Distant Healing Intention: Definitions and Evolving Guidelines for Laboratory Studies

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Abstract

This paper provides definitions and a discussion of evolving guidelines for conducting research on the effects of distant healing intention (DHI) on living systems in the laboratory. We consider the relevance of DHI laboratory research to applied healing, special theoretical challenges, and other considerations that distinguish DHI research from other domains of laboratory science. Two sample protocols for investigating DHI are provided, one involving the human autonomic nervous system as the "target" of distant intention, and the other involving cell cultures.

In essence, DHI differs from other alternative healing modalities because it postulates that mental intention alone can affect living systems at a distance, unbounded by the usual constraints of both space and time. This postulate challenges scientific assumptions that often go unexamined, including the nature of causality, the distinction between subjective and objective states, and the efficacy of double-blind protocols in controlling for experimenters' intentions.

Previous laboratory research in this domain suggests that DHI effects warrant serious study, but most scientists and funding agencies are unaware of the evidence or the relevant literature. By following these evolving guidelines, researchers' designs and their ultimate publications will conform more closely to the quality standards expected by scientific journals, and such publications will in turn attract the attention of a broader range of scientists. This seems especially important for alternative healing research in general and for distant healing in particular, as both realms enjoy broad public support but have largely eluded serious attention by mainstream science.

Definitions

Distant healing intention

In a medical context, DHI postulates that *the intentions of one person can influence the health of a distant person*. In more general terms, DHI postulates that the *intentions of one or more persons can interact with the physiological, psychological and/or behavioral status of one or more distant living systems*. DHI is a subset of a broader class of controversial phenomena suggesting the existence of direct mind-matter interactions.

Many terms are used to describe forms of distant healing interventions. They include intercessory prayer, spiritual healing, non-directed prayer, intentionality, energy healing, pranic healing, non-local healing, non-contact Therapeutic Touch, level III Reiki, external Qi Gong, and Johrei. Each of these terms describes a particular theoretical, cultural, and pragmatic approach to influencing a healing or biological change through mental intention of one person toward another (Schlitz & Braud, 1997).

DHI laboratory studies focused on basic science therefore explore the question: *Can intention alone interact with a distant living system?* Process-oriented DHI studies study personality, environmental, and physical factors associated with DHI effects.

Distant

The word *distant* in DHI specifically means shielded from ordinary physical and psychological influences by means of spatial, temporal, and/or sensory shielding, i.e. exclusion of all known causal pathways of human interaction. This distinguishes DHI from mind/body/energy therapies in which healers are in touch with or in close proximity to the "target" living system.

Intention and intentionality

Because *intention* is a key element in DHI, it is useful to consider the semantics of this word before getting into methodological details of conducting experiments. For purposes of the present discussion, *intention* is defined as a mental state directed toward achieving a goal (Malle & Knobe, 1997).

A distinction can be drawn between two related mental states directed at goals – *desire* and *intention*. These two states differ in three respects (Malle & Knobe, 2001).

- *Intention is directed at* the intender's action, whereas *desire* can be directed at anything ("I want O," where O can be any object or state of affairs, including another person's actions or experiences). Thus, we can desire what we consider to be impossible, but we cannot intend to do what we consider to be impossible.

- *Intentions* are based on a certain amount of reasoning whereas *desires* are typically the input to such reasoning ("I *intend* to A because I *desire* O"). A desire can be triggered simply by the presence of an attractive object; an intention often involves some deliberation and decision making.

- *Intentions* often come with a commitment to perform the intended action, whereas *desires* often do not carry such commitments.

A related concept is *intentionality*. Intentionality specifies under what conditions people judge a behavior as being intentional. This judgment is based on five conditions: (1) An action is considered intentional if the healer had a *desire* for an outcome, (2) a *belief* that the action would lead to that outcome, (3) a desire to actually *perform* the action, (4) the *skill* to perform the action, and (5) *awareness* of fulfilling the intention while performing the action. Intentionality is thus an attribute of a conscious and willful *action* while intention is a mental *state* associated with a subjective purpose (Malle & Knobe, 1997).

To be clear, intentionality is not the same as *mind* because intentionality is a property of objective *actions*, whereas mind is essentially the "apparatus" that harbors and produces subjective mental states. Intentionality is also not the same as *consciousness*, which may be defined as the capacity to be conscious, or the state of conscious awareness. Intention is also distinct from *attention*. Intentions are mental states representing future actions, whereas attention is a mode of focusing or selecting objects of perception, thinking, and awareness.

One may also distinguish intention from related psychological concepts like expectations, schemas, scripts, and so on. For example, a patient entering an oncologist's office automatically brings a set of expectations. A very different set of expectations would be brought to a visit to Traditional Chinese Medicine healer's office. These expectations may include rhetoric, power relations, behavioral repertoires, etc. Such variables can influence a healing attempt, but they are distinct from the concepts of intention and intentionality.

A major difficulty associated with studying the effects of intention in DHI research is that every experiment (indeed, any activity involving more than one person) consists of *multiply interacting* intentions. For example, a *healer* maintains an intention to perform actions resulting in measurable changes in a distant living system, a *patient* maintains an intention to allow the distant influence to

manifest, and an *investigator* intends to produce a successful study. But intention does not stop there. The coworkers or management of the investigators hold other intentions, and readers of articles describing the research may hold still other intentions. It is not clear how, or indeed if, these sets of intentions can be cleanly disentangled given that the notion of *distance* in DHI assumes that intentional effects are not limited by space or time (these assumptions are discussed in more detail later). These complex, entangled sets of intentions are unavoidably present in every DHI experiment.

Relevance of DHI Research to Healing

Despite only modest scientific proof of its efficacy and a lack of adequate theoretical explanations, many people regularly use some form of DHI, such as prayer, in the hope that it will help friends and loved ones who are ill. The problem is that in addition to doubt about its efficacy, even those who regularly practice DHI don't know how much, how often, or how long they should practice DHI to be effective. These are the types of questions addressed by both clinical and laboratory DHI research.

