionary foundations of fear, yet the reader finds little reference to this extensive literature.

Another serious omission is the life of monsters as verbal entities. Once described, talked about, and cast into stories, the verbal monster can evolve as readily as any organism, and a great deal faster. No argument about things rarely seen but often discussed should overlook the prospect that exaggeration and stereotyping shape the beast, rumor and boasting build it up, and the pressures of pleasing an audience betray facts in favor of an entertaining story. A verbal entity also enjoys mobility. Stories pass from mouth to mouth often over great distances, and those stories or others reformulated out of borrowed plots and motifs circulate ideas about monsters without need for experience. Kaplan's emphasis on the ancient Greeks is understandable since those monsters are famous and familiar, but his hermetic treatment hastens to a natural source without considering cultural influences on the Greeks, who were, after all, well-traveled and exposed to the ideas of many surrounding peoples. He makes only occasional mention of the monsters of Mesopotamia, barely touches on chimerical Egyptian gods such as Thoth, with the ibis head, or Horus, the falcon god, and says nothing at all about Alexander's encounter with the wonders of India. The ultimate source of such figures may have been natural, but a small bet might be in order that the proximate origin for chimerical figures of classical antiquity was a traveler's tale, a statue or image traded in a market, or some other instance of culture contact. If scholars have hitherto done our understanding a disservice by downplaying the importance of experience, this book would gain balance with more acknowledgment that transmission of ideas also contributes to our monsters.

The book succeeds in what it sets out to be, a tour through a gallery of scary, semi-imaginary beings from both long ago and here today, and proposals for scientifically reasonable origins of each. These goals sacrifice depth for breadth and the result is limited, disappointing for the anomalist interested in the possibility of genuine cryptozoological entities, or for the scholar concerned with a well-rounded discussion of all aspects of monster theory. The casual reader or newcomer to the field will find a readable, informative, and entertaining introduction to monsters and an answer for some of the questions foremost in any reader's mind. A nuanced understanding will require deeper pursuit, but this book is a good starting place, not least because of its respect for experience in the creation of seemingly fantastic stories, and for its reminder that monsters are not just things of the past. Adaptable and meaningful still, they may change shape but they continue to haunt the shadows beneath every bed.

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BOOK REVIEW

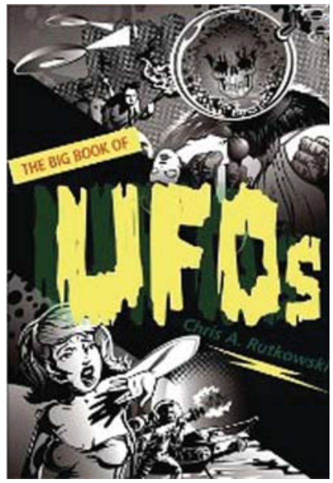
The Big Book of UFOs by Chris A. Rutkowski. Dundurn, 2010. 396 pp. $19.99. ISBN 978-1554887606.

You don't need to wander very far into Chris Rutkowski's aptly named *The Big Book of UFOs* to get the message: The guy knows his stuff. He's got it in chronological order, from pre-20th century trends all the way up to the double-aughts of the 21st. He's got it broken down into narrow categories: contactees, abductees, implants, hybrids, hoaxes, debunkers; you name it, this 396-page tome likely has a reference.

Much of the material, such as Roswell and SETI, is so familiar that you don't even have to be a hardcore student of the genre to recognize it. But some entries are so arcane it's doubtful that even Jeopardy-caliber UFO nerds could pass the test. Category: The 1890s Wave. Answer: During the Teddy Roosevelt Administration, sightings of unknown "airships" became so common that this company took out newspaper ads with graphic UFO illustrations stating "This Is What You Saw," accompanied by the slogan "High up in quality, low in price." Question: What is White Star Baking Powder?

