

LETTERS TO THE EDITOR

Comment on "The Effect of the 'Laying-On of Hands' on Transplanted Breast Cancer in Mice"

by W. F. Bengston and D. Krinsley

In this provocative paper (JSE, Vol. 14, No. 3, 2000), Bengston and Krinsley report that five independent experiments on mice with adenocarcinoma, which is typically 100% fatal, treated with "laying on of hands" by trained individuals demonstrated an overall remission rate of 87.9% ($n = 33$). Tumors among the experimental mice developed a "blackened area," ulcerated, imploded and closed, leaving the mice to live a normal lifespan (Bengston & Krinsley, 2000, p. 353). Furthermore, reinjection of cancer into the mice in remission did not result in new carcinomas, suggesting that the mice had developed a lasting immunity to the disease.

These results are undeniably similar to numerous experiments utilizing the principles of "radiation hormesis" for treatment of cancerous tumors. Large and small doses of ionizing radiation evoke opposite, or reverse, biologic effects; this has been defined as hormesis (Luckey, 1980). Excess radiation, >10 Gy (>1000 Rad/y), is harmful. Low dose irradiation, <1 Gy (<100 Rad/y), has been demonstrated to be beneficial. This hormetic effect has been shown to exist for drugs, hormones, vitamins, and essential minerals as well as for ionizing radiation (Heiby, 1988). Numerous animal studies indicate a radiation-induced hormesis occurs in major physiologic functions (Luckey, 1980, 1991) and at all levels ranging from biochemical to organismal (Macklis & Beresford, 1991). Studies include the statistically significant, $p < 0.01$, decreased cancer mortality rates in more than 30 experiments with rodents reported in peer-reviewed journals (Luckey, 1991).

Of significance to the conclusions drawn in the Bengston and Krinsley study indicating "that there is a stimulated immune response to treatment" (Bengston & Krinsley, 2000, p. 362) is the finding by researchers at the Komae Research Laboratory in Japan that confirmed an increase in the anti-cancer substances in mice spleen cells (interleukin-6, which enhances immune activity, and TNF, or tumor necrosis factor- α , which "blackens" and kills cancerous cells, similar to that noted in the Bengston and Krinsley observations) demonstrating that the anti-cancer effect observed is due to enhancement of immune activity by the administration of low dose radiation (Keiichiro Ishii Komae Research Laboratory, 1996).

This anti-cancer trend extends to humans as well. One of the most remarkable studies related to radiation hormesis among humans was conducted on seriously ill Stage I and II non-Hodgkin's lymphoma patients by Dr. K. Sakamoto of the Tohoku University in Japan. Fractionated doses of 10 cGy 3/week or

15 cGy 2(/week were given for 5 weeks for a cumulative dose of 150 cGy. Both whole body and half-body low-dose ionizing irradiation were tested. The results demonstrated that half-body irradiation (HBI) of the rib cage area (thorax from xyphoid process to suprasternal notch) was as effective as whole body irradiation (TBI). In some patients, tumors completely outside the HBI field disappeared after HBI alone. Analysis of peripheral lymphocytes demonstrated immune system stimulation. The 10-year survival of patients receiving only standard protocol local high-dose radiotherapy and chemotherapy is 65% ($n = 94$) compared to 84% ($n = 23$) 10-year survival of patients receiving additional low dose TBI or HBI ($p < 0.05$) (Sakamoto, 1997).

In addition to Sakamoto's long-term non-Hodgkin's lymphoma research, others have also clearly demonstrated cancer suppression with low-dose total body irradiation. Chaffy et al. (1976) reported that whole body doses of less than 25cGy were effective for tumor control of lymphosarcoma, while Choi et al. (1979) demonstrated cancer remissions with low-dose TBI with advanced non-Hodgkin's lymphoma. Others, like Holder (1965), observed that multiple myeloma could be successfully treated by low doses of TBI. Despite the difficulties inherent in demonstrating hormesis in carcinogenesis testing protocols, there is sound epidemiological support for its existence. Radiation hormesis researcher, Myron Pollycove, M.D., cites several examples, including reductions in lung cancer incidence with increasing radon exposure and reductions in breast cancer rates in Canadian women exposed to low doses of radiation from lung fluoroscopy. Additionally, Pollycove (1998) concludes that a ten-fold increase of annual background radiation stimulates overall biosystem activity by approximately 20%, producing a significant decrease in the metabolic rate of mutations and corresponding decreases of cancer mortality and mortality from all causes.