Epidemiology of DHI practice

It is difficult to quantify the prevalence of the use of DHI as a complementary and alternative medicine (CAM) therapy in the United States because it is so commonly practiced within American religious and spiritual life. A national survey in 1996 found that 82 percent of Americans believed in the healing power of prayer; 64 percent felt that physicians should pray with patients who request it (Wallis, 1996). A study by Cassileth (1984) found that 19 percent of cancer patients report they have augmented their conventional medical care with prayer or spiritual healing. A survey of women in the American Cancer Society's support groups for women with breast cancer showed that 88 percent found spiritual or religious practice important in coping with their illness (Johnson & Spilka, 1991), although the extent to which specific prayers or intentions of healing were part of their activities was not clear. In acute illnesses, such as cardiac events, these numbers rise even further. Saudia and colleagues (Saudia, Kinney, Brown, & Young-Ward, 1991), for example, found that 96 percent of patients stated that they prayed for their health before going in for surgery. In certain cultural or ethnic groups, seeking healing prayers or spiritual healing from an identified practitioner is commonplace (e.g., Suarez, 1996).

As a whole, the population of the United Kingdom is less traditionally religious than the United States, but there are more distant healers in the UK (approximately 14,000) than there are therapists from any other branch of complementary and alternative medicine (CAM) (Astin, Harkness, & Ernst, 2000). This indicates that DHI is widely practiced even without any religious background.

Spiritual healing, energy healing, and prayer are rapidly gaining acceptance among conventional medical professionals. In a 1996 survey of Northern California physicians (Wallis, 1996), 13 percent reported using or recommending prayer or religious healing as an intervention. Non-Contact Therapeutic Touch is used formally by nurses in at least 80 hospitals within the United States (Maxwell, 1996), and has been taught to more that 43,000 health care professionals (Krieger, 1979). Among the lay public, Reiki International, one of the largest training organizations for "subtle-energy healing" therapies, reports having certified more than 500,000 practitioners worldwide. While Reiki healing is frequently performed through physical contact, one form of Reiki is claimed to be effective over distances of thousands of miles (Schlitz & Braud, 1985).

Laboratory evidence for distant healing intention

Anecdotal claims of DHI have been reported in a wide variety of conditions ranging from malignancies and genetic illnesses to wounds (Dossey, 1993). In a narrative review of controlled experimental and clinical studies published before 2001, Benor (2001, 2002) found statistically significant evidence for such effects in 88 of 138 studies. Among these studies 50 were rated as of excellent methodological quality and 37 (74%) of them yielded statistically significant results. However, the extent of selective

reporting in that literature is unknown, and many of those studies did not use double-blind, randomized trial designs.

Simple life forms

Controlled laboratory experiments involving non-human living systems have shown replicable effects of DHI on life forms including enzymes (Bunell, 1999), fungi (Barry, 1968; Tedder & Monty, 1980), yeast (Haraldsson & Thorsteinsson, 1973), bacteria (Nash, 1982; Rauscher & Rubik, 1980), cancer cells (Rein, 1992; Snel & Hol, 1980), and on hemolysis of red blood cells under osmotic stress (Braud, 1990; Braud & Schlitz, 1989). These studies were conducted under randomized and blinded conditions such that the person conducting the measurements did not know whether the preparation had been in the treatment or control groups. The "healers" in the above studies included Western and Eastern European healing practitioners of considerable renown [e.g., in the studies of cancer cells (Braud, Davis, & Wood, 1979; Rein, 1992) and wound healing (Grad, 1965)], as well as volunteers, students, and laboratory personnel in positive studies of hemolysis (Braud, 1990; Braud & Schlitz, 1989), and bacterial growth (Nash, 1982). While some of these experiments were conducted under acceptable controls and resulted in statistically significant effects, the extent of selective reporting in this literature is unknown, requiring caution in interpreting the results.

Animal models

Significant evidence of DHI has also been reported in animal disease models such as amyloidosis in hamsters (Snel & Ver der Syde, 1995), murine malaria (Solfvin, 1982) and experimentally induced goiter and surgical wounds in mice (Grad, 1965). Watkins and Watkins (1971) found more rapid recovery from anesthesia in animals receiving DHI, an effect that was later replicated by Schlitz (1982). A more recent exploratory study of tumorigenesis (Snel & Ver der Syde, 1995) found increased survival in rats injected with ascites tumor cells treated at a distance by an experienced healer when compared to untreated animals and similar results were reported by Bengston & Krinsley (2000). In the former study, rats were treated by a professional healer who was several miles away, and those rats showed significant benefit compared to a no-treatment control. These experiments, although small in number and in need of further replication, appeared to be conducted under sound methodologies, providing support for the hypothesis that DHI may be able to modify a variety of biological processes.

Human studies

Laboratory investigations have also found evidence for DHI on the human autonomic nervous system (Schmidt, in press; Schmidt, Schneider, Utts & Walach, 2002). In this realm there are two major experimental paradigms, one dubbed *DMILS* (Direct Mental Interactions with Living Systems) and the other *Remote Staring*. The former assesses whether there are significant changes in sympathetic autonomic nervous system activity as measured by electrodermal activity (usually skin conductance) in subjects toward whom an unseen "influencer" in another room was sending intention for relaxation or for physiological excitation at random intervals. A meta-analysis of these studies yielded a small but highly significant overall effect size (Cohen's d = 0.11, p = .001) for more than 1,000 blinded and randomized sessions.

In Remote Staring experiments (Braud & Schlitz, 1989, 1991; Schlitz & Braud 1997; Schlitz & LaBerge, 1994; Wiseman & Schlitz, 1996, 1997), the distant "influencer" or "starer" gazes at the other subject during randomized intervals by means of a closed circuit video system. A meta-analysis by Schmidt, Schneider, Utts & Walach (2002) reports a significant effect size (Cohen's d = 0.13, p = 0.01) for an updated set of these experiments in which the receiver's skin conductance was targeted.

Special Theoretical Considerations

The idea that mental intention can causally influence distant living systems evokes two basic scientific problems: The first is the assumption that "action at a distance" is not possible. Restated, this

assumption presumes that all observable phenomena are causally connected, and that all causal connections are proximally (i.e., spatiotemporally) contiguous. Thus, a phenomenon based on "distant influence," with no (known) observable causal connections, is scientifically forbidden. The second problem is that there are no well-accepted theoretical reasons to expect that mind can directly interact with matter, excepting perhaps a mind interacting with "its" brain. These two problems are sufficient to cause most scientists to seriously doubt that distant healing is what it is claimed to be. And it is understandable why staunch skeptics assume that apparent DHI effects can be completely explained as a combination of wishful thinking, poor methodologies, embellishment, or in extreme cases, fraud.