On the other hand, Rutkowski's sojourns into remote corners of the world could also prepare us for some future Geography category competition. There were the miners who saw triangular lights outside Taparko, Burkina Faso, on Christmas night, 2005. The following year, at Port el Kantouni, Tunisia, a couple of people reported seeing low-flying rods with running lights. Some encounters are more unfortunate than others. A dog dies in Uruguay from internal bleeding in 1977 as a possible result of exposure to UFO radiation. The same year, a Polish resident of Piastow suffers headaches and faceburn after an alleged encounter. In 1975, a fellow living near Macheke, Rhodesia, is thrown to the ground and rendered unconscious when struck by a "bright white beam of light" outside his house.

The Big Book of UFOs is also something of a laundry list, with declassified government documents thrown in with cultural trivia. Readers are reminded, for instance, that the Robin Williams sitcom *Mork & Mindy* was actually a *Happy Days* spinoff, in which Richie Cunningham's abduction was derailed by Fonzie's intervention. "This episode aired several months after the infamous 'jump the shark' episode. . . . " There are interactive features as well, such as a checklist to tell if you yourself might've been abducted by aliens.



In other words, there is no narrative thread unifying Rutkowski's *Big Book*. There doesn't even appear to be an agenda, aside from a reference book–like compendium of 100-plus years of weird goings-on in the sky. But Rutkowski, a Canadian astronomer and science writer who's spent more than 30 years on this dim trail without so much as the satisfaction of seeing a UFO himself, is careful not to make a case for one theory over another. In fact, he reviews the hoaxes and offers conventional explanations for incidents where conventional explanations are the most logical.

But for a long-time UFO watcher with a low threshold for official nonsense, Rutkowski's 2010 *Big Book* apparently delights in highlighting some of the more twisted explanations offered by authorities and debunkers.

In 1966, for instance, during what would appear to be a precursor to the crop circle mystery, an Australian farmer saw a spinning "football-shaped object" one morning that evidently left reeds and grasses in two locations depressed in a clockwise rotation. Although the Royal Australian Air Force confirmed that no aircraft had been in that location, the local police attributed the swirls to a helicopter, and the event to the farmer having seen "sunlight gleaming on the rotating blades."

The Big Book serves up a number of such contortions, but one of the highlights involves a retired British intelligence officer named Angus Brooks. In 1967, as a British Airways employee, Brooks reported a complicated, shape-shifting UFO hovering near a field amid a "Force 8 gale wind." The Ministry of Defense jumped on the case because it occurred "between an atomic energy station, an underwater weapons base, and a USAF communications base." The MoD concluded that Brooks must have fallen asleep, and when he awoke was confronted by a "vitreous floater" in his eyeball. "However," Rutkowski writes, "as the witness himself noted, it would have been nearly impossible to fall asleep while sitting in such a strong wind."

If you're looking for a wide-angle take on what the UFO fuss is all about—cults, astronauts, even the Face on Mars—*The Big Book* is as good a place as any to start. But from there, you're on your own.

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Article of Interest

The Tobacco Beetle in Egyptian Mummies by Dominique Görlitz. *Migration & Diffusion*, 2011. 11 pp. http://www.migration-diffusion.info/article.php?year=2011&id=239

Dominique Görlitz is a German experimental archaeologist specializing in ancient watercraft trials. He here reviews the evidence concerning the presence of tobacco leaves, residues of nicotine and cocaine, and tobacco beetles in the tombs/mummies of the ancient Egyptian Pharaohs Ramses II and Tutankhamen. In this article, he contributes valuable information and observations additional to what is covered in this writer's earlier works (see Jett 2002, 2003–2004, of which Görlitz appears to be unaware).

While a French team was restoring Ramses's mummy during the 1970s, it discovered shredded *Nicotiana* sp. leaves in the lowest parts of the abdominal cavity. The tobacco was part of a homogeneous mixture of finely chopped plants of various kinds, surrounded by the resins of embalmment. There was nicotine in the wrappings as well—on which also appeared an imago of a tobacco beetle, *Lasioderma serricorne*. Like domesticated tobacco, this beetle is believed to be of New World origin. The tobacco-fragment samples were obtained with long biopsy tweezers from inaccessible sites through previously made artificial openings, seemingly obviating the possibility of contamination or of nineteenth-century insertion. The relevant material cannot at present be carbon dated, because the sample in Paris has disappeared.

