Thought Field Therapy Is Proven Efficacious: Why the Critics Are Wrong About TFT

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ABSTRACT

Critics of Thought Field Therapy, in the October 2001 Journal of Clinical Psychology, make the critical reasoning error of confusing demonstrations of a replicable phenomenon, (TFT), with proposed theories which purport to explain such a phenomenon, (i.e., such as "meridians" or "individual energy toxins"). Various misrepresentations by the critics of some claims made by Callahan and his TFT colleagues are highlighted. The difference between "proof" and "persuasion" is addressed. TFT's demonstrated efficacy is circumstantial evidence for the existence of an energy meridian system.

Counter-explanations such as placebo, suggestibility, statistical regression to the mean, etc., are shown to not be plausible explanations after all. Contrary to the claim of some critics, inappropriately low HRV has in fact been shown to be a marker of psychiatric disorder. Critics are challenged to try experiments with TFT, and see for themselves that the claims for TFT are indeed credible. With the robust findings as presented by Callahan and his colleagues, which are easily replicable, Thought Field Therapy's efficacy has been proven.

Key Words: Thought Field Therapy, heart rate variability, (HRV), subjective units of distress, (SUD).

Introduction

This paper is in response to the October 2001 Journal of Clinical Psychology (JCLP) issue, devoted to the topic of Thought Field Therapy. After reading all the articles, I felt it necessary to respond in detail to objections I had to the critics' articles including: numerous critical reasoning errors, misunderstandings of the actual claims for TFT, several straw-man arguments, a false a priori assumption, arguments from false premises, and citing of references that, when checked, are found to not at all support the specific point being made, by the critic. One critic even cites an article he co-authored that actually contradicts the point he's making, and instead supports a significant point made by TFT proponents!

Please note, I am not in any way an official spokesperson for Dr. Callahan, or for Thought Field Therapy. Except where otherwise noted, the page numbers I cite in this article refer to the October 2001 JCLP.

McNally's Criticism

First, let's carefully examine Richard J. McNally's criticism (McNally, 2001), of Callahan's Journal of Clinical Psychology article, "The Impact of Thought Field Therapy on Heart Rate Variability", (Callahan, 2001a). On page 1172 McNally claims, "the flaws in his [Callahan's] work render it uninterpretable". He goes on to criticize Callahan's method of choosing 'successful' cases, by saying, "This method of selecting cases is unlikely to persuade anyone that TFT is an effective treatment for anything", and also, McNally claims the cases are "poorly characterized". McNally offers several alternate possible explanations for Callahan's results, including uncontrolled demand characteristics, therapist expectancy (bias), and placebo. He also claims that Callahan applied HRV "out of context" as an outcome measure, and also, that "any of these pre-treatment/post-treatment changes could have occurred with the mere passage of time". Thus, McNally's point is that there is no phenomenon actually occurring, when someone gets a proper TFT treatment.

Is There a Significant Phenomenon Here at All? Vs. Do We Have an Adequate Theory To Explain It?

It is very important, when criticizing TFT or any new therapy, to separate such critiques into at least two separate types of questions: A) Is there a significant phenomenon at all, here? Has any phenomenon actually been demonstrated, one that cannot be written off as merely due to a placebo response, or due to demand characteristics of the experiment/demonstration, therapist bias, or any other more parsimonious explanation? If the answer is "yes", then (and only then) can the second type of question properly arise: B) What is the cause of this phenomenon? How and why does TFT work? What are the actual mechanics of it, that cause it to work?

In a collection of essays on the philosophy of science, in an article titled, "The Truth Doesn't Explain Much" Cartwright writes, "Scientific theories must tell us both what is true in nature, and how we are to explain it. I shall argue that these are entirely different functions and should be kept distinct. Usually the two are conflated." (Cartwright, 1988, p129). Many TFT critics, including most of the critics in JCLP, engage in much conflating! For whatever reason, very often, TFT critics hopelessly confuse these two separate, distinct issues, in their attempts to "debunk" TFT. As a matter of simple, good critical reasoning, it is quite possible, when studying a new, allegedly effective treatment, to answer "yes" to question A, ("what is true in nature"), and yet either have no idea what the answer to question B ("how we are to explain it") is, or harbor doubts about any proffered theory purporting to explain the phenomenon. If the answer to question A ("what is true in nature", i.e., "Is there something significant and/or remarkable going on here?") is "yes", then no amount of skepticism about question B explanations can ever refute the answer to question A. Indeed, to claim that question A is refuted, because the skeptic finds the answers to question B inadequate, is the gross error of "begging the question." Yet, McNally continually makes this very error, when he criticizes Callahan and TFT for "lacking any credible theory…" (P.1173).

Callahan's purpose, in his article, obviously was not to offer or justify a theory to explain how and why TFT works, (a question B type question). His goal was much simpler. He was merely demonstrating, using HRV as one outcome measure, the simple, easily verifiable (replicable) "raw fact" that a truly significant phenomenon is occurring, when TFT treatments are properly applied; a phenomenon that cannot be explained away by any more parsimonious counter-explanation, (placebo, demand characteristics, therapist expectancy/bias, etc.) In other words, Callahan was answering a type A question. What a critic cannot rationally do is argue that, if Callahan's theories to explain the alleged efficacy of TFT are not satisfactory, then therefore the "raw fact", the phenomenon itself must not actually occur.

It's very interesting to me that McNally uses Creation Science, and the lack of critical reasoning behind Creation Science claims, to illustrate why "not all ideas deserve our scrutiny". I personally have had significant experience debating with and debunking arguments from Creation Science advocates. Let me make this very clear: McNally and many other TFT critics make the very same critical reasoning errors in their arguments against the claims for TFT, that Creation Science advocates quite often make, criticizing evolution!

To illustrate what I mean: Just like many of TFT's critics, Creation Science advocates confuse the theories of evolution, (a type B question), with the straight-up, undeniable fact of evolution, (Question A). Reasonable people could doubt or question any particular theory of evolution, offered to explain the mechanics behind evolution, such as the theory of punctuated equilibrium, (roughly, the theory that evolution occurred little or not at all for vast time periods in a particular population, "punctuated" by relatively short periods of large jumps in evolutionary development). However, any solid, even devastating evidence which "debunked" punctuated equilibrium would NOT constitute a refutation of the FACT of evolution, (i.e., the "raw fact", evident to anyone who cares to closely observe the evidence, that more complex species came about due to changes in simpler species, over vast amounts of time).

Creation Science advocates often tout valid scientific disputes about various proposed theories to explain how and why evolution actually occurred, as evidence that evolution itself (the basic fact that more complex species evolved from simpler species) is thusly refuted or in serious doubt. Likewise, using similar flawed reasoning, TFT critics think that criticism or alleged "debunking" of answers to question B, (how and why TFT works, the actual theory behind it) is somehow a refutation of the answer to question A, that yes, a phenomenon is in fact occurring, when TFT treatments are applied, which cannot be more parsimoniously explained by placebo, therapist bias, expectancy, etc. Thus, no criticism of concepts such as "Qi" or "energy meridian" can ever be a refutation of the simple, demonstrable and repeatedly demonstrated matter of fact, that TFT works.

The Three Categories of Scientific Claims

Callahan was demonstrating a phenomenon which requires an explanation. He was not attempting to formally offer a theory which explains the phenomenon. Such theory would be the proper and appropriate topic for another article (see, e.g., Stop the Nightmares of Trauma, by Callahan and Callahan, [2000].)

As Callahan wrote in his third article in JCLP, ("Thought Field Therapy: Response to our Critics and a Scrutiny of Some Old Ideas of Social Science", [Callahan, 2001c, on p.1252]), medical doctors recognized the phenomenon that nitroglycerin relieved angina, many years before it was understood why and how nitroglycerin worked to relieve angina. As another example of the difference between phenomenon, and theories that purport to explain how and why a particular phenomenon works, consider the following: "A series of laws, called the gas laws, were discovered, showing for example, how the volume of a gas increases with temperature. Yet no one knew the deeper reason for these particular laws or what lay behind them." It was not until the theory of atoms and molecules was developed that the basis for the well-established laws was known (Peat, 1990, pp. 72-73).

McNally cites Philip Kitcher's book, Abusing Science: The Case Against Creationism, which is a critical analysis of the basic premises of Creation Science, and which explains why so-called Creation Science is really a pseudo-science. In that book, the three categories of scientific claims from Kitcher, which Kitcher considered the claims of Creation Science in the light of, are claims about the theory that allegedly explains the how and why of a phenomenon whose existence is not questioned or doubted by either Creationists or evolutionists. That phenomenon, namely, is the fact that there exists an amazing diversity of complex species of life on earth. The three categories that McNally cites Kitcher as outlining (Kitcher, 1982, pp.168-169) are:

> Theories having considerable empirical support;
> Theories with less support;
> Theories with so little support, that they can be ignored.