While thoroughly reviewing the theoretical implications of DHI is beyond the scope of this paper, it is useful to point out that the "physically impossible" objection to DHI has been essentially obsolete for over a century. One of Einstein's central complaints about quantum mechanics was its predictions about "spooky actions at a distance," and yet subsequent experiments have repeatedly demonstrated that the fabric of the universe is indeed nonlocal, i.e. it not only allows "spooky action," but – the argument can be made – its very essence is nonlocal (Accardi & Regoli, 2001; Kwiat, Barraza-Lopez, Stefanov & Gisin, 2001; Namiki, 1990; Rowe et al., 2001). Likewise, the role of consciousness in the physical world has been pondered by virtually all of the founders of quantum theory (Stapp, 1988, 1997), indicating that at some level mind and matter may be inseparable concepts.

In sum, classical physics and common sense may disallow the possibility of DHI phenomena, but our most accurate descriptions of the physical world, as captured in the formalisms and experiments of modern physics, do provide an accepted physical substrate for DHI. To be clear, this does not mean that DHI is adequately *explained by* early 21st century physical theories, but it does mean that dismissive rhetoric such as, "DHI is impossible because it violates the laws of physics," is based on an incorrect understanding of what Nature allows.

Conducting DHI Studies

Targets of DHI have ranged from single cells to groups of humans. As a result, there are dozens of disciplinary-specific study designs that could potentially be covered in a guidelines document. Later we will consider in detail protocols for two of the most common classes of DHI studies. First we examine factors common to most DHI studies.

Basic considerations

A number of preliminary steps are important in generating quality research in any field. These include the value of pre-stating the experiment's hypotheses, procedures, sample size, and analyses. It is particularly important to determine in advance whether one-sided or two-sided hypotheses will be tested. In DHI research, a two-sided hypothesis would include the possibility of detrimental effects of DHI.

Exploratory analyses also play a valuable role in the scientific process, but results of such analyses should be identified as such. For the sake of credibility, it is sometimes useful to send planned protocols to a trusted, independent third party so that analyses may be verified following a study's completion. And all results should be reported regardless of outcome. This is valuable to avoid creating a "filedrawer problem" in which only positive studies are reported, thereby leading to over-estimates of effect sizes.

Research on controversial topics

While use of state-of-the-art design, measurement and analysis techniques are important in all laboratory studies, it is useful to keep in mind that research on controversial topics attracts severe critical scrutiny. Many scientists will presuppose that the claimed phenomena are impossible, and this bias will negatively influence their assessment of the methodology. Thus, DHI research designs and

execution require exceptional care. As an important first step in ensuring high quality work, we recommend that investigators who find themselves treading on new disciplinary ground obtain assistance from subject-matter specialists. We also recommend paying close attention to the vast literature of parapsychology, because for over a century researchers in this domain have identified dozens of design and execution pitfalls associated with studying subtle phenomena (Radin, 1997). Following these recommendations will help ensure that the language and concepts underlying the experiment are palatable to the mainstream, that the measurement and analytical results are state-of-the-art, and that known pitfalls and design flaws are addressed.

Special considerations

DHI experiments have several design elements in common: A *healer* (or volunteer acting as the healer) is asked to *mentally influence* one or more objective variables associated with a *distant living target system*. The variables may include growth rates in cell cultures, wound healing rates in mice, or fluctuations in human electrodermal activity. The healer's intention is often manipulated experimentally by randomly switching it – with suitable instructions – in a counterbalanced fashion between *treatment* and *control* conditions. The outcome measure involves comparison of the dependent variables in the target system when exposed to the treatment vs. control periods.

Because of the special role of distance in DHI, an important design feature in these experiments is ensuring that the healer cannot gain sensory information about the state of the target system, nor can the healer infer the state of the target indirectly. When human subjects (hereafter human targets of a distant healer are referred to as "subject") are the target of intentional influence, it is especially important to scrupulously isolate the healer and the subject against sensory leakage and any other information that the subject might consciously or unconsciously use to infer the healer's state of mind or instructions.

The basic DHI test is deceptively simple: A healer attempts to mentally influence a distant subject. But to illustrate the type of care required to design and execute a rigorous DHI experiment, we list some of the types of questions that should be considered when designing DHI experiments.

Randomization

Proper randomization is required to avoid providing experimenters, healers or subjects with hints about the identity of the targets. For example, in a DHI cell culture experiment, if a healer knew the condition of the target (e.g., treatment or control condition), he or she might behave differently, and this difference – rather than the healer's intention – might provide a mundane explanation for the observed results. Likewise, if the subject in a study investigating the effects of DHI on human physiology was able to infer the distant healer's intention, then the subject's physiological changes would be better attributed to expectation effects rather than distant intention.

Five methods are commonly used to generate random target orders: hardware-based truly random number generators, pseudorandom computer algorithms, random number tables, mechanically shuffled cards or tossed dice, and hand-shuffled cards or tossed dice. The first four methods are acceptable, the fifth is not because such sequences are often inadequately random.

Use of a truly random, hardware-based random number generator (RNG) is the preferred method. Such RNGs are typically based on quantum events inherent within electronic components. If such a device is properly constructed and operated, it will produce numbers that conform closely to the theoretically expected distributions and sequences of truly random numbers. Even though hardware-based RNGs can be excellent randomizers, it is recommended that, regardless of the method used, experimenters follow these guidelines:

1) Run both global (long-term) and local (short-term) control tests on the randomizing device.
2) Pre-specify the types of randomness tests to be conducted to evaluate the device.

3) Provide references that describe the nature of the randomizing device and manufacturer, if applicable.
4) Describe procedures for choosing the entry point into random number tables, if applicable.
5) Run RNGs under control conditions exactly as they were used in treatment conditions.
6) Describe environmental factors, including how RNG circuits were shielded against voltage fluctuations and power spikes.
7) Describe how pseudorandom seed numbers were selected, if relevant.
8) Provides references for pseudorandom algorithms, if applicable.
9) Describe details of how mechanical RNGs (dice or card shuffling) were subjected to randomness tests before, during and after each experimental session.
10) Keep a record (written protocol, video tape or data file) of the random number generation process.

Sensory shielding & blinding

Because DHI presumes the existence of connections between healer and patient that transcend all known sensory cues and expectancy effects, it is important for these studies to include rigorous sensory shielding and blinding procedures. Achieving such a high level of control is more complicated than is commonly thought, mainly because there are dozens of ways to be fooled by others, or to fool oneself, and because some topics like DHI tend to attract people whose egos are closely aligned with their wish to have, or be thought to have, special abilities. Some of these highly motivated people may attempt to demonstrate their claimed abilities by willfully or inadvertently cheating.