Later, a team led by the Munich forensic pathologist Svetlana Balabanova detected residues of nicotine in other ancient and medieval Egyptian mummies, finding greater concentrations of the alkaloid and/or its metabolites in artificially mummified cadavers than in naturally desiccated ones. This suggests deliberate use of *Nicotiana* as an antiputrefactant in the mummification process, in addition to ingestion. Certain other Old World plants—including solanaceous species, sour cherry, common polypody, and stonecrop—carry nicotine, but in concentrations too slight to account for the mummies' concentrations.

The fairly recently discovered wild tobacco of Namibia belongs to the Australo–Pacific "subgenus *Sauveolentes* which contains almost no nicotine" (p. 9) and so is not a contender as the source for Egypt.

The tobacco beetle was also found in food jars in the tomb of King Tut, whose inner chambers were sealed until 1922. Like Ramses's *Nicotiana*, Tut's beetles cannot be directly dated, because they are no longer extant.

Entomologists assume the species to have originated in the Americas, where it would have evolved its unique tolerance for nicotine, a toxin characteristic of the beetle's preferred food. It is unable to fly far, certainly not across oceans. It is spread mainly by carriage with its host, dry tobacco; consequently, it is unlikely to have spread to the Old World *without* its host. Görlitz suggests, therefore, that *Lasioderma* is an archaeozoan (pre-Columbian–introduced) rather than a neozoan (post-1492–introduced) species, carried across the Atlantic in Pharaonic times or earlier.

Regarding coca, only the American species of the genus *Erythroxylum* carry cocaine (although some Old World species do contain other alkaloids, including the cocaine relative tropane), and only the two South American domesticated species have this alkaloid in sufficient concentration to account for the residues in the mummies. Seemingly, the presence of cocaine evolved following the continental-drift separation of the Old World and New World populations of *Erythroxylum*.

The author concludes,

> it must be accepted that these species were introduced from there [the New World] into the Old World cultures. We do not have strong indications today about who and which society realized these cultural interactions. It seems likely that people from young [Upper] Paleolithic cultures in Spain or their descendants—the Basques—were responsible for this pre-Columbian network trade. (p. 9)

Görlitz does not consider the possibility of trans*pacific* carriage.

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Note: This review will also appear in *Pre-Columbiana: A Journal of Long-Distance Contacts*, *5*(2–4), *6*(1).

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Article of Interest

Research Grants: Conform and Be Funded by Joshua M. Nicholson and John P. A. Ioannidis. *Nature*, *492*, 2012, 34–36.

The article's subtitle tells it all: "Too many US authors of the most innovative and influential papers in the life sciences do not receive NIH funding . . . "

This fits in a little-remarked genre: evidence that contemporary science is very different from the popular view of it as behaving objectively by virtue of the scientific method and peer review. Even as many such articles document flaws in clinical trials, statistical incompetence in much of the medical literature, and failures of peer review (for example, Altman 1994, 2002, Bauer 2013, Ioannidis 2005, Ioannidis & Panagiotou 2011), there are no effective followup ventures to improve matters.

In this instance, Nicholson and Ioannidis document with compelling data what actually is known to everyone trying to do research in biology or medicine: What matters is whom you know, not what you know or what you have discovered. The National Institutes of Health (NIH) has not been consistently supporting the best investigators, those whose work has had the greatest impact. One reason, with which again all insiders are familiar, is that the NIH study sections that make recommendations on grants are populated by people who themselves are very likely to have funding through NIH, albeit their work has had comparatively little impact. To exaggerate only slightly, grant proposals from geniuses are adjudicated by mediocrities.

Nicholson and Ioannidis suggest that one partial remedy would be for NIH to direct funds primarily to people of proven accomplishment instead of by the project-grant system that prevails in almost all funding of scientific research. The absurdity of that almost universal system is, again, widely recognized: Proposals for funding are expected to explain what the work will accomplish and what its impact will be, which cannot possibly be done if the work is to be truly creative and exploratory. So what gets funded are routine banalities. Richard Muller (1980) long ago noted what one has to do: Present a banal proposal and then bootleg as much as possible of the grant for really worthwhile work.