McNally confuses phenomenon with theories. The three above mentioned categories are to be applied to the conflicting theories generated to explain the origin, the why and how, (question B) of this undisputed and obvious fact, (question A: that there are indeed an amazing diversity of species on earth). It's as if, (to use another example), finding what he thinks are weaknesses in both the particle theory and the wave theory of light, he concludes that the existence of light is therefore not an established matter of fact about the world!

Likewise, Callahan is merely demonstrating that a very significant phenomenon exists (that when and only when precise and appropriate algorithms or causally diagnosed TFT "tapping" is applied, clients are both relieved of specified psychological distress, and at the same time HRV improves, within mere minutes, to a degree completely unprecedented in medical and psychological literature). One can see that something is going on, here, while questioning the proposed theory offered to explain it. It would be more fair for McNally to say, simply, that Callahan has demonstrated the fact that significant relief from psychological problems, and at the same time, unprecedented improvement in HRV occurs when using TFT; but that McNally is not persuaded that "meridians" or "thought fields" actually exist, or, if they do, adequately explain how and why TFT works. Those "how and why" questions would be fascinating questions, to be explored in future articles. If we make a discovery in the future of something that better explains this phenomenon than the theory of meridians and thought fields, such a discovery would lead us to modify or replace our current theory.

I am belaboring my point, about the difference between question A-type questions, (matters of demonstrated fact), and question B-type questions, (theories or concepts proffered to explain the how and why of such demonstrated matters of fact). I have to belabor the point, in order to underscore McNally's (and many other TFT critics') errors in critical reasoning, and thusly "un-muddy" the waters previously muddied by McNally and others. Since Callahan was merely aiming to demonstrate that TFT is efficacious, the only pertinent question for the critics is: Did Callahan in fact demonstrate this? If there is a more parsimonious explanation, (placebo, therapist expectancy/bias, "regression to the mean", etc.), then Callahan would have failed to prove that TFT is efficacious. Throughout this article, I will explicitly and specifically respond to each such proposed, supposedly more parsimonious explanation offered by TFT's critics, and show that none of those alternate explanations can effectively explain the results.

How Callahan Developed His Theory: "The facts came first, and then the theory followed from the facts"

In a personal communication, Callahan commented on the critics' conflating of phenomenon with theories, and explains briefly in outline how he developed his theory, based on the observable facts:

"These articles did not contain my theory yet the critics ignore the robust facts presented in each article [Callahan, 2001a; 2001b; and 2001c; Pignotti and Steinberg, 2001; Sakai, et al., 2001; and Johnson, et al., 2001] and choose to attack what they call my 'theory'. This is a gross reasoning error in their approach, as Barger points out, and it allowed them to pretend that the robust facts presented in our articles were irrelevant. Here is a very brief response that actually does contain snippets of my developing theory of TFT.

> "Regarding my theory: Once an interested reader understands the profound facts cited in the six TFT articles in the October [2001] Journal of Clinical Psychology and in our other books and articles, it should be understood that these and other robust empirical facts provide the basis for the theory I have been developing over the last 22 years. Stop the Nightmares of Trauma, [Callahan and Callahan, 2000] by my wife, Joanne and me, shows why I maintain that our results more closely resemble hard rather than soft science (p. 59).
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> "The concept of 'Perturbation', also explained in this book, (pp. 135-166), is a fundamental concept in my developing theory. Anyone who replicates my work, I maintain, will be forced to come up with a similar or the exact same concept; the facts require it.
>
> "Theories in science are an attempt to integrate and bring diverse facts together and can often lead to new discoveries when the theory is grounded in reality, i.e., based on robust reproducible facts. New discoveries have continued to be revealed over the last two decades of doing TFT.
>
> "I did not begin with a theory but began developing it as the robust facts revealed themselves through my causal diagnostic procedures and my treatment.
>
> "I have no special attachment for my theory and like any good scientist, I stand ready to modify or overthrow my current theory, or aspects of my theory, when and if new facts

require it. Until then, it should be recognized by serious readers of my work that every nuance of my theory is thoroughly grounded in the highly reproducible and numerous unusual and new exciting facts revealed through my therapy. The facts came first, and then the theory followed from the facts.

"In Stop the Nightmares of Trauma, [Callahan and Callahan, 2000] pp. 163-166, e.g., it may be seen how new facts required me to modify my basic concept of Perturbation (which fit the facts exactly when dealing with psychological problems) into the more precise and general concept of Healing Data (hd), which helps explain results that go beyond what can reasonably be called psychological problems.

"A good example of how my theory can explain otherwise puzzling robust facts common in the field of psychotherapy is my explanation of why it is, (i.e., what purpose is served), by the fact that trauma victims often have repetitive nightmares and become obsessed with the horrible event; i.e., until TFT removes this problem in minutes (see chapter 16 in Stop the Nightmares of Trauma, [Callahan and Callahan, 2000])". (Callahan, 2002, personal communication).

Kline Repeats McNally's Error, Conflating "Phenomenon" with "Theories", plus Makes an Unwarranted A Priori Assumption

Kline makes the very same critical reasoning errors that McNally does. Kline writes, "Callahan's article [Callahan, 2001b] provides no evidence for the efficacy of TFT, nor evidence for the credibility of TFT's rationale…Callahan's article sheds no light on why he believes as he does. It represents a disjointed series of unsubstantiated assertions, ill-defined neologisms, and far-fetched case reports that blur boundaries between farce and expository prose. (Kline, 2001, p.1187)."

There's a lot to unpack there, in Kline's statement. When he writes that Callahan provides, "…no evidence for the credibility of TFT's rationale", notice that he too, like McNally, is conflating question A-type issues with question B-types. Remember, presenting the "evidence for the credibility of TFT's rationale", by which I presume Kline means, how and why TFT supposedly works, the theory behind it, was never Callahan's purpose in this article. Remember that supposedly debunking a theory which purports to explain a phenomenon can never make a demonstrated phenomenon untrue.

Unpacking Kline's statement still more, notice the pejorative of labeling Callahan's case reports as "far-fetched" and that they "blur the boundaries between farce and expository prose". To restate Kline's objection, he seems to find the reported results as literally unbelievable. That is to say, Kline, if I correctly understand him, is saying that he cannot believe that such previously unheard of results could have actually occurred.

Kline just refuses to accept that such results actually happened. This is a dogmatic position. His argument against Callahan is an a priori assumption that such dramatic results are impossible. A skeptical, rational, scientifically minded critic should instead focus on finding out for him or herself whether or not such claims are credible. All he has to do is just see if he can replicate the results. It is, properly, not a question of theory, here, but of demonstrability and replicability.

This a priori assumption that such results are impossible, ("far-fetched" and "farce" were the key words) reminds me once again of the a priori assumptions of certain Creation Science advocates.

Kline might be surprised to find that he's making the same kind of error that Creation Science advocates often make. The Young Earth Creationists will clutch at any straw to argue, for example, that there must be some very serious flaws in radiometric dating which shows the earth as billions of years old. Further, they flatly refuse to accept that light from distant stars traveled millions, even billions of years, to reach earth, since such acceptance would require them to let go of their cherished a priori belief that the Universe is a mere six thousand to ten thousand years old. They look at any demonstration that would show conclusively that the earth is at least billions of years old, not thousands, and walk away proclaiming to not be persuaded!

Likewise, Callahan has shown Heart Rate Variability being raised as a result of TFT, within mere minutes, to a degree unprecedented in the research literature. Yet some critics are not persuaded that anything of significance requiring a radical new theory of some kind has actually been demonstrated. To quote Callahan, "However, the changes in SDNN found after TFT treatment are unprecedented in the current literature…The author, [Callahan] has not been able to find any studies or even a single case that showed the degree of change documented here with TFT. The changes were brought about by treatments that took only minutes to carry out". (Callahan, 2001a, p.1165) Common sense would rule out placebo, therapist expectancy, demand characteristics of the treatments, etc., as major factors in the results, because if any of those factors, or all of those factors combined had the ability to improve HRV even half as much as what Callahan reported, you would find an occasional report of such improvements, somewhere in the literature on HRV; at least a few cases, here and there. Kline cites no research that supports his contention.