Fortunately, as a result of decades of critical assessments of parapsychological studies investigating DHI-like phenomena, guidelines have evolved to help ensure the design of adequate shielding and blinding (Sheldrake, 1998). Note that many of these guidelines specifically address ways to prevent healer and subject fraud or opportunistic cheating, as it is unfortunately naïve to assume that all participants in an experiment will be trustworthy. Questions to consider include the following:

<u>Preventing Sensory Leakage</u>

1) If the target sequence was written, was the paper under the target record destroyed or secured (to avoid impressions in that paper from being viewed by healer or subject)?
2) Could sound, infra-sound and/or vibrations have been accidentally transmitted between healer and subject?
3) Were tests conducted in the healer's room using sound and vibration, similar in pitch but greater in volume than any the healer might make under supervision, to detect whether such sounds could be detected at the subject's location?
4) Were subject and healers separated by at least one interposing room?
5) What sort of visual shielding was used to prevent the subject from seeing the healer's movements, shadows, or changes in illumination associated with the healer's position?
6) Were there any weight or size differences in target containers, when cell cultures were targets?

<u>Data Integrity</u>

7) How were the target sequences shielded during the experiment?
8) Were data records secure, were duplicate data held by more than one lab member, and were data cross-checked at the end of the experiment?
9) Who was present during the experimental trials?
10) How were non-laboratory personnel supervised during experimental trials?
11) In experiments where targets were in close proximity to the subjects, what sort of containers held the targets?
12) Were subjects allowed to see or handle any target container used in any trials?
13) If the target container (such as cell cultures) was handled by the healers at any time, could that container have been rendered transparent or translucent through contact with water, alcohol, oil, or light (as in sealed envelopes or paper boxes)?

Blinding Procedures (Subjects)

14) Were conditions under which the experimenter randomized the targets secure? Was each subject alone and out of sensory contact with all other subjects?
15) Was the randomizing method accessible to subjects at any time?
16) How and where were the targets (test conditions, cell cultures, etc.) stored?
17) Would it have been possible for subjects to reconstruct target sequences from prior published accounts of laboratory randomization procedures?
18) In subject-healer designs, were the healers allowed to signal readiness for a trial after they knew the target (the timing of such signals might provide the subject with clues)?
19) Were subjects in contact with anyone who knew the target identity at any time during the experiment?

Blinding Procedures (Experimenter)

20) Did the experimenter who interacted with the subjects have any relevant knowledge of the targets that would allow him or her to solve the task?
21) Did that experimenter have access to the relevant target sequence?
22) Did that experimenter have any relevant knowledge about the generation and the structure of the target sequence that would allow him or her to figure out how the sequence would continue after certain events (e.g. one could infer in an design with A and B targets that because AAA-sequences were never observed to occur, then if we just had an AA sequence, the next target must be B)?

Preventing Fraud

23) Did any of the participants (subject or healer) in the study have an interest in or knowledge of conjuring?
24) Was the experimenter trained to detect slight of hand or other conjuring tricks, had they occurred?
25) Did any suspicious activity occur during the experiment? Equipment moved, items missing or displaced, doors ajar, etc.
26) Did any of the participants claim to have special abilities?
27) Were procedures in place to prevent opportunistic cheating, such as preventing unauthorized or unsupervised access to computers, the target materials, and the testing area?
28) Were the participants' accomplices prevented from gaining access to or tampering with data records?
29) Were precautions taken against radio or other communications being used between healer and subject, including walkie-talkies and cellphones?
30) Did target containers bear secret marks, codes or seals impressed by the experimenters to detect attempted replacement or breaching?
31) Could target containers have been opened surreptitiously by the participants?
32) Were healers supervised in some way so that any obvious attempts to signal the subject could have been detected?
33) Were all access points between healer and subject locations secured, monitored, or guarded during the experiment?
34) If a target was on display in a room, was that location screened or guarded against peeking, and could the room containing the target have been surreptitiously modified in any way by a confederate who might have had access to the target?
35) If an healer were present who might have colluded with the subject, was supervision and/or visual and auditory shielding used to prevent accomplices from detecting signals from the healer?
36) Were subjects prevented from deliberately or inadvertently altering their responses to match targets after feedback was provided?

37) Were experimental procedures in place to prevent the healer from substituting another target for the real target?
38) What was the relationship between the subject and healer?

All sensory shielding and blinding methods should be thoroughly tested before the start of data collection, and all test runs including identified shortcomings should be documented. For establishing a laboratory environment as well as experimental protocols and procedures fulfilling the above criteria, we recommend the following:

- Establish an experimental protocol that describes all procedures of the ongoing study, including the hypotheses, number of preplanned sessions, and preplanned statistical evaluation procedures. This protocol should be held by a person not otherwise involved in the study.

- Develop detailed laboratory documentation that includes descriptions of all equipment, layout, calibration protocols, data of pilot runs, and test protocols documenting the sensory shielding conditions.

- Invite scientists not involved with the study to critically inspect the laboratory, equipment, materials and procedures. Such reviewers might be able to identify shortcomings that are not recognized by the staff.

Source of DHI Effects

In conventional experiments, the process of selecting dependent and independent variables is straightforward because we can safely assume that influences act in real-time, they are localized in space and time, and we know the source of the influences or treatments. In DHI experiments, all of these assumptions are challenged by the phenomenon under study. That is, for nonlocal effects supposedly mediated by intention alone, we cannot unambiguously assume that we know the source of the intentional effects, or even where or when they will occur. That is, DHI may not occur in direct time synchronization with distant mental intention, or at the precise location of the target, or be attributed primarily to the subject or healer. Anyone involved in the experiment, even peripherally, must be considered part of the experiment.

Because of these ambiguities, designers may wish to include both spatial and temporal phase shifts into the planned analyses. In addition, because intention is postulated to play a key role in DHI, it is difficult to know *whose* intentions are reflected in the experimental outcomes. It can be argued that the experimenter's intention is the primary "influencing" agent in a DHI study. In this case, because DHI designs often include differential measures (i.e., comparison of treatment vs. control conditions), the primary experimental goal may be to obtain a significant difference between the two conditions rather than to demonstrate a significant treatment effect *per se*. Such "differential effects," often observed in parapsychological experiments with differential designs (Rao, Sargent et al. 1983), has important consequences in interpreting a study's outcome.

For example, in a DHI study one might obtain a significant positive effect either by causing say, more mice in a treatment condition to live than in a control condition, or by causing more mice in the control condition to die than in the treatment condition. The statistical result can be identical in both cases, but of course the interpretations are quite different.