While this article's analysis adds to documentation of the problem, the suggested amelioration is not likely to be feasible under present circumstances, because the judgments as to who the most accomplished people are would again by made by the study sections whose record is the funding of banalities (Bauer 2012).

The broadest context for the present dysfunction in science lies in societal drives for "equity" and against "elitism." Because in the past all sorts of judgments led to discrimination against females and members of various minorities, institutions have increasingly sought to make judgments objective, and this is too often interpreted as quantitative; or, judgments are left to committees whose composition is supposed to ensure equity by including representatives of historically deprived groups. But as is well-known, committees asked to design horses are prone to come up with camels; only individuals can judge *quality*; and there are no quantitative measures of quality. Consequently, in much of society and much of science and much of medicine, judgments are being rendered that do not reflect quality.

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ARTICLE OF INTEREST

Awaiting a New Darwin by H. Allen Orr. *The New York Review of Books*, February 7, 2013, pp. 26–28.

Orr's article is a review essay on philosopher Thomas Nagel's recent *Mind and Cosmos: Why the Materialist Neo-Darwinian Conception of Nature Is Almost Certainly False.* Some reviews are at least as important as the book they are about. As it is, this essay is a valuable contribution to the intellectual discourse on the topics of evolution, mind, and life.

Professor Orr is a biologist at the University of Rochester. He argues that Nagel does not make his case that there are disabling problems with the Neo-Darwinian accounts for the vast evolutionary changes that have transpired with organisms over Earth's history. He dismisses Nagel's skepticism as only an "argument from incredulity."

But he does share Nagel's skepticism and sense of mystery regarding the reduction of mind and consciousness to "matter": ". . . we haven't the slightest idea how it would work." And, "Brains and neurons obviously have everything to do with consciousness but how mere object can give rise to the eerily different phenomenon of subjective experience seems utterly incomprehensible" (the Hard Problem of Consciousness). Orr points to the writings of another philosopher, Colin McGinn, who contends that our mystery about consciousness is a reflection of our cognitive limitations. Orr adds, most pertinently, "All other species have cognitive limitations, why not us?" and, ". . . the mysteriousness is not so much a challenge to Neo-Darwinism as a result of it."[1]

Erin Pauwels Collection/Art Archive/Art Resource

"A Sun of the Nineteenth Century" cartoon from *Puck* magazine showing Charles Darwin as a shining sun, chasing the clouds of religion and superstition from the sky, 1882 [reprinted in Orr's review]

Orr acknowledges "The origin of life is admittedly a hard problem. . . . "

Yet he thinks that "big progress" has been made. It should be commented that there are separable questions (which Orr does not offer) regarding life: What is life?[2] How did it occur on Earth? And, How did life forms (organisms) change over the eons of evolutionary time? To my knowledge, Darwin did not try to tackle the first two questions.

On the first question, "What is life?", perhaps there is a "Hard Problem" analogous to what David Chalmers called the "Hard Problem of Consciousness." Orr does not go in this direction.[3]

Notes

[1] Those interested in pursuing this line might wish to read "Kant's A Priori in the Light of Modern Biology" by Konrad Lorenz, in *Konrad Lorenz: The Man and His Ideas* by Richard I. Evans (1975), writings by Harry Jerison on the evolution of intelligence, and writings on evolutionary epistemology by Donald Campbell.

[2] That is, beyond the conditions and structures that enable it, just as consciousness seems not explainable by the conditions and structures that enable it.

[3] Further reading might include *Essays on Life Itself* edited by Robert Rosen (1999), *What is Life? With Mind and Matter and Autobiographical Sketches* by Erwin Schrödinger (Foreword by Roger Penrose) (1944/Canto Classics 2012), and *What Is Life?* by Lynn Margulis and Dorion Sagan (1995/2000).