But how is radiation hormesis linked to hand-mediated alternative healing modalities? Preliminary studies by Benford et al. with "laying on of hands" bioenergy healing techniques demonstrated statistically-significant decreases in external gamma radiation measurements during the course of actual therapy sessions. These studies concluded that individuals, skilled in the art of bioenergy techniques, induce the fluctuation of high-energy light waves (photons) more dramatically than those who are not trained in bioenergy techniques, regardless of purposeful intentions to heal. During these initial preliminary tests involving bioenergy therapists and volunteer subjects, total counts were recorded in 100-second trials separately over the subject's crown, heart, abdomen and pelvic regions using a NaI(Tl) crystal scintillator which detected gamma radiation from approximately 100 KeV to 3 MeV. Later tests involved 300-second counts over the heart and pelvic regions only.

The results demonstrated that gamma radiation levels markedly decreased during therapy sessions of 100% of subjects and at every body site tested regardless of which therapist performed the treatment. In many instances, the gamma counts fluctuated by thousands within the short time periods analyzed. T-tests were used to determine statistical significance, with p values ranging

from $p = 0.035$ to $p < 0.0001$, in the 100-second trials, and $p < 0.00001$ in the 300-second trials (Benford et al., 1999, in press).

It has been theorized that this decrease in external gamma counts is due to 1) an enhanced absorption from the readily available gamma radiation in the environment; 2) changes to the rate of emissions from naturally-produced internal gamma radiation in the body; or, 3) a combination of both mechanisms. In any case, the crucial net effect is increased internal ionizing radiation leading to enhanced radiogenic metabolism (Benford, 2001).

Also of note are the authors' anecdotal findings that "speed of remission is a function of size of the animal," (Bengston & Krinsley, 2000, p. 364), or restated, mice responded much better than humans. This would be logical if, in fact, part or all of the anti-cancer effect noted is due to modulation of ionizing radiation during the healing sessions. Why is this the case? The healers tested in the above referenced gamma radiation experiments only influenced a proportionately small amount of gamma radiation, most likely not enough to single-handedly induce a full remission in a large animal like a human. However, given a smaller target, it is logical to posit that these subtle doses might be more effective, thus, produce the full remissions observed in this study. In a videotape demonstrating a QiGong healing of a man with a bladder cancer, it took 4 healers to obliterate the large tumor (Chan, 1996). Have the QiGong Masters discovered the need for more than one healer during these "big jobs?" Is there a link between hand-mediated healing energy modalities and radiation hormesis in treating diseases? If so, what variables play a role in determining the healing energy "radiation absorbed dose," or rad, per person and per disease?

This groundbreaking study raises a number of intriguing and crucial questions within the auspices of alternative healing and cancer therapies. It, perhaps, will initiate a new paradigm in which people with cancer and other diseases are treated by the medical community. I hope the authors will continue their work until all these questions are satisfactorily answered.

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Directions in Healing Research: A Reply to Benford

The commentary by Benford is interesting and informative. Let me state at the outset that I have no expertise or experience in the area of radiation hormesis, though I would like to take this opportunity to react to selected comments and to suggest some potentially fruitful lines of healing research.

Benford writes that the cures of mammary adenocarcinoma that we produced in mice (Bengston & Krinsley, 2000) are similar to experiments which use radiation hormesis for treatment of cancerous tumors, including statistically significant decreases in cancer mortality rates in more than 30 experiments with rodents. It is indeed suggestive that low dose radiation is reported to also produce a blackened area in tumors and to enhance immune activity. I suspect, however, that the pattern we found of blackened area to ulceration to tumor implosion to full life-span cure is unique.

The studies by Benford et al. (1999) which demonstrate statistically significant decreases in external gamma radiation during laying-on of hands are important and suggestive of an important line of research. That is, what physical changes accompany successful healing through non-traditional means? Since the publication of our article, many researchers have contacted me to inquire about what we know in this area, and they have suggested a variety of electromagnetic detection tests that they would like to perform in our future experiments. To date I have responded simply that we have not yet tested any physical parameters in either the immediate environment of the animals or any changes in the volunteer subjects themselves. I fully agree that these are important areas for inquiry.