Randomization issues

One theory proposed to account for mind-matter interactions observed in inanimate systems, known as Decision Augmentation Theory (DAT) (May, Spottiswoode, Utts & James, 1995), suggests that the randomization process used in an experiment may help produce favorable results in DHI studies. DAT is based on the idea that people may have the ability to select favorable subsets out of random

sequences. Thus, when considering the many links involved in the causal chain of events that constitute all experiments, it may be that the investigator separates the treatment and control groups through the randomization process, in such a way as to optimize the inherent differences in people so that those in the treatment group are inherently slightly healthier and those in the control group are slightly sicker. The final outcome of the experiment will then appear to show that DHI caused the treatment group to fare better than the control group, but the actual source of the difference occurred at the very beginning at the experiment, mediated by the experimenter's "intuitive sorting skills" rather than by a healer or DHI.

If DAT is a viable explanation for DHI phenomena, it is better to use as many "decision points" as possible in the randomization process, rather than just one, if the goal is to obtain a significant outcome. For example, suppose 50 people are available in an experiment, with half being randomly assigned to a treatment and half to a control group. DAT implies that rather than generating one list of 25 numbers to assign people to the treatment group, it is better to generate a new, independent random decision for each individual's assignment until one group or the other has 25 in it. Certain experimental design choices might help to ameliorate whether this problem is operative, such as pre/post or within subject designs. Of course, if the goal of an experiment is not merely to optimize the statistical outcome, but to test a mind-matter interaction hypothesis vs. an experimenter "intuitive sorting" hypothesis, then it is better to use a single decision point.

Expectancy effects

Evidence of experimenter effects in DHI research (Juniper & Edlmann, 1998; Wiseman & Schlitz, 1997; Wiseman & Schlitz, 1999) and in general within parapsychology (Kennedy & Taddonio, 1976; Palmer, 1993; Schmeidler, 1997; Schmeidler & Michaeleen, 1981; Smith & Gordon, 2002; White, 1976) suggests that the attitude and beliefs of experimenters can be primary influencers of experimental outcomes, even beyond well-known psychological expectancy effects (Rosenthal, 1976). This effect may be mediated through the experimenter's intention, or it may be a psychosocial influence whereby the experiment's beliefs and goals are conveyed to the healers and other subjects in the study. A related observation, dubbed the "sheep/goat" effect (Lawrence, 1993), suggests that the beliefs of the subjects are related to observed outcomes, even when proper controls against expectation effects are in place.

Talent

Whatever else DHI may be, it is assumed to be a human ability. As such, as in any form of human performance, the healer's natural talent is probably an important factor. Little is known about talent in DHI, or whether such skills can be trained, but it seems likely that this sort of talent is distributed among the general population in much the same way as say, musical or sports talent (Utts, 1996). Thus, to help optimize the results of a DHI experiment, it is advisable to either incorporate a performance screening selection phase to identify people with demonstrable DHI ability, or to select healers on the basis of previously recognized experience and experimental results.

Environmental effects

Research on the relationship between local sidereal time (time according to the stars rather than local clock time) and remote viewing quality (Broughton & Spottiswoode, 2000; Spottiswoode, 1997) suggests that *when* and *where* sessions are conducted may be an important factor. For example, Spottiswoode (1997) found that "In an existing database of 1,468 free response [clairvoyance test] trials, the effect size increased 340% for trials within 1 hour of 13.5h LST [local sidereal time] (p = 0.001). An independent database of 1,015 similar trials was subsequently obtained in which trials within 1 hour of 13.5h LST showed an effect size increase of 450% (p = 0.05) providing confirmation of the effect (p. 109)."

Similarly, research on the relationship between geomagnetic field flux and results in both clairvoyant and telepathy experiments (Braud & Dennis, 1989; Persinger & Krippner, 1989; Radin, McAlpine & Cunningham, 1994; Radin & Rebman, 1998; Radin, Taylor & Braud, 1995) indicates that some uncontrollable but detectable geophysical factors may be correlated with results in DHI studies.

Physiological reactivity

Research on DHI effects in humans suggests that effect sizes are positively correlated with the target person's physiological autonomic reactivity as expressed e.g by indicators of "electrodermal lability" (Braud & Schlitz, 1983). That is, people with many spontaneous fluctuations in their autonomic electrodermal activity (EDA) are more likely to show positive DHI effects than those who are "stabile."

Analysis issues

Standard statistical models for experimental designs assume that observations are independent of each other and identically distributed, and that the observed results from one person to the next are not correlated with or influenced by each other. But if there really is an "interconnectedness" among living systems, as DHI phenomena suggest, then these assumptions are violated. In proof-oriented research focusing on hypothesis testing, this is not a serious problem because those analyses are performed assuming the null hypothesis is true, and when it is true the models are adequate. However, in most process-oriented research, and even when formulating confidence intervals in proof-oriented research, statistical assumptions may break down, leading to incorrect results.

Standard analyses typically compare means and proportions and use natural chance variability as a "yardstick" for making those comparisons. For instance, a treatment and control mean may be compared using a two sample t-test, which determines if the difference between them is large by comparing it to the natural chance variability in whatever effect was measured (e.g. white blood cell counts or electrodermal activity). However, the most interesting and meaningful information in that data may be patterns of "coherence" indicating interconnectedness, or changes in underlying effects over time, and such patterns may be overlooked with these standard analyses.

Conventional statistical models also often assume that there is a fixed effect that does not change over time. But, perhaps (as an example) as society's beliefs change to reflect a greater focus on spirituality, the effect of distance healing may become stronger. Or, as an experiment progresses, perhaps world events change in a way that causes individuals' immune systems to become stronger or weaker. Or perhaps there are inherent, psychologically, or psychosocially-mediated "decline effects" in these phenomena, as some researchers speculate (Bierman, 2000; Bösch & Walach, 2002a, 2002b; Lucadou, 2000). Such dynamic factors produce changes in effect sizes over time, but those changes would not be due to variations in the DHI treatments being studied. There are statistical models and methods that can accommodate correlated, time-varying observations and can be used to test for changes over time. The problem is that conventionally there may be no *a priori* reasons to expect that such complications might exist, so they can easily be overlooked when designing experiments. As a result, we suggest that DHI protocols should anticipate that effect sizes may be unavoidably dynamic and should plan for that possibility accordingly.

Sample DHI Protocols

The two most popular classes of DHI laboratory studies are experiments that (1) investigate a person's intention to interact with a distant person's autonomic nervous system, and (2) investigate a person's intention to interact with distant cell cultures. In both cases, the key independent variable is a healer's intention and the key dependent variables are fluctuations in a distant person's physiology or the activity of cells *in vitro*, respectively. The protocols presented here are simple examples of designs typically used in these classes of experiments.