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56th Annual Convention of the Parapsychological Association Ora Domus La Quercia, Viterbo, Italy

August 8–11, 2013

Program Chairs: Massimo Biondi and Patrizio Tressoldi
Arrangements Chairs: Ulisse Di Corpo and Antonella Vannini

All submissions to the 2013 PA convention, except proposed workshops, must be submitted electronically. They should be emailed, as attachments, to the chair of the Program Committee, Dr. Patrizio Tressoldi at patrizio.tressoldi@unipd.it

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First International Conference on Life Energy, Syntropy, and Resonance

Viterbo, Italy, August 1-4, 2013
Hotel Domus La Quercia, Viterbo, Italy

World Institute for Scientific Exploration
instituteforscientificexploration.org

Program Committee: Dr. Antonella Vannini, chairman, Dr. Richard Blasband, Dr. Dominique Surel, and Dr. Ulisse Di Corpo. Send an abstract to antonella.vannini@syntropy.org by June 15th and the full paper by July 15th. Presentations should be at least 30 minutes, but not more than 60 minutes, with 15 minutes allowed for questions/discussion.

Registration: €120 or $160. Presenters are not required to pay. Register here: http://wisewiki.org/tiki-index.php?page=Registration+for+the+International+Conference+on+Life+Energy%3A+Syntropy+and+Resonance
After registering, please send an email to the organizer Dr. Ulisse Di Corpo: ulisse.dicorpo@syntropy.org A limited number of 100 places are available.

Workshops: Three workshops will be held:
The Syntropy and Life Energy Workshop (August 5)
Life Energy and Methodology Workshop (August 6)
Controlled Remote Viewing: A Transformational Experience Workshop (Aug. 7)
After registering for the conference, send an email to the organizer Dr. Ulisse Di Corpo: ulisse.dicorpo@syntropy.org

32nd ANNUAL CONFERENCE OF THE SOCIETY FOR SCIENTIFIC EXPLORATION
DEARBORN (DETROIT), MICHIGAN JUNE 5–JUNE 8 2013
UNSETTLED SCIENCE

Aerial Anomalies (UFOs)—Eddie Bullard (Keynote)
Mark Rodeghier; Michael Swords
Historical Anomalies (Pre-Columbian Contacts)—Stephen C. Jett (Keynote)
Mind Anomalies (Levitation, Macro-PK, Yoga)—Donald J. DeGracia (Keynote)
Larry Dossey, Erik Schultes
Evening Panel—Do topics under the SSE umbrella have enough in common that we can learn from each other?

PROGRAM CHAIR: Huyghe@anomalist.com (proposals were due by Feb. 15)

The 2013 Annual SSE conference will be held at the historic Dearborn Inn, in Dearborn, Michigan, beginning with the Wednesday evening Reception June 5, and running through the Saturday evening Banquet June 8. The Inn is 12 miles from Detroit Metro Airport.

CONFERENCE HOTEL: Dearborn Inn Marriott, 20301 Oakwood Blvd., (313) 271-2700/ SSE rate $110 per night (code SSESSEA). Smoke-free environment, in-room Internet $12.95/day, free parking, gym, outdoor pool, 2 restaurants. The Inn was built in 1931 by Henry Ford. SSE will be in the Alexandria Ballroom.

CUTOFF DATE FOR THE SSE ROOM RATE IS APRIL 27, 2013. SSE rooms probably will fill up long before that.
http://www.marriott.com/hotels/travel/dtwdi-the-dearborn-inn-a-marriott-hotel/

http://www.marriott.com/hotels/travel/dtwdi?groupCode=ssessea&app=resvlink&fromDate=6/4/13&toDate=6/9/13

OTHER HOTELS: There are more economical accommodations at the nearby Comfort Inn, 20061 Michigan Ave., Dearborn, which offers complimentary breakfasts and shuttle service to nearby locations, such as local restaurants and the Henry Ford Museum. There are many other nearby hotels.
http://www.comfortinn.com/hotel-dearborn-michigan-MI385

WELCOME RECEPTION: free, Wednesday evening June 5th, 6:00–9:00
FRIDAY FIELD TRIP: $65 including lunch and bus trip: LaPita Restaurant, Edsel Ford Home and Grounds, Detroit Institute of Art
SATURDAY NIGHT BANQUET: $65

PROGRAM COMMITTEE: Patrick Huyghe (Chair), Erik Schulte, Henry Bauer
LOCAL HOST: Carl Medwedeff carlgm@yahoo.com
REGISTRATION AND PROGRAM AT www.scientificexploration.org

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