Case Studies vs. Controlled Studies: Demonstrating "Raw Facts"

Focusing on case studies rather than controlled studies is perfectly reasonable, in this context. Why? Because to find even one single case that demonstrates that something as totally unexpected as this is occurring is notable. After all, Galileo didn't need a second telescope, to control for "observer bias". When Darwin pointed to Archaeopteryx, a fossil that showed features of a dinosaur, but with feathers, thus demonstrating a transitional form between dinosaurs and birds, Darwin didn't need to look at a "placebo" fossil! As a transitional form, Archaeopteryx is solid evidence of the phenomenon, the "raw fact" that evolution has indeed occurred. Just like Galileo pointing his telescope to the sky and inviting skeptics to "just look"; like Darwin pointing to the fascinating features of archaeopteryx and saying, to skeptics, in effect, "just look;" so Callahan demonstrates HRV improvements within minutes, after a TFT treatment, unprecedented in the research literature, and says, in effect, "Just look!"

All three examples are the answers to what I referred to earlier as Type A questions. Galileo may indeed have developed quite inadequate theories to explain the how and why of astronomical phenomenon; and many of Darwin's theories, or speculations about the why and how of how evolutionary history actually occurred, have been rejected by subsequent generations of scientists. No such objections to their theories, though, can ever lead any rational person to dismiss the basic "raw facts" of their basic discoveries.

If Kline wishes to speculate that a different system other than a theoretical energy meridian system is the conduit through which TFT's procedures actually work, a separate discussion on that topic may be indicated. No objection to concepts such as "meridians" or "energy" can negate the demonstration that HRV measurements improve within mere minutes to a degree unprecedented in the literature, in clients after receiving a TFT treatment. Such improvement occurred not just in one individual, but in twenty cases reported by Callahan.

The Relationship Between SUD and HRV

Let's look at some more of Kline's errors. On page 1189, Kline writes, "Callahan suggests that HRV can provide better information about therapeutic progress than SUD (subjective units of distress)". That is not Callahan's claim at all. Actually, what Callahan said, (referring to a recent study by Dishman, et al., 2000), was, "A statistically significant relationship between self-rated perceived emotional stress and HRV was found; the more self-rated stress a person was under, the lower their HRV. This statistically significant relationship between self rated anxiety/emotional stress and HRV was found to exist independently of age, gender, trait anxiety, cardiorespiratory fitness, heart rate, blood pressure, and respiration rate. This study refutes the notion that a positive change in HRV after a successful psychological intervention is due solely to slowed respiration rate, as these differences were found to exist independently of this factor (p.1158)."

In his discussion in his first article (Callahan, 2001a, p1165), Callahan concludes, "However, these changes, [i.e., the dramatic improvements in HRV] do show that something clinically significant has occurred and that TFT is worthy of further study and research in both psychology and cardiology". Callahan is not formally offering any new theory to explain how and why TFT is so efficacious. He's reporting a replicable phenomenon that cannot be easily explained away by counter-explanations, (placebo, expectancy bias, demand characteristics, etc.).

Another recent study, which shows HRV being used as an outcome measure of a psychotherapy intervention, is Carney, Freedland, Stein, Skala, Hoffman, and Jaffe, (2000). In that study, depressed patients with coronary heart disease were treated with Cognitive Behavioral Therapy (CBT). The SDNN of the patients on average got 4% worse, after 16 sessions of CBT.

First, this study illustrates that Callahan and his colleagues are not alone in using HRV as an outcome measurement of a psychotherapy intervention. Second, when Kline names "regression to the mean" (p1189) as a supposedly plausible counter-explanation for Callahan's results, we do not appear to see "regression to the mean" in the Carney study. The SDNN scores in the Carney study ended up with 4% decline after Cognitive Behavioral Therapy. As Callahan suggested (p1257, Callahan 2001c); expecting low HRV to "regress to the mean" is like expecting the gas tank in your car, if it's near empty, to spontaneously "regress" to the "mean" of being half full, all on its own!

A False Analogy: EEG and "mind reading"

Let's look at yet another error of Kline's. Kline points out that to be "objective", HRV measurements must be properly measured, properly quantified, and properly interpreted. That's fine, so far; but Kline further writes, "Simply stating that the measures are objective and scientific because they are physiological or medical is not enough. This is like saying that we can use brain waves to communicate telepathically simply because we can measure electroencephalograms (EEG's) and they seem sort of like radio signals" (Kline, 2001, p. 1189).

Kline's analogy is false. If you insist on an analogy to using EEG as an objective measurement of a therapy, the analogy would be: If a hypothetical pre-treatment EEG measurement was abnormal and very worrisome to a doctor, and yet post TFT treatment EEG's, taken just a few minutes after the pre-treatment EEG's, indicate normal, healthy EEG, this would lead to the conclusion that some significant intervention has occurred, (if EEG improvements were consistently observed).

The analogy breaks down because EEG's are very unlike HRV in at least one crucial respect: While people have been shown again and again to directly change EEG quite significantly within short periods of time through biofeedback, relaxation, etc., HRV measurements are quite stable over time. Prior to Callahan's research, HRV has not been shown to fluctuate significantly from moment to moment, day to day, or even week to week. HRV has not been shown to be easily and radically manipulated or influenced by the subject's current moods or behaviors or beliefs.

Kline continues, "The ability to measure EEG's is not tantamount to the ability to read minds. Neither is the ability to measure HRV tantamount to a scientific validation of treatment efficacy". No, "the ability to measure" is not the claimed validation-anyone with the right equipment and knowledge has the mere "ability to measure" EEG or HRV: But the consistent improvements in HRV are tantamount to scientific validation. Just as, not the mere ability to measure EEG, but rather, consistent improvements in EEG would indeed be solid evidence in favor of a therapy's efficacy, under certain conditions. (Notice, I said, "solid evidence," not necessarily conclusive proof.) Kline's argument from analogy against Callahan's claims is sloppy and imprecise, and makes me wonder if he really understands what it is exactly that Callahan is actually claiming. When you correct his sloppy analogies, you see that positing that Callahan is arguing that the "mere ability to measure HRV is tantamount to scientific validation of treatment efficacy" is just a straw man argument against Callahan.

Another Straw Man Argument From Kline, plus the Five Possible Psychophysiological Relationships

On page 1190, Kline writes, "If SUD correspond closely to HRV changes as he claims, then the incremental validity of HRV changes for indexing therapeutic progress is questionable, and he makes no effort to assess this". This is a straw man. Callahan has never argued that SUD [subjective units of distress] ratings and HRV changes closely correspond in a one-to-one type relationship. If Callahan really were arguing that there was a one-to-one correspondence between SUD ratings and HRV, then any other intervention that caused calm and relaxation, even temporarily, such as guided imagery, meditation, or even a good night's restful sleep, if it causes a reduction in SUD ratings, would also cause a corresponding improvement in HRV. Kline has not read the literature carefully. Even when people report feeling better after weeks or even months of more conventional psychotherapy interventions, no such significant improvements in HRV have been reported in the literature. Some improvement in HRV is reported, but nothing that even compares to the HRV improvement within minutes, reported by Callahan, after TFT treatments.

Kline introduces the five types of possible relationships between physiological and psychological phenomenon, as spelled out by Cacioppo and Tassinary in their 1990 book, Principles of Psychophysiology. The 2000 edition of the book is re-titled Handbook of Psychophysiology, (Cacioppo, Tassinary, and Berntson, 2000), and pp.16-21 describe four possible relationships, (the fifth type referred to by Kline would be the null set-no relationship). Those four possible relationships are: outcome, marker, concomitant, and invariant. Invariant would be the strongest possible relationship between the physiological and the psychological phenomenon, mere outcome the weakest. Kline then sets up a straw man.

Kline writes, "Callahan assumes that HRV stands in both one-to-one and context free relations to better health. According to Callahan, increased HRV is sine qua non of better mental and physical health. This is a questionable assumption to say the least…" (Kline, 2001, p. 1190). This is the straw man: Callahan does not assume that "increased" (improved) HRV "is sine qua non of better mental and physical health." Kline is representing Callahan as arguing that the relationship between inappropriately low (or high) HRV and psychological problems is a one-to-one, context free (i.e., invariant) relationship. This would of course imply that everyone with normal HRV also has no significant psychological problems, and that everyone with significant psychological problems also has inappropriately low (or high) HRV.

Kline says, rightly, "the null relation is, of course, our beginning assumption. It is incumbent upon investigators to demonstrate the other types". Cohen, Matar, Kaplan, and Kolter, (1999) conclude that low HRV is, at least, a marker of psychiatric disorders. (See my remarks below in response to Herbert and Gaudiano's argument based on Cohen, et al., [1999]. I go into Cohen, et al.'s conclusion that HRV is a marker of psychiatric disorders, in greater detail, in my remarks below about Herbert and Gaudiano's JCLP article).
Callahan's actual argument is much more modest than what Kline represents it to be. We know that HRV measurements outside of a certain range are a strong predictor of mortality. We also know that there are a multitude of factors that seem to affect HRV, and that there are quite likely other factors that we don't even suspect yet, which also affect HRV. Regardless of what those other factors are, so far only TFT has been shown to cause hazardous HRV measurements to improve within minutes to well within that "normal" range. Kline would do well to analyze Callahan's actual claims, and not "read into" Callahan's reports more than is actually there. Callahan is not saying that there is an invariant, one-to-one relationship, regardless of context, between HRV and good mental health.