Once there are reliable physical healing effects, as Benford has apparently also obtained, innumerable research possibilities emerge. Can, for example, the healing effect be screened or filtered? Does distance degrade effects? Do multiple treatments or simultaneous treatments by healers produce additive effects? Can the healing "energy" be stored in a physical medium? Serious col-

laboration among researchers from the physical and biological sciences can help address these questions.

There are a few points of clarification that need to be made in Benford's commentary. Citing one of her publications (2001), Benford writes that "the crucial net effect [of healing] is the increased internal ionizing radiation leading to enhanced radiogenic metabolism." While I have no basis to comment on the applicability of this hypothesis to our work, I can comment upon the inferences she draws to some anecdotal observations in our paper. At the end of our paper, we noted anecdotally that in producing remissions in other mammals, the speed of remission is a function of the size of the animal. We wondered whether this is a function of a given amount of energy being generated by the healer or perhaps is a function of the metabolic rate of the animal. Benford notes that the differential rate is explainable if we assume that the healer can influence only a small amount of radiation, and the anti-cancer effect is due to this modulation of ionizing radiation. Simply put, the relatively small effects produced by healers will be more efficient on relatively small targets. Or, in her words, "mice responded much better than humans."

This may not be the case. Our anecdotal comments were not intended to compare mice to humans but really were directed toward many different species of progressively larger mammals, including humans. Benford notes that QiGong masters have discovered the necessity of four healers for "big jobs" such as a man with bladder cancer. We have not found this to be so. Speculating, if we were to carry out an experiment trying to simultaneously cure 50 mice, I suspect they would remiss at the same rate as if we had treated 10 mice. In short, my guess is that it really is a function of internal metabolic rate. In any event, the good news is that now that we have a reliable healing effect we can readily resolve this issue and hopefully will know the answer soon.

Finally, I would like to briefly comment on a new direction to healing research first alluded to in our paper. While all of the aforementioned basic research questions are important, it is possible to simultaneously proceed on another front. Among our observations with the mice was that once cured, no mouse ever had a recurrence of cancer (this is also true of other mammals). Further, even upon reinjection, no mouse was able to contract the cancer again. This phenomenon, I believe, may allow us to reproduce the healing effect using more conventional means. Even if we don't yet understand the physical and biological mechanisms by which the mice were cured, it still may be possible to use cured animals to recreate the remissions without the laying-on of hands. Could we take the blood of cured animals and develop a vaccine against breast cancer, even if we don't yet understand the underlying mechanisms by which the animals were cured? I believe it is worth trying immediately, even as we design more basic research. In addition, I welcome the thoughtful collaboration of researchers such as Sue Benford.

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**Comment on "The Effect of the 'Laying-On of Hands'
on Transplanted Breast Cancer in Mice"
by W. F. Bengston and D. Krinsley**

I have been a JSE subscriber for several years and have always been impressed by the high level of academic standards the journal meets. It was therefore a disappointment to note that the Journal's peer review process failed to meet those standards in the Fall 2000 issue containing the paper "Effect of the 'Laying-on of Hands'" by William F. Bengston and David Krinsley. The conclusions of the paper are truly astounding: that it is possible to repeatedly cure at least one type of cancer in mice by the laying on of hands.

The failure of this paper to meet the elementary requirements of scientific exposition are twofold. The lesser failure is on p. 355 where Bengston reports that "Our research grew out of an attempt to empirically test a New York—based healer." He goes on to relate that the success of this "healer" in treating various medical conditions. However, he never names the healer. Since it is therefore impossible for an independent scientist to confirm the credibility and ability of the healer who allegedly developed the healing methodology, the normal scientific standards for checking claims are thwarted.

The greater failure is on p. 356, where Bengston fails to describe the processes used to bring about the miraculous remissions of mouse cancer. About the healing treatment Bengston has only this to say:

Over the course of months of questioning by Bengston about the process by which the healer was able to treat others, techniques were developed wherein the healer claimed that complete skeptics could be trained to reproduce healing effects. The techniques did not involve belief of any sort, nor did they include meditation, focused visualization, spiritual discipline, or lifestyle changes. The initial techniques involved a series of routine mental tasks that were not directly intended to produce healing. Subsequent to mastery, these would be followed by laying on of hands. The mental techniques required several weeks of practice to achieve sufficient mastery to move to the laying on of hands techniques.