Effect of distant intention on human electrodermal activity

This experiment is a laboratory analog of common distant healing practices (e.g., intercessory prayer) in that it tests the effects of one person's intention on another person's physiological condition. Much of the design focuses on ensuring that sensory and inferential cues that might systematically affect the behavior or physiological status of the healer, the subject, or the experimenter are eliminated. In this way it may be inferred that the healer's intention (under ideal conditions) is the independent variable that is explicitly manipulated in the experiment. This class of experiments is known as a *DMILS* (direct mental interaction with living systems) test.

Hypothesis

One person's intention can interact with a distant person's autonomic nervous system activity.

Statistical power

A meta-analysis (Schmidt, Schneider, Utts & Walach, 2002) estimated that the effect size per session in this sort of experiment (known as "remote staring", or "the feeling of being stared at") was Cohen's $d = 0.13$. To provide power $1\text{-}\beta = 0.8$ and alpha = 0.05 for a one-sample *t*-test, we require 368 sessions. As described below, the best type of analysis for this study is a permutation test, but power and sample size for such tests can only be computed by simulation (see e.g. Oden, 1991). Power and sample sizes for a one-sample *t*-test provide a good approximation.

As mentioned previously, effect size estimates usually assume that a phenomenon is stationary, whereas this is almost certainly wrong for DHI for many reasons. Thus, a standard power analysis is useful only for testing the null hypothesis under the assumption that the average effect size will be as stated, and is relatively fixed. Fortunately, because meta-analytic effect sizes usually take into account variability across many different types of conditions, this assumption is conservative and usually adequate.

Human subjects

A volunteer *healer* (S, or *sender* of intention) and *subject* (R, or *receiver* of that intention) are recruited for one experimental session. S and R read and sign informed consent forms describing the nature of the experiment, they may fill out additional questionnaires, and then R is seated in an electromagnetically and sound-shielded testing chamber and S is seated in a remote room in front of a video monitor. The locations housing S and R are separated by at least one intervening wall, and sound tests are conducted before the experiment begins to ensure that R cannot hear or feel any sounds or vibrations issuing from S's room.

Subject

R's electrodermal activity (EDA), a convenient marker of autonomous nervous system activity, is monitored with an electrodermal monitor, which is typically a computer-based device that digitizes and stores the analog signal at 10 Hz or greater, with 16 bits or greater resolution, over a recording range of at least 0 – 20 μS. In this example protocol, because the physiological monitor is located along with R inside an electromagnetically shielded chamber, to maintain the electrical seal the resulting digitized EDA-signals are sent outside the chamber through optical fibers.

The experimenter (E) places 8mm diameter Ag/AgCl electrodes on the thenar and hypothenar eminences of S's non-dominant hand by means of double-sided adhesive collars. The electrodes are coated with an isotonic electrode paste (typically with 0.5% saline in a neutral base) to ensure proper electronic contact with S's skin. The electrodes should be in place at least 15 minutes before the start of a data recording session to allow polarization of the electrode gel to stabilize.

A video camera inside the chamber is aimed at R and he or she is told that S will be observing his or her image at random times from a distant location during the session length of 30 to 40 minutes. After the investigator ensures that R's EDA data is being recorded and stored properly, he or she places headphones playing white noise around R's ears and secures R in the shielded chamber.

Healer

E then joins S in the distant room, and explains that when R's image appears on the video monitor S should intend that R become physiologically active or aroused, and when a different image appears (typically a neutral pastoral or forest scene) S should think about something else. One or the other image will appear on the video monitor for 20 seconds, with a short random interval between images. S is asked to continue performing the intend vs. no-intend task until the video monitor indicates that it is time to stop, generally in about 30 to 40 minutes. After S indicates that the instructions are understood, E secures S in the sender's room and returns to the experimenter's station, which is typically located between the S and R locations.

Experimental conditions

E now commands a computer to generate a randomized, counterbalanced sequence of random "intention to arouse" and "control" or no-intention-to-arouse instructions. The computer does this without feedback, so E does not know the sequence of instructions presented to S. These sequences consist of an equal number of ABBA and BAAB sequential orders (where A = intend and B = control). These sequences can be generated by having the computer produce a sequence of say, 16 random bits, with an equal number of 0s and 1s, and where 1 is associated with the ABBA sequence and a 0 is associated with BAAB. In this example experiment we therefore have a total of 16 bits x 4 orders = 64 *epochs*, where each epoch is defined as a 20 second recording period.

The randomized ABBA / BAAB order is employed to ensure that slow-moving physiological drifts in R are counterbalanced across the recording session; the equal number of ABBA / BAAB orders ensure that shorter-term fluctuations are also counterbalanced. To further guarantee that R cannot intentionally or inadvertently anticipate S's intention, each epoch is followed by a randomly determined 5 to 15 second inter-trial period, and the starting point of the experiment is randomly selected by the computer sometime within a two-minute window. This is why the sessions are described as taking between 30 and 40 minutes – the actual length of the experiment is not pre-specified. These strategies ensure that both R and E remain blind to the experimental conditions during the session.

The order of the conditions (A and B) is of major importance for these kind of studies. A minor error in the randomization and balancing procedure can introduce a fatal bias into the data set. Any proposed counterbalancing sequence applied must fulfill three criteria:

1) There has to be the same number of A and B epochs and they have to be the same length.

2) The sequence of the epochs must be randomized.

3) The sequence of the epochs must also be balanced to overcome natural variations in the recorded variables.

Criterion (3) is not always easy to accomplish. Sequences such as e.g. ABBAAB appear to be perfectly balanced at a first glance, but do not fulfill the criterion. A simple test for any sequence works as follows:

1. Write down the sequence and add below every letter a number starting with one and increasing with each step by one.

A B B A A B

1 2 3 4 5 6

2. Add all numbers below A and all numbers below B separately.

A = 1+4+5 B = 2+3+6

3. Compare the two sums for A and B. If the sequence is perfectly balanced the sum will be the same (unlike in this example).