The critics of TFT are dismissive of clinical results, (i.e., SUD ratings improving, even in the "toughest" most intractable cases, and even in skeptical clients), as if clients' consistent reports of reducing and eliminating distress "don't count". Kline also rejects a physiological measurement, HRV, as evidence of TFT's efficacy, even though Kline cannot offer a plausible more parsimonious explanation for HRV improvements, within such short timeframes (mere minutes), consistently, of 50%, 75%, 100%, 150%, and even more. Kline might do well to try to find even one single report of same-day, same-week, or even same-year improvements in HRV (pre-treatment vs. post-treatment measurements), other than Callahan's, of even close to the kind of improvements reported by Callahan. You may not understand the "how and why": exactly how TFT works and exactly why people are reducing psychological problems, just by "tapping". TFT's strengths are in the results it produces. Callahan's results call for replication.

Relaxation, Power of Suggestion, "Attentional Demands", Changes In Respiration Rate, Etc., Cannot Adequately Explain The Results

Kline writes, "Callahan neglects to mention the multiple determinants of HRV" (P.1190). This would be a relevant objection, only if the HRV improvements reported by Callahan were slight, marginal improvements. Kline also writes, "…making it possible that any changes observed are due simply to gross movement artifact (i.e., maybe people are more likely to keep still while they are being tapped (p.1190)." Kline is wrong. No one's HRV was being measured while they were being treated with TFT.

Kline and other critics would do well to try a simple experiment. Try to even temporarily manipulate an unhealthy, unusually low HRV, and make it jump up, (even temporarily) doing anything other than TFT. If they cannot intentionally manipulate a same-day significant increase in HRV, using relaxation, meditation, hypnosis, "placebo" treatments, power of suggestion (telling a subject to expect HRV improvement while applying ANY non-TFT treatment), etc., then we can safely assume that any improvement in HRV beyond 10%, (let's stipulate for sake of argument), is due to TFT. Since the average HRV improvement in the case studies was over 150%, Kline's argument at best would be that perhaps 10% of the HRV average improvement was due to "other" factors, and "only" 140+%, then, was due to TFT.

When one finds any way to consistently reproduce the raising of low HRV within a thirty minute time frame by any ethical means, in the context of an experiment, by as much as 75%, (half of the average improvements in the twenty cases presented by Callahan), then maybe we could consider that another factor than TFT may be a plausible alternate or co-factor responsible for these results reported by Callahan.

On pp. 1190-1191, Kline writes, "Among its many correlates, HRV has been related to frequency of sexual intercourse, but not masturbation, in cohabiting couples (Brody, Veit, & Rau, 2000)." The Brody, et al., study does not show any cause and effect relationship between HRV and intercourse between co-habiting couples. The Brody, et al., study never assessed a "before" and "after" HRV measurement. That study never looked into whether intercourse, (or any other intervention) could raise or lower HRV; each subject in that study had his or her HRV measured once, using the five-minute HRV test, (blood pressure and Valsalva ratio were also measured). After these measurements, the subjects were asked to fill out questionnaires, reporting various health habits, as well as sexual attitudes and habits, then were dismissed. There was a reported statistically significant relationship between self-reported frequency of intercourse in cohabiting couples, and HRV, although the authors suggest that since the sample of subjects surveyed was small, their results should be considered preliminary, not conclusive.

The study did not show, indeed was not even examining, the key question that would be relevant in the current context: Whether or not intercourse with a cohabiting partner, (or any other intervention), could raise low HRV, (or lower HRV that was too high). Only if some other intervention than TFT is shown to improve HRV, and not only improve HRV, but improve it to the degree that TFT has been shown to, and to do so within mere minutes, will that intervention be a plausible, more parsimonious counter-explanation for Callahan's results. Since the Brody, et al., study was not a therapy outcome study, the Brody, et al., study is not even relevant to the point that Kline is making about TFT and HRV.

Kline also writes, "Although HRV is interesting within the context of treatment outcome assessment, it is erroneous to hinge evaluation of treatment efficacy solely on HRV measurement. (p. 1190)." Kline is mistaken. Callahan in fact uses SUD ratings as the primary way to evaluate treatment efficacy.

On page 1191, Kline writes, "RSA [respiratory sinus arrhythmia] is greatest when respiration is slowest, which provides a more parsimonious explanation of anxiety and panic effects than does a blocked meridian or an individual energy toxin. Unfortunately, there is no evidence that Callahan controlled for respiration rate." Further, Kline continues, "It is certainly plausible to suggest that the attentional demands of being tapped could lead to alterations in respiration rate, which in turn could lead to alterations in HRV." Kline seems to forget that NO intervention has previously ever been shown to increase HRV within minutes by even as much as 10%. When and only when Kline or other critics can show cases where even 75% improvement in HRV can be achieved by relaxation, slower breathing, guided imagery, "attentional demands", etc., then and only then will they have presented what might be a plausible more parsimonious explanation for the almost immediate HRV improvements after receiving a TFT treatment.

Phenomenon Vs. Explanation: HRV and Individual Energy Toxins

On page 1191, Kline also writes about "Individual energy toxins and chemical sensitivities: Beyond SUD and Spuds". Kline does not grasp the concept "individual energy toxin", and such a phrase will not make sense until the term "energy system" is grasped. It is beyond the scope of the current discussion to delve into exactly how the concept of "individual energy toxin" can be validated by any inquiring critic but the facts as presented in the JCLP articles by Callahan and his colleagues, e.g., the impact on HRV, are easily replicated.

Kline makes a critical reasoning error of assuming that raising objections to the explanation of a demonstrated phenomenon, (i.e., to how and why we get such an easily replicated result), is tantamount to showing that there is no observable, easy to replicate phenomenon of some kind, at all. I need not be an expert on modern physics or astronomy to assert that moons orbiting the planet Jupiter do in fact exist. I do not need to have a clear and convincing causal explanation for why and how the planet Saturn has rings around it, to know for certain the simple phenomenon that Saturn has rings. Regarding Callahan's articles, he is merely demonstrating the observable fact that you get these results, which cannot be easily otherwise explained away. The concept, "individual energy toxin" is one explanation of one thing that TFT seems to be correcting, in some cases. We have reasons for why such theories as energy meridians and individual energy toxins are the most parsimonious explanatory concepts, to explain the how and why questions raised by these results. The validation of such theories would be a quite proper subject for a future series of articles. The current articles by Callahan and his colleagues merely demonstrate that a very significant phenomenon is occurring when TFT treatments are given.

Somatization is not an adequate or plausible explanation in the case presented by Callahan, (Callahan, 2001a, p. 1178), for the woman's reactions to wheat and Irish breakfast tea, (Kline, 2001; p. 1191). No study has ever shown that HRV can drop by such a degree within minutes just by merely believing that such items will make her anxious or depressed. HRV is generally a very stable phenomenon. No study has ever shown that suggestibility has ever caused any massive rises or drops in HRV, within minutes.

Kline's Objection: SDNN is "frequently non-specific"

Kline writes, on page 1190, "Since SDNN is frequently non-specific, we do not know whether any changes, if indeed legitimate changes had been documented, would have been mediated by sympathetic and/or parasympathetic nervous systems…However, it is an important methodological consideration for any study purporting to assess HRV."

First, "the SDNN is known as a very global index of HRV…" (Coumel, Maison-Blanche, and Catuli, 1995, p.211). Second, Callahan has responded to Kline's objection in his article, "Thought Field Therapy: Response To Our Critics and a Scrutiny of Some Old Ideas of Social Science", (Callahan, 2001c). On page 1254 of that article, Callahan describes a personal meeting with Professor Marek Malik, an authority on HRV.

Callahan writes, "An HRV report yields many interesting indices; we chose to present the SDNN scores for this is the measure of variability itself and SDNN is the index used to predict death and vulnerability to problems (Callahan [2001] a and b, this issue). SDNN is the most stable score in HRV and the most resistant to change. After my HRV papers were submitted, I had a personal meeting (Malik, 2000) with an authority on HRV, Professor Marek Malik (1998, and Malik and Camm [1995]) in London. A year earlier, I had sent him a sample of five cases where we had dramatic increases in SDNN. Malik took note of our unusually large increases in SDNN a year later (after our personal meeting), and he checked to make certain that SDNN was not increased at the expense of increasing the sympathetic nervous system. After examining the records of this sample, Malik concluded: '[The large increases in SDNN]…were not likely due to increased sympathetic activity (Malik, 2000).' As profound and unprecedented improvements in SDNN become more common it will be important to ensure that the increase of SDNN not be at 'the expense of increasing the sympathetic nervous system.' …. Because TFT rapidly eliminates general stress as well as varied psychological problems it is not surprising that we found the autonomic nervous system is typically put into better balance after successful TFT treatment." (Callahan, 2001c, page 1254).