Bengston adequately describes what the healing techniques do not involve but fails completely to describe what they do consist of. In the remainder of the paper Bengston identifies these techniques only as "mental processes" or

"the technique." Thus does it appear that Bengston may want to keep his methodology secret for his own subsequent aggrandizement. Obviously, such a course meets neither the spirit nor the letter of the scientific method and regrettably casts serious doubt on the veracity of the astounding feats of healing Bengston claims to have accomplished.

I would be very interested to receive your reply to my charge that JSE published a paper which apparently departed from well-established and accepted scientific standards.

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Scientific Disclosure: A Reply to DeHarpporte

DeHarpporte accuses the *Journal of Scientific Exploration* of failing to meet its normally high level of academic standards by publishing our paper. His commentary deserves a serious reply.

We are accused of two transgressions in failing to report adequate details. The first is that we never disclose the name of the New York—based healer who, with Bengston, developed the healing techniques. DeHarpporte opines that since it is impossible for an independent scientist to confirm the credibility and ability of the healer, the standards of science have been subverted.

My reply to this charge is simple. The lack of identification of the healer is deliberate and irrelevant and serves to protect the privacy of an individual who is completely unrelated to the experimental work. Our paper never makes empirical claims about the healer, nor are we interested in assessing his "legitimacy." A simple thought experiment can illustrate: imagine that the healer is assessed by someone such as DeHarpporte and is found to be wanting in some way. Would our empirical results be any less noteworthy? They would perhaps be even more perplexing, but I submit that our data stand with or without this unnecessary disclosure.

The second charge of non-disclosure is more important: that is, "where Bengston fails to describe the processes used to bring about the miraculous remission of mouse cancer." First, I take exception to the characterization of the remissions as "miraculous." I suggest to DeHarpporte that the phenomenon of remissions will yield to scientific description and explanation, though we certainly do not understand them at present.

I have indeed not adequately described the techniques used in our experimental work. And I understand the expressed frustration. Had others written the paper, I too would be clamoring for more details. To reinforce, I have had at least a dozen requests for more information about the techniques since publication. There are several reasons for this lack of disclosure. First, I don't know how to reduce the techniques to a manageable length so that they would be comprehen-

sible and reproducible. I trained groups of people in the second through fourth experimental procedures described in the paper. The training period lasted six weeks and involved a number of different techniques for mastery. And in each case, the pace and procedures of training were modified to the idiosyncrasies of the group. Simply put, we were not following a lock-step procedure, and I don't even know if I could summarize all that we did in a hundred pages.

A second reason is perhaps born out of caution. It is not, as charged by DeHarpporte, "that Bengston may want to keep his methodology secret for his own subsequent aggrandizement." Instead, it is simply this: I don't know which of the myriad techniques produce the healing, and as such, I don't want to make unsubstantiated or premature claims. I taught numerous mental techniques and at least two methods of laying-on of hands, all of which were applied to the mice. And, we treated for a very long period of time. We have not yet determined if all this was necessary to produce the healing effect.

In our paper we described our frustration with some on-site control mice also remissing. Recall that we had to send a second control group to another city to have the predicted fatality pattern. This clearly suggests that we don't understand which techniques are truly necessary in order to effect cures. Future work, with properly trained people, will enable us to know what is necessary. With this in mind, I welcome replication by independent labs.

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Bengston and Krinsley (*JSE*, Vol. 14, No. 3, 2000) report their successful healing treatment of cancerous mice, but they are puzzled about the results of the third experiment: biology students, chosen by the "extremely skeptical chairperson," were unable to produce remissions in the laboratory but able to do so at home (p. 362).

Perhaps the "scientific logs" were the important factor or, perhaps, the intellectual "field" of the laboratory was different than that of home? [See Rosenthal, Robert. (1966). *Experimental Effects in Behavioral Research*. New York: Appleton-Century-Crofts.]

Best wishes to all involved in the research — including the mice!