Sum A = 10

Sum B = 11

Dependent variable: EDA

EDA is measured as skin conductance (SC) allowing for the assessment of two different parameters. Skin conductance responses (SCR) reflect fast-moving or *phasic* changes that can scored by their size (amplitude) or by their frequency (numbers per minute). Skin conductance level (SCL) is a slow changing or *tonic* measure of overall arousal that can be scored taking the mean of all data within a certain epoch. As there are EDA "non-responders," an inclusion criteron may be applied to the data. E.g., a session will be included in the study if the signal shows at least 6 nonspecific skin conductance responses (NS.SCR) above a threshold of 0.01 μS throughout the session. The presence of NS.SCR events can be determined via standard analytical methods applied to the signal (Boucsein, 1992; Schmidt & Walach, 2000).

Analysis

A permutation test is recommended to analyze EDA results (Blair & Karniski, 1993; Lunneborg, 2000; Manly, 1997) Such tests are nonparametric and guarantee the most sensitive results. The EDA signal may be statistically evaluated within-session for several pre-specified parameters; summary statistics per session may then be combined across sessions to provide an overall assessment of the experimental results. In some cases, data may be pooled across subjects and then evaluated as one grand dataset.

The following is an example of how such a permutation test may be conducted. Within a single session, all "intend" epochs and all control epochs are averaged, sample by sample, to create a single physiological curve for the mean intend and mean control epochs. Through this procedure two average epochs of 20 seconds each are obtained. Then the beginning of these two average epochs is clamped to zero by subtracting the value of the first sample from every other sample of the superimposed epoch. Next a mean difference epoch is calculated by subtracting the two mean epochs, sample by sample. Finally these difference scores are summed, resulting in a single, overall difference between the mean intend and mean control epochs. Call this value D_o. Under the null hypothesis, $D_o = 0$.

The next step is to conduct an exhaustive permutation of all possible *intend* and *control* conditions that could have been used in this experiment, and to compare the resulting D value for those conditions against the D observed in the actual experiment.

As a simple example, let's say we ran a short experiment with only 16 epochs. We would generate the random epoch assignments with 4 random bits, containing 2 0's and 2 1's. If the original experimental sequence was say, [0 1 1 0], then with 1 = ABBA and 0 = BAAB the order of the epoch assignments in the actual experiment would have been [BAAB ABBA ABBA BAAB]. We partition the SCL data according to these assignments and calculate $\mathbf{D_o}$ as described above.

Now we test all possible ways that these epochs *could* have been ordered. There are 6 possible ways: [0 0 1 1] [0 1 0 1] [0 1 1 0] [1 1 0 0] [1 0 1 0] [1 0 0 1]. We relabel the original SCL data points as though they were collected using the other 5 assignments, and calculate the resulting **D** value in each

case. Now we rank all resulting **D** values in increasing order and see where $\mathbf{D_o}$ lies. Thus *p = rank of* $\mathbf{D_o}$ / 6.

Now returning to our sample experiment, we used 16 random bits with an equal number of 1's and 0's, leading to 12,870 possible sequences. Assuming that S's intention resulted in greater SCL arousal as compared to the control conditions, then $\mathbf{D_o}$ will be ranked high among all possible values. Now determine p =rank of $\mathbf{D_o}$ / 12870. This p-value may then be converted to a one-tailed z-score using an inverse normal transform. A positive z-score will represent more arousal during *intend* than during *control* epochs.

To evaluate the results of the entire experiment, each session's z score is combined across sessions using the Stouffer Z method (i.e., SZ = sum(z)/sqrt(N), where N is the number of sessions). The final evaluation is the probability of that resulting Stouffer Z, which is itself a standard normal deviation.

Effect of distant intention on cell cultures

This protocol is designed to assess the response of cultured human cells to DHI. The use of cells as the object of DHI provides some advantages over human physiological or disease models. These advantages include (a) inexpensive "recruitment" of a homogeneous target population, (b) the ability to establish a control group devoid of psychological effects (e.g., a placebo effect), (c) the absence of extraneous healing intentions (e.g., prayers or distant mental intention from family members), and (d) rapid acquisition of objective outcome measures.

Protocol overview

Each trial involves a comparison of samples receiving experimental treatment with control samples receiving no treatment, and the type of treatment (DHI or control) is masked from the researchers handling the cells. In addition, trials are included that compare control treatment to control treatment (Walleczek et al., 1999) to assess environmental effects on the cell cultures independent of DHI. These multiple controls strengthen the design and allow us to pinpoint the underlying DHI mechanism.

Cell cultures

This sample protocol quantifies cellular responses to DHI, if any, using cultured human brain cells. Samples of cells are placed in separate culture plates inside an opaque, plastic treatment box. Prior to the start of the study, the cell population is expanded, divided into aliquots, and frozen viably for long-term storage. A fresh aliquot is thawed at the start of each trial to ensure uniformity in the genetic profile of the cells throughout the series of trials and replications. This strategy eliminates the potential for mutations to develop that might render cells less responsive.

For experimental sessions involving only control treatments, all manipulations of the cells will be the same as during a DHI treatment session except for the involvement of a healer. In these sessions, the treatment box will remain in the treatment room for 20 minutes without anybody entering the room and without anybody observing it.

Outcome measure of cell growth: Colony-forming efficiency

The assessment of colony-forming efficiency measures a cell's ability to duplicate itself repeatedly, thereby forming a colony, under various treatment conditions. This methodology, first developed in the 1950s (Puck & Marcus, 1956), was considered by the 1970s to be the gold standard assay in studies of *in vitro* sensitivity to therapeutic healers (Weisenthal and Lippman, 1985), and it remains a mainstay in the measurement of cell response *in vitro* (Gupta et al., 1996). Following standard methods (Yount et al., 1996), the experimental cells are seeded into 30 mm culture plates. For each experimental trial, 12 plates are seeded with experimental cells (6 independent samples for random assignment to treatment Box A and 6 for assignment to treatment Box B). To ensure the accuracy of colony counts, 100 cells are

placed into each culture plate so as to yield no more than 30 colonies per plate (plating efficiency is typically 5–30% with these cell types). Twenty-four hours after seeding, the plates will be randomly divided into two groups for treatment and then returned to their assigned positions. Two weeks after treatment, the cells will be fixed and stained, and colonies of more than 50 cells will be counted. Images of each plate will be documented (using a digital imaging system) and archived. A low-power stereoscope will be used to count colonies manually.

Blinding and randomization procedures

All samples will be coded so that persons interacting with the healers and those handling the test samples do not know the contents of the treatment boxes. In creating the target system, there are several randomization steps that should be followed to eliminate experimenter expectation effects, handling effects, and biases in analysis. In this sample protocol, the handling of the target system is divided into steps that involve blinding each researcher involved in the experiment. The steps are as follows:

1. Scientist #1 prepares 12 cell culture plates and labels each with a five digit random number. Next each plate is placed into a randomly assigned position in a cell culture incubator. Scientist #1 then supplies the identifying codes to scientist #2, who randomly assigns each plate to treatment box A or treatment box B. Scientist #2 then sends copies of the identifying codes and random assignments to an independent peer (a code keeper).