"Proof" Vs. "Persuasion"

Kline states, "In sum, the evidence that Callahan has offered in support of his far-fetched theory and practices is not convincing." Callahan offered no formal theory in his articles, he merely demonstrates a phenomenon, that when TFT treatments are given, HRV improves by an average (among cases outlined) of 150%+. That these demonstrations are not convincing to Kline that something of significance is going on, here, does not mean that Callahan hasn't proven TFT's efficacy, in these articles. There is a big difference between "proof" and "persuasion". Just because someone is not persuaded that a conclusion is true does NOT mean that the conclusion hasn't been proven.

To quote philosopher Dr. Antony Flew, from his book, Thinking About Social Thinking, (second edition, 1995, p. 247): "…But the fact that someone, and that someone an otherwise reasonable and relevantly well-equipped person, refuses to be persuaded by some material is not a sufficient reason for concluding that that material does not constitute a valid proof. It is, therefore, always wrong to argue direct from the fact that people are not persuaded to the conclusion that no valid proof has been presented to them; and doubly wrong to speak neither of proving something nor of persuading someone but of proving or failing to prove something to someone. What you should say is that you have failed to persuade them, perhaps through no fault either of yours or in your proof." (Flew, 1995).

Since Callahan's purpose was only to use HRV and SUD's to demonstrate that TFT is efficacious, regardless of the alleged mechanisms by which it works, the question is: Has he in fact demonstrated this? You can no more "debunk" the fact, (demonstrated by Callahan's case studies) that TFT is efficacious, by merely expressing skepticism about "energy" and "Qi" and "meridians", than a Creation Science advocate can "debunk" evolution per se, by raising skepticism about punctuated equilibrium, as a good theory to explain why and how evolution actually occurred. If the punctuated equilibrium model is discarded as an inadequate explanation, the simple phenomenon, that evolution has occurred, (by whatever known or unknown mechanisms) is still a demonstrated fact. By the same token, if a mechanism other than changes in "energy" in "meridians" is a superior model to explain the results that even skeptical inquirers can replicate, TFT's efficacy is still a fact. It remains a proven and easily demonstrated fact, even if, echoing certain Creationists, some skeptics proclaim, "I'm not persuaded".

TFT's Demonstrated Efficacy is Circumstantial Evidence For the Existence of A Meridian Energy System

Now, no known and widely accepted theory explains how and why TFT works. TFT's efficacy is now an established phenomenon, whether that fact is "accepted" by critics, or not. When a newly discovered fact about the natural world cannot be fitted into current, conventional models, (i.e., theories), it is the models, the conventional theories, that must be scrapped or modified. Solid reason to give credence to the "energy meridian" theory as the most parsimonious explanation is this:

Any "inquiring skeptic", carefully following every step and keeping in mind all caveats, can get the same results with TFT algorithms that non-skeptics can, on either skeptical or non-skeptical test subjects.

These results are only achieved by tapping on precise points on the body, (in a precise order), and are not achieved when tapping on any points other than those precise points.

These precise points correspond exactly to beginning and/or end points of acupuncture "meridians", identified thousands of years ago, long before Callahan developed TFT.

Mere chance is not a reasonable, plausible explanation for why it is that the beginning and/or end points of these ancient acupuncture "meridians", and only these points, are precisely the points that, when tapped in a precise order, (even in people who know nothing about meridians) cause such a response. Therefore:

The efficacy of TFT is at least circumstantial evidence for the existence of an actual meridian system, regardless of our ignorance of why and how exactly it works, etc. Keep in mind that evidence of the existence of the meridian system is not tantamount to evidence that every claim about this meridian system is true. It is a phenomenon of nature, and needs further scientific study.

Kline and others get confused, it seems, when Callahan refers to the meridian system, "Qi", and other unconventional concepts, assuming that Callahan is implicitly accepting the mystical, unscientific belief-systems of New-Agers, or whatever, when he uses these terms. He is not. To use an analogy: To argue that believing in the existence of a body bio-electric "subtle energy" system which corresponds to the ancient acupuncture meridians is tantamount to mysticism or pseudo-science, is like arguing that believing in the existence of other planets in our solar system is tantamount to believing in astrology! Belief in "Qi" and "energy meridians" is not essential to accept that the basic claims of TFT are in fact true.

Why Dramatic Claims are Easier to Prove (Or Refute!) Than More Modest Claims

Kline flatly claims that case studies are not sufficient to validate efficacy claims. Once again, he is mistaken about methodology. The reason that the claims of TFT can rationally said to be proven, (i.e., that the phenomenon exists; which, remember, is a separate question from how and why it works) by case studies, and why, rationally, these case studies are sufficient proof of efficacy, is because of the very nature of the claims for TFT. The principle is: The more simple and straightforwardly dramatic the claim, the less you need to rely on statistical analysis, control groups, etc.

If I claimed, hypothetically, that I could merely reduce your intense fear of heights by 20%, (measured by self-reported SUD ratings), over a period of, say, five months of therapy, I would want a control group, and a researcher conducting the experiment double blind, if possible, with a fake "placebo" therapy. It's easy to imagine that a marginal reduction in your distress would be explained by any number of non-specific factors. However, if I claimed that I could cure your intense height phobia completely, for life, within five to ten minutes, when no other therapy has worked, why would I need anything other than a series of similar case studies, (assuming that, like Callahan, I get the same results, again and again, on even the "toughest" cases), to tell me that my treatment is efficacious? That is not a rhetorical question. That is a philosophical question that I would like to hear Callahan's critics give a thorough and thoughtful response to.

Consider this statement from Julian L. Simon: "In much-even most-research in social and physical sciences, statistical testing is not necessary. This is because where there are big differences between different sorts of circumstances-for example, if one medicine cures 90 patients out of 100 and the other medicine cures only 10 patients out of 100-then we do not need refined statistical tests to tell us whether or not there really is a difference. And the best research is that which shows big differences, because it is the big differences that really matter. If the researcher finds that she/he must use refined statistical tests to reveal whether there are differences, the differences do not matter much." (Simon, 1992, p. 19).

An Analogy: A Hypothetical "Healing the Blind" Treatment; The Difference Between "Miracle Claims" and Scientific Claims

Let's draw an analogy. Suppose that I claimed that I could give perfect 20/20 vision to any person blind since birth, within mere minutes, just by touching their eyes in a certain precise way, with my bare hands. This would indeed be an extraordinary claim! Indeed, if I said that only I (or my followers) could give the blind perfect 20/20 vision like that, that would fall under the category of "miracle-claim". If, however, instead of claiming to be a miracle worker and demanding "faith", before such healing is possible, I actually published a precise set of "algorithms" which even militantly skeptical "Pharisees" could follow, replicate, and get similar results, my claim would then no longer be a "miracle claim" at all. It is now a scientific claim: My claim is easily falsifiable, and my algorithms are easily replicable by any "inquiring Pharisee". (I.e., a "Pharisee" might conceivably follow my precise algorithm and not get the results I claim, or get similar results with a fake "placebo" algorithm, thus falsifying my claim).

Dramatic results from my hypothetical healing-the-blind algorithm would not need a control group at all, let alone a statistical analysis, to "validate" my claims. If, again and again, consistently, and following an algorithm that any "Pharisee" could easily duplicate, congenitally blind people, within mere minutes, are given perfect 20/20 vision without surgery or drugs, the simple fact is that these results would be so dramatic that you would not need double-blind studies, (pardon the pun) to know that something quite extraordinary has occurred, and that the treatment is efficacious. If my hypothetical claims were more modest, predicting only marginal improvements and only in people with slight vision problems, then a much more rigorous methodology would be required, to confirm the more modest claims.

The more modest the claim, the harder it is to verify. If I claimed that my hypothetical treatment improved eyesight in people with modest vision problems by an average of 5% over a two month period, that would be harder to verify, because it would be easier to imagine that other, non-specific factors could reasonably account for such merely marginal, merely "statistically significant" improvements. Thus, I would want to control for the possibility that such slight improvements could be caused by: relaxation/stress reduction, suggestibility, dietary changes, or any other possible alternate factor. If on the other hand, an openly skeptical "Pharisee" could easily follow my healing-the-blind algorithm, on even congenitally blind "Pharisees", it would be quite plain from the case studies alone whether or not the subjects within mere minutes achieved 20/20 vision, thus verifying the remarkable claim!