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Anomalies in Anomalous Research: A Reply to Sprinkle

The results in our third healing experiment are indeed puzzling. To recap: in our previous experiments, some control mice which were visited by people trained in the healing techniques developed the remission pattern of blackened area, ulceration, and tumor implosion to full cure. This of course implies that some sort of "field" is responsible for the remissions.

Among the questions in our third experiment was to find out if all volunteers could independently remit the mice. Because of the apparent "field" effect of remissions, we reasoned that if even one volunteer were able to produce remission, then all experimental mice in the lab would remit. The question to us was whether each individual could remit a mouse at home, where there was no chance of contact with trained people. We predicted that the remission rate would be higher in the lab than at home.

The results contradicted our prediction. All five of the volunteer's home mice remitted, while the mice of the three biology students died in the lab. Sprinkle suggests that perhaps the scientific logs of the biology students were the key factor, or perhaps the intellectual field of the laboratory was different than at home.

In the paper we speculate that intellectual activity is antagonistic to the production of healing effects (Bengston & Krinsley, 2000), and so we have no disagreement with Sprinkle. But that really doesn't explain why the control mice remitted in that experiment, presumably due to a field effect, yet the experimental mice did not. Even if the biology students were unable to generate healing in the lab because of their scientific logs, the "overflow" effect of the two successful healers in the lab should have had an effect on the biology students' mice, which were in close proximity. The only logical way to explain the death of the biology students' laboratory mice, even while the control mice in the lab remitted and all home mice remitted, is to posit that the biology students somehow exerted a "negative" effect on their individual lab mice.

Sprinkle's related idea is that the intellectual field of the lab was different than that of the home. This is most certainly the case. The lab was clearly not one supportive of healing research, and surely the biology major volunteers felt uneasy. Perhaps this had an effect on the overall pattern of healing. Bernard Grad, a pioneer in healing research, has always stressed that a supportive environment is necessary for successful healing results. In addition, he has maintained that a negative environment can produce negative effects (Grad, 2000).

Does any of this make sense? Frankly, the results of the third experiment continue to baffle me. If indeed there was a negative influence by the biology students on their experimental mice, then why were the non-biology students able to cure their lab mice? Obviously I must conclude that more research is needed and that even anomalous results have anomalies.

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Are Plate Tectonics the Wrong Answer to the Right Question?

The critique of plate tectonics by David Pratt in JSE (Vol. 14, No. 3) is very stimulating and contains impressive, detailed data. But it left me wishing for mention of the important possible role of fundamental physics in the evolution of the earth and its possible connection with cosmology.

Pratt points out that when Wegener's inferred separation of the continents with time was finally accepted that plate tectonics then postulated continents skating aimlessly about on a soft upper layer of the mantle. But as long ago as 1958 S. W. Carey reported detailed geological data which contradicted this model, including evidence that no significant subduction of one continental plate under another had occurred. Carey and K. M. Creer (1965), among many others, showed how accurately the continents fitted together in the past and argued how the observed sea floor spreading in the mid — Atlantic ridge supported the expanding earth interpretation. The irony here is that the long refusal of conventional geology to accept Wegener's discovery was because of the belief that the continents should not float around. But the expanding earth interpretation kept them anchored in basaltic rock and explained their gradual separation with time. Pratt now brings more strong evidence to bear falsifying the hypothesis of drifting continental plates. What explanation is left?

The Olympia conference (1993) had a whole section of geologists arguing that the expanding earth was powered by a secular gain in mass. (I was there and I recommend reading it.) Ironically, further along in the same JSE issue as Pratt's article, is mentioned (p. 484) Tom Van Flandern's exploding planets hypothesis (the asteroid belt parent and the original Mars). That certainly makes one consider what would happen if a planet kept adding mass at its core.

Then on p. 449 of JSE (Vol. 14, No. 3), the 18th century physicist Le Sage is cited as postulating gravity is pushing and caused by a sea of gravitons. If these much faster than light gravitons are absorbed in the process of furnishing gravity then they should add mass to fundamental particles as time goes on. It is suggested on p. 449 that this is the cause for the very controversial non-velocity redshift in quasars and galaxies which threatens to disrupt current cosmology. So perhaps this fundamental change in the assumptions of physics is connected with the observations which are being contested under the rubric of plate tectonics.

It is appropriate to quote Creer from his 1965 article "... we should beware of rejecting the hypothesis of [earth] expansion out of hand on grounds that no