2. On the following day, scientist #1 transfers half of the culture plates (six independent samples, according to the plan supplied by scientist #2) from the incubator into positions randomly assigned in box A. Scientist #1 then transfers box A to the treatment room, immediately leaves the area, and signals scientist #3 via an electronic signaling system (a nonverbal signal) that the box is in place and ready to receive treatment. Remaining blinded to what type of treatment was delivered, scientist #1 returns 30 minutes later to carry box A back and place the plates in their original positions in the incubator. This is repeated immediately for the second session of the trial with the remaining half of the plates in box B.

3. Each trial is randomly assigned as DHI/control or control/control, and scientist #3 sends copies of these assignments to the code keeper. Each session is either a control session, with the treatment room remaining empty, or a DHI session, with the healer escorted into the room by scientist #3 to conduct the healing treatment. Both the healer and scientist #3 are blind to the identifying codes on the plates.

4. After being returned to the incubator, all cell culture plates remain in position for two weeks to allow time for cells to divide and form colonies. The colonies are then fixed and stained for counting. Scientist #4 counts colonies in each culture plate using only the random five digit codes and sends copies of the results to the code keeper.

5. A biostatistician, who is blind to what treatment each group had received, conducts the statistical analysis. Data analysis is completed using only the code number of the culture plate. The codes are revealed after completion of the statistical analysis.

Definition of an experiment

The data consist of N replications of treated vs. control plate colony counts. Each run results in counts of colony-forming units (CFUs) on 12 plates, 6 for DHI and 6 for control. Two means are calculated for each run: one mean for the 6 treated plates and one mean for the 6 control plates. A paired t-test is used to compare the means of the N runs (DHI vs. control). Sometimes a log transformation is necessary to make the mean counts normally distributed.

Statistical power and data analysis plan

Data from previous studies or a pilot study should be used to determine the sample size required to achieve specified power at a specified significance level. Often, power is set at 80% and the significance level is set at $p = 0.05$.

The biostatistician should be blind as to which set of plates are treated. This will ensure that unrecognized bias does not influence the statistical analysis. It may also be useful to examine factors such as chronological time or plate storage location to see if they had any influence on experimental results. If there is evidence of such effects (i.e., if these factors are considered statistically significant when entered into the analysis of the data), then the analyses will be repeated with these factors entered as covariates.

The example protocol is typical of a proof-oriented study, which is useful in establishing the efficacy of an effect, but it does not help in clarifying mechanisms. There are many ways one might wish to examine the underlying processes, for example one might explore whether the effect varies with distance, or treatment duration, or the healer's motivation, or focus of intention, or healing "dose."

Conclusions

This paper has focused primarily on guidelines for conducting proof-oriented DHI research. Previously published studies have explored DHI using process-oriented designs, but the phenomenon is so complex that little has been learned about the underlying processes. For example, almost nothing is known about the characteristics of the healers. Here a synthesis of qualitative and quantitative designs may be useful (e.g., Schlitz and Braud, 1985). Other basic issues that need to be investigated include the effects of geophysical and meteorological environments, psychosocial variables, ways of measuring intention, questions of spatial and temporal variables, ways of shielding the effect, development of explanatory models, and DHI phenomena in evolutionary history. DHI may play a role in clinical outcomes, but this has yet to be fully addressed (cf. Dusek, this volume). For the most part, human subjects in DHI studies have been healthy volunteers without clinical symptoms. Thus, the extent to which laboratory-based meta-analytical effect sizes may translate to the pragmatic treatment of disease remains unknown.

It should be clear by now that the guidelines discussed in this paper can assist but not guarantee that a study will be methodologically sound. Every research question is unique and each investigation has its own distinctive pitfalls and challenges. Even the most pristine study protocols are influenced by factors like the difficulties of ensuring subject compliance, higher than anticipated drop-out rates, and limited resources. But beyond all the usual practical problems, a key challenge in conducting DHI research is measuring and controlling the independent variable: *intention*. How do we establish what is actually going on in the mind of the healers? Measuring subjective states is a non-trivial problem, and finding ways of measuring intention objectively remains an elusive research ambition. Also, given that the central hypothesis is that intention has nonlocal effects, how do we control *whose* intention is affecting the experiment, and *when*? Moreover, because human behavior is dynamic, it is not reasonable to assume that the beliefs and intentions of the investigator(s) remain constant, or that they can be strictly shielded from influencing the system under study.

Indeed, the recursive nature of the independent variable (i.e., the researcher's intention to study intention), combined with our limited understanding of phenomena that apparently transcend time and space, makes it unrealistic to expect that DHI studies should be easy to replicate. Nonetheless, standardized experimental protocols with systematic variations of select parameters should eventually increase the likelihood of replication and foster the development of theoretical models to explain the observed phenomena.

Given that this research domain raises fundamental questions, the current lack of adequate explanatory models may lead to undesirable variation in the execution, analysis, and interpretation of results. Because of these uncertainties, researchers should explicitly examine and formally state their

theoretical assumptions in published reports. In particular, most scientists tend to imagine that DHI works something like a radio, in which a "transmitter" sends "energy" to a distant "receiver" who accepts it and is thereby influenced, and hopefully healed. However, the postulate of DHI implies that what is commonly believed to be independent, separate and isolated (i.e., intention) is in fact not isolated at all. In this sense, studies on DHI do not necessarily examine how independent variables unidirectionally influence dependent variables but, rather, how a manifold of *interdependant* variables influence each other in *nonlocal* ways. For example, there is a class of theoretical models that rely on the assumption that DHI are not traditional causal effects but nonlocal correlations. Such models, inspired by quantum mechanics, postulate that healers and subjects are parts of the same system, and thus behave in correlated ways. Such non-local correlations have been empirically demonstrated and are in agreement with known scientific principles (Atmanspacher, Römer & Walach, 2002; Walach, 2000; Walach & Schmidt, in print).

In the end, no written guidelines can replace active communication with peers who have had direct experience with these research issues. And no guidelines can avert the challenges that come with a topic that is fraught with risk, unintended consequences, and baffling mysteries. These challenges, both frustrating and joyful, always accompany the intention to explore frontiers of the known.

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