Callahan's claims are definitely of the more dramatic type. Thus, it's rather easy to see that when you find even one case where, within minutes, HRV is improved by an amount unprecedented in all medical and psychological literature, something quite significant, requiring some kind of new explanation, is going on. Instead of one isolated case, Callahan presented twenty. He has thusly proven what he claimed in the conclusion to "The Impact of Thought Field Therapy on Heart Rate Variability ", (Callahan, 2001a), namely, "…The changes in SDNN found after TFT treatments are unprecedented in the current literature". (P. 1165) And, "The author [Callahan] has not been able to find any studies or even a single case that showed the degree of change documented here with TFT. The changes were brought about by treatments that took only minutes to carry out". (P.1165).

Hume's Maxim Only Applies To Claims of Unrepeatable Phenomenon, Not Easily Replicable Phenomenon, Such As TFT's Results

Kline invokes the philosopher David Hume. Kline writes, "The onus is on him [Callahan] to show evidence of a 'high cure rate' rather than simply reiterating his unsubstantiated assertion. Consider Hume's maxim: 'That no testimony is sufficient to establish a miracle unless the testimony be of such a kind that the falsehood would be more miraculous than the fact which it endeavors to establish'. In short, incredible claims require incredible evidence". Kline is muddying the waters, because Hume's famous maxim is not even relevant here. Hume's famous argument against miracles was precisely that miracles were unrepeatable, and thus the only possible evidence for a miracle was other people's testimony.

It was not against "mere" extraordinary claims that Hume was speaking, in that quote; it was against un-testable claims of unrepeatable experiences that are claims to be violations of natural law, (in other words, miracle-claims!) In sharp contrast, TFT is replicable by any inquiring skeptic. Even Kline himself could get similar results with TFT, if he cared to simply replicate TFT's simple algorithms, on any appropriate test subject of Kline's own choosing. Thus, the Hume quote is completely irrelevant, here: If Hume were alive today, he might argue that since the results can be easily replicated even by skeptics, the truth of the claims for TFT are not based on "mere" testimony, as the alleged miracles in the Bible for example clearly are. Callahan is NOT making any "miracle-claims".

Herbert and Gaudiano's Article Uses An Ad Hominem Argument Against Pignotti and Steinberg

In the article by James Herbert and Brandon Gaudiano, "The Search for the Holy Grail", (Herbert and Gaudiano, 2001), on page 1208, Herbert and Gaudiano's complaint about Pignotti and Steinberg's (Pignotti and Steinberg, 2001) use of HRV is that: "Without exception, these articles and internet postings are not written by psychophysiologists whose primary interest is in HRV as an index of psychopathology or as a measure of treatment outcome. Instead, these reports are written by promoters of TFT, most of whom do not have specific expertise in medicine or psychophysiology, and all of whom view HRV as a unique tool for demonstrating the powerful effects of this therapy. This fact lends support to the hypothesis that Pignotti and Steinberg's primary interest is in the promotion of TFT, rather than the evaluation of HRV per se."

Herbert and Gaudiano want to argue that because Pignotti and Steinberg are "promoters of TFT", and "do not have specific expertise in medicine or psychophysiology ", that Pignotti and Steinberg, presumably, must be nothing more than pro-TFT "spin-doctors", instead of earnest researchers. The basic facts about HRV are not that difficult for an intelligent layperson to comprehend, in broad outline. If such great expertise were essential, perhaps Herbert and Gaudiano should either: A) Explain their specific expertise in medicine and/or psychophysiology, and how they are thusly able to be relied on to correctly interpret the data that Pignotti and Steinberg supposedly are not expert enough to correctly interpret, or B) Have reviews made by people with the requisite training and expertise. If such level of expertise is needed, to properly interpret HRV results, then trained experts, not Herbert and Gaudiano, should be doing the criticizing. If such expertise is NOT required, then Herbert and Gaudiano's criticism is not valid, and is a mere ad hominem.

The "Peer Review" Objection, Answered; and, Yet Again, Objections About the "Implausibility of the Theory"

Earlier, on page 1208, Herbert and Gaudiano write, "Three independent reviews of TFT have pointed out the absence of empirical research published in peer-reviewed journals on the technique, and have discussed the implausibility of the theory behind the treatment". One of those three "independent" reviews listed is their own article, "Can We Tap Away Our Problems?", which was published in 2000 in Skeptical Inquirer magazine, (Gaudiano and Herbert, 2000).

That particular "independent review" is nothing of the kind. I have earlier written a detailed, five page rebuttal to that specific article. That particular article badly misrepresents and distorts many of Callahan's actual claims about TFT, then uses those gross misrepresentations as straw men, then proceeds to blow down those straw men. Gaudiano and Herbert's 2000 Skeptical Inquirer article, after distorting what Callahan's actual claims are, proceeds with many errors in critical reasoning. Please don't take my word for it: First, read their article, then read my five page rebuttal/letter, available at Callahan's web site at: www.tftrx.com, under "reference material", and titled, "An Answer to the Skeptics: A Response to 'Can We Tap Away Our Problems? A Critical Analysis of Thought Field Therapy'", (Barger, 2001).

In Herbert and Gaudiano's JCLP article, notice that the gist of the complaint about claims for TFT are not that the claims aren't actually true, but merely, "absence of empirical research published in peer-reviewed journals" and "the implausibility of the theory behind the treatment". First, to establish that a significant phenomenon of some kind is occurring, which will require some kind of explanation, it is sufficient merely to show that it is easily replicable by anyone, even an "inquiring skeptic", and to eliminate all the obvious counter-explanations that on the surface would seem more parsimonious. My point is simple: IF such a significant phenomenon is actually occurring when TFT treatments are administered, this is a fact that is no less a fact, if peer-reviewed journals don't publish studies about it! I do not wish to imply anything one way or the other about why peer-reviewed journals other than JCLP are not publishing articles about TFT. New techniques, which are later accepted as incontrovertible fact very often meet either resistance and/or even indifference, before later being accepted.

While it would be invalid to argue from analogy that, just like Galileo's claims, these claims are valid, because brilliant innovative ideas often have been initially attacked; it is at least as invalid to argue that because a new, allegedly innovative therapy or idea is attacked and/or otherwise rejected or ignored by so-called "mainstream" scientists, that therefore the claims are false and/or not credible. Yet, this is the true gist of Herbert and Gaudiano's complaint about lack of published articles in peer-reviewed journals.

[To clarify: When Callahan draws an analogy between himself and Galileo, he's emphasizing a very different aspect of Galileo's relationship to the skeptics. It's not merely that Galileo's claims were very controversial, yet were actually true, but that Galileo's basic factual claims were easily verifiable by any of those skeptics, at any time, (by looking through his telescope, for themselves). Likewise, any modern critic of TFT can easily "look through the telescope" (so to speak), by just trying the simple, quick TFT algorithms for themselves, and seeing for themselves whether or not they get similar results.]

HRV Improvements Significantly Greater Than Ever Previously Reported, In the Research Literature

On page 1209, Herbert and Gaudiano write, "…case descriptions suggest that most patients reported an improvement in symptoms, a decrease in subjective distress, and a normalization of HRV immediately following treatment." Very often, there wasn't a mere "normalization of HRV immediately following treatment", there was (often) an improvement greater by far than any HRV improvement reported to date in medical or psychological literature for any other intervention. The point that Callahan made, that HRV does not respond to placebo still remains unrefuted, so placebo, suggestibility, etc., can be eliminated as plausible counter-explanations of these improvements. (See the sections below: "Why Placebo Is Not A Plausible Explanation", "Is The Autonomic Nervous System Easily Susceptible To The Placebo Effect?", and especially, "Kleiger, et al., (1991) Shows That HRV Is Not Influenced At All By Placebo").

The Five-Minute HRV Test Vs. The 24-Hour Test

Herbert and Gaudiano claim, on page 1210, that important differences exist between the five-minute HRV test and the 24-hour test. It's just not good enough for Herbert and Gaudiano to criticize Pignotti and Steinberg's choice of the five-minute test verses the 24-hour test, without stating what those "important differences" are.

Callahan spelled out the differences in measuring HRV between using the five-minute test vs. the 24-hour test, in his first article, "The Impact of Thought Field Therapy on Heart Rate Variability

(HRV)", (Callahan, 2001a), and explained why the short term test is in fact preferable to the 24-hour test. Callahan wrote: "Two types of tests for HRV exists: the long-term 24 hour Holter monitor test and the short term HRV test which can be 2-15 minutes in length. (Bigger, Fleiss, Rolnitzky, Steinman 1993). Short-term measurements of HRV have the advantage that they can be done over very short periods of time in which both the physiological and the psychological state of the individual being monitored is constant (Kautzner & Hnatkova, 1995). This is opposed to the 24 hour Holter Test in which the daily activities are generally unknown. It has been observed that: '…indexes of HR variability calculated during a 24 hour period include not only HR rhythms caused by respiration, blood pressure control, and thermoregulation, but also slower diurnal rhythms. HR variability determined from short electrocardiographic recordings under standard conditions may therefore be a better predictor of sympathovagal balance, and hence, of the risk of sudden cardiac death compared with 24-hour recordings.' (Kawachi, Sparrow, Vokonas & Weiss, 1995, p. 884)". (Callahan, 2001a, pp. 1155-1156).

Is the Data Really "Un-Interpretable", Or are Herbert and Gaudiano Straining to Avoid the Obvious, Common-Sense Conclusion?

Let's consider Herbert and Gaudiano's allegations of "methodological weaknesses." The results and conclusions drawn by Pignotti and Steinberg can only be refuted by showing at least one single, more parsimonious explanation that is even plausible, to raise reasonable doubt. Yet Herbert and Gaudiano look at those results and claim that Pignotti and Steinberg's study, "…is so devoid of methodological rigor that the resulting data are simply un-interpretable". Yes, and maybe Galileo just painted some spots on the lenses of his telescope, making the data gathered by just looking through the telescope "un-interpretable"! Is the data really "un-interpretable", or do Herbert and Gaudiano just wish to avoid the obvious, most common sense conclusion, that indeed something quite significant is in fact being demonstrated?

Alternate Explanations, Such as Statistical Regression, Mere Passage of Time, Etc., Rejected as Not Plausible

Look at Herbert and Gaudiano's list of possible alternate explanations: statistical regression, mere passage of time, non-standardized methods, placebo effects, and experimenter demand characteristics. Now, let's critically examine this list of what may seem on the surface to be reasonable, possible, plausible, more parsimonious explanations. The burden of proof is of course on Callahan and his colleagues to prove their claims. To clarify, the basic claim is that, using HRV measurements, (and SUD ratings), HRV measurements taken immediately after a TFT treatment session show such an unexpected improvement over HRV measurements taken immediately before a TFT treatment session, that the only reasonable conclusion is that the TFT treatment was the active ingredient, the cause of such improvement. Furthermore, since that degree of improvement is achieved, again and again, after TFT treatments, and never in all medical or psychological literature to such a degree for any other known intervention, the efficacy of TFT is thus demonstrated.

Mere statistical regression to the mean has never, in medical literature, shown HRV improvements of 150%+, so we can therefore rule out statistical regression to the mean as a plausible counter-explanation. "Mere passage of time"(the amount of time we're talking about here is minutes), has never, in all medical or psychological literature, ever been shown to lead to HRV improvements of 150%+. The same principle applies to experimenter demand characteristics and placebo effects.

To refer to my earlier analogy: If a man claimed that anyone could bring 20/20 vision to most any blind person, even a militantly skeptical blind person, by merely touching their eyes in a particular, precise way; and if indeed he brought 20/20 vision, a "normalization" of eyesight to a group of blind people; and further, claimed that this was not a miracle but an easily repeatable, replicable phenomenon, then the mere demonstration alone, without control groups using placebo anti-blindness algorithms, etc., would be enough to prove that his anti-blindness algorithm is efficacious. Think about it: Why would he need to control for these other explanations, if the margin of improvement is so much greater than the plausible imaginable improvement likely due to placebo, demand characteristics of the demonstration, etc.? Only if the results were much more modest would these other suggested counter-explanations become plausible. (See the section below, "Placebo Controls Are Not Always Necessary").

In case anyone thinks that these results occur even in part due to the fact that clients "believe in the treatment", there are two points to keep in mind. First, HRV is very stable: No one has ever been shown to be able to increase their unhealthy low HRV, by any treatment, just because they strongly "believe in" that treatment. Second, any trained TFT practitioner will tell you that TFT clients are, if anything, even more skeptical, (if that is possible), than the professional skeptics. Yet, in spite of even militant skepticism from clients, TFT yields the same results for militantly skeptical clients that clients who "believe in" the treatments get.

Cohen, et al., (1999) Shows That HRV is Indeed a Marker of Psychiatric Disorders

Herbert and Gaudiano quote Cohen, Matar, Kaplan, and Kotler (1999) as saying, "HRV may hold promise as a biological marker for certain forms of psychopathology". Actually, Cohen, et al., are much bolder in their claims than Herbert and Gaudiano let on. On page 64 of that Cohen, et al., article, Cohen, et al., also say, "Changes in ANS [autonomic nervous system] function are known to accompany various mental disorders". They also say, (on page 64), "The degree of specificity of this physiological parameter is equivocal, as we are to date uncertain whether specific psychiatric disorders are accompanied by specific characteristic autonomic phenomena, or whether these phenomena represent nonspecific markers reflecting a hyper aroused state stemming from the stress of suffering from a psychiatric disorder, per se". (Emphasis added). They go on to say, "This tool [HRV] could serve to elucidate the autonomic changes characterizing specific psychiatric disorders". (Again, emphasis added).

Cohen, et al., then, are saying something very different from what Herbert and Gaudiano imply. Cohen, et al., are saying that, "Changes in ANS function are known to accompany various mental disorders," and go on to point out very simply that it's unclear, (at present) whether such measurements can measure specific "psychiatric disorders" or whether HRV measurements "represent nonspecific markers" of some kind of psychiatric disorder. Either way, Cohen, et al., are saying that it is indeed a marker of psychiatric disorder, of at least some type. Even if it's merely a "nonspecific marker" (i.e., we can't tell by HRV measurement alone whether the client is suffering a panic attack, is depressed, is suffering post traumatic stress disorder, etc.,), Cohen, et al., are clearly stating that a significant relationship between HRV measurements and psychiatric disorders exists. Perhaps Cohen, et al., would have been a better choice of reviewers to submit the Pignotti and Steinberg article to, for critical evaluation, since, unlike Herbert and Gaudiano, they have some specific experience using HRV as an outcome measure for psychological interventions.

Even if we assume that HRV measurement in this context is no more than a "nonspecific marker" showing that some kind of psychiatric disorder exists, (though the exact kind is not specified by HRV), the simple fact that such a relationship between HRV and psychiatric disorders exists, and that TFT treatments have been demonstrated to cause very significant improvements in HRV within mere minutes, is indicative of TFT's efficacy.

Herbert and Gaudiano misconstrue Cohen, et al., (1999). They write, "Cohen, et al. state that HRV has the potential of providing a promising measure of clinical improvement if future research can confirm it's utility for this use". The "potential" that Cohen, et al. refer to is for HRV to perhaps eventually "elucidate the autonomic changes characterizing specific psychiatric disorders." As a mere "nonspecific marker(s) reflecting a hyper-aroused state stemming from the stress of suffering from a psychiatric disorder, per se", according to Cohen, et al., (1999), this is not a mere "potential", but a demonstrated fact.

Remember, Cacioppo, Tassinary, and Berntson, (2000), quoted earlier, states on page 16 of the Handbook of Psychophysiology, that, "…any logical inference based on the assumption that one is dealing with an outcome relationship holds as well for marker, concomitant, or invariant psychophysiological relationships". Thus, even as a mere marker that some kind of psychiatric disorder exists, (which only means, in this context, that Pignotti and Steinberg cannot rely on HRV alone as a diagnostic tool to ascertain what the specific "psychiatric disorder" is), Pignotti and Steinberg's inferences from the premise that HRV is at least a marker of "psychiatric disorder" are indeed valid. Herbert and Gaudiano, on page 1211, state, "Such findings point to the fact that HRV is not the only-or necessarily even the most important-variable to assess when determining the effectiveness of a treatment for psychological symptoms". This is merely a straw man argument. The "marker" relationship between HRV and psychiatric disorders is just that - a marker, not a concomitant or invariant relationship. To quote Cacioppo and Tassinary (2000) again, this time from page 19 of their Handbook…, "Although invariant psychophysiological relationships offer the greatest generality, physiological concomitants, markers, and outcomes also can provide important and sometimes otherwise unattainable information about elements in the psychological domain."

"Highly Sensitive" is an Ambiguous Phrase, Which Begs the Question: How Sensitive?

Herbert and Gaudiano go on to argue, on page 1211, "…HRV is highly sensitive to a variety of measurement artifacts, and must be assessed under standardized conditions to yield reliable data". On the surface, that statement seems reasonable and in accord with common sense; however, when Herbert and Gaudiano use the phrase "highly sensitive" in this context, this is an ambiguous phrase. This of course begs the question, "how sensitive is HRV measurement to these 'variety of measurement artifacts?'" Let's see some numbers. How much change in HRV measurement has ever been shown, in the literature, to have ever occurred, even in an isolated case or two, between HRV measurements taken mere minutes apart? If "highly sensitive" turns out to mean that HRV has been shown to improve by 2%, 5%, or 10%, in any past reported studies, (let's stipulate, for sake of argument), due to "demand characteristics", or "placebo", or any other non-TFT factor that Herbert and Gaudiano can suggest, then in the improvements in HRV reported by Pignotti and Steinberg, Herbert and Gaudiano have the enormous bulk of the improvements in HRV, still left to explain. Since they can find no plausible counter-explanation, the simple fact of TFT's efficacy has thus been demonstrated.

Sitting Upright vs. Lying Down, While Measuring HRV

On page 1211, Herbert and Gaudiano criticize the fact that Pignotti and Steinberg had their clients sitting upright while HRV measurements were being taken, not lying down:

> Interestingly, clients in Pignotti and Steinberg's case studies are described as resting in an upright position when HRV measurements were taken; most psycho-physiologists measure HRV by having individuals rest in a supine or lying down position. For example, Sleight and Casadei (1995) note that the 'upright position not only increases the relative contribution of the sympathetic (LF) component, but decreases the overall variability (p.317).'" (Herbert and Gaudiano, 2001, p. 1211).

To the degree that the sympathetic component was increased and the variability reduced by sitting, this would work against TFT results! Sitting, may therefore give a more stringent test of a therapy's efficacy.

As stated above, under the section, "Kline's Objection: SDNN Is 'frequently non-specific'", Callahan has shown test results and consulted with an authority on HRV, Professor Marek Malik, who checked and confirmed that the increased SDNN readings "were not likely due to increases in the sympathetic nervous system," contrary to Herbert and Gaudiano's implication. This is in spite of the fact that the measurements were taken while the clients were sitting upright instead of lying down. (Also note, the clients were sitting in the same position, in both the before and after-treatment HRV measurements).

Were Post-Treatment Improved HRV Measurements Due to "Less Movement During Post-Treatment Assessment?"

On page 1210, Herbert and Gaudiano claim, "It is possible that movements by the subject could increase noise in the HRV recording, …making results difficult to interpret. For example, any HRV changes in Pignotti and Steinberg's clients could simply have been due to less movement during the post-treatment assessment, independent of their ECG [electrocardiograph] function". In response to this charge, in Callahan's article, "Thought Field Therapy: A Response To Our Critics and a Scrutiny of Some Old Ideas of Social Science", Callahan wrote:

> "Herbert and Gaudiano suggest movement might influence HRV scores measured with the photoplethysmyograph (PPG) instrument. Most of our tests were done with electrocardiograph (ECG) methods but those who used PPG had their subjects sit still for the five minutes of the test (treatment was never done during a test). Giardino (2001) did a comparison of PPG with ECG and found the results were comparable when the subject is at rest." (Callahan, 2001c, page 1254).

Any Proposed Counter-Explanation Must Be at Least Plausible

A critic cannot reasonably arbitrarily start saying, 'Well, it might have been caused instead by this or that'. If the proposed counter-explanations are not shown to at least be plausible, (again, not "proven", merely "plausible"), then the counter-explanations fail, and are to be rejected. In a criminal court trial, it's the difference between proof beyond a "reasonable doubt" and proof consisting of absolute mathematical certainty, beyond any and all doubt and skepticism, both reasonable and unreasonable. Herbert and Gaudiano are straining to avoid the obvious conclusion, that TFT is indeed demonstrated to be efficacious. My Young Earth Creationist acquaintances strain just as hard to avoid having to concede that the earth has been quite amply demonstrated to be far, far more than six to ten thousand years old.

Why Placebo is Not a Plausible Explanation

Let's consider Herbert and Gaudiano's remarks that placebo is at least a plausible counter-explanation. On page 1212, they write, "Furthermore, Cowan, Kogan, Burr, Hendershot, and Buchanan (1990) found that cardiac patients could learn to increase their HRV through biofeedback training. Thus, the idea that HRV is immune to placebo effects in the way that those effects are understood in the psychotherapy literature is not supported by the literature". Notice the two things Herbert and Gaudiano's citing of Cowan, et al., omits; without which their attempted refuting of the notion, 'HRV is not affected by placebo', fails. 1) What was the largest percentage of HRV improvement in Cowan, et al.'s study, that was attributed to biofeedback? Is that percentage of improvement even close to the percentage improvement reported in case after case, by Pignotti and Steinberg? 2) What was the minimum amount of time it took the clients in the Cowan, et al., study to reach their individual peak HRV measurement? Was it weeks? Was it months? A relatively modest HRV improvement after weeks or months of biofeedback sessions is not equivalent at all to the degree of HRV improvements within minutes reported by Pignotti and Steinberg.

Only if the Cowan study showed very significant improvements within minutes, instead of marginal, modest HRV improvements over weeks or months, would the Cowan study's results imply a plausible counter-explanation for Pignotti and Steinberg's results. In fact, Cowan, et al., (1990) report that HRV improved roughly 8%, after five weeks of biofeedback training. That five weeks of biofeedback training consisted of one-hour sessions, twice a week, and the subjects were asked to do some relaxation/breathing exercises at home, four times a day. The Cowan, et al., results show that even when trying to manipulate HRV by relaxation, breathing, etc., that after hours of training over several weeks, Cowan, et al., were able to demonstrate only an 8% improvement in HRV, a modest fraction of what Pignotti and Steinberg show after minutes of TFT treatment. Thus, Herbert and Gaudiano's attempted refutation, by suggesting that placebo is a plausible counter-explanation and citing Cowan, et al., fails.

If a few minutes of relaxing, breathing deeply, getting attention and 'positive expectations of improvement' from a therapist could possibly cause the degree of improvements in HRV reported by Pignotti and Steinberg, then those clients in the Cowan, et al., study, who received many hours of such attention, should have at least as significant a set of results as Pignotti and Steinberg, (and Callahan) report. If we are to infer anything at all from Cowan, et al., about the possible role of placebo, suggestibility, therapist expectancy, etc., it would be that such factors, undoubtedly present in the Cowan study, are at best shown to make an 8% difference, and at that, only after many hours of biofeedback training spread over several weeks. Thus, if for sake of argument we stipulate that placebo is a plausible explanation at all for some fraction of Pignotti and Steinberg's results, placebo would only explain, at best, 8%, of the improvements reported.

Is the Autonomic Nervous System Easily Susceptible to the Placebo Effect?

On page 1211 of Herbert and Gaudiano's article, they write: "It is well established that the autonomic nervous system can be brought under some degree of voluntary control, and can be influenced by placebo treatments (Ross & Buckalew, 1985)." The essay by Ross and Buckalew that Herbert and Gaudiano cite does not draw that conclusion, at all. They are implying that the Ross & Buckalew essay they cite lends support to the notion that the autonomic nervous system "can be influenced by placebo treatments".

In that essay, titled, "Placebo Agentry: Assessment of Drug and Placebo Effects", Ross and Buckalew (1985) actually say this about the ANS and its ability to be affected by placebo:

"For affective measures, self-perceptions of relaxation and activation seemed conducive to placebo manipulation, though changes in general mood appeared less reliably altered. While clinical indices of placebo manipulations were numerous, lack of replication and differences in evaluation clouded conclusions; nevertheless, strong evidence exists for the suggestion that the physical nature of the placebo may be a significant factor in determining its effect. The measures surveyed ranged from anxiety to writing speed, from angina pain to ward activity, and from functions mediated by the autonomic nervous system to clearly higher-level mental processes. Naturally, there are appreciable problems in any attempt to draw firm conclusions about the nature and extent of variables responsive to placebo effects, due to the lack of comparability between and replications of studies, different modes of evaluation, and different control conditions." (Ross and Buckalew, 1985, p. 73).

My point is that this is a far cry from Herbert and Gaudiano's characterization that the Ross and Buckalew essay supports their claim that "It is well established that the autonomic nervous system can be brought under some degree of voluntary control, and can be influenced by placebo treatments". There's a big difference between "it is well established" vs. "there are appreciable problems in any attempt to draw firm conclusions…" If Herbert and Gaudiano want to argue that HRV is easily manipulated by placebo, in direct contradiction to the conclusion drawn in, for example, Kleiger, et al., (1991), let them find and cite articles or other sources that in fact support that such voluntary control of HRV, and susceptibility to placebo, is "well established". As they are searching for such a source, they should also keep in mind that "A series of three studies by Ray, et al., (2000), found that the degree of hypnotic susceptibility in an individual was not related to degree of change in HRV." (Callahan, 2001a, p. 1157-1158). On top of the Cowen, et al., (1990) study, Ray, et al., (2000), is further evidence that HRV is not under the degree of voluntary control or susceptibility to manipulation that Herbert and Gaudiano imply that it is.

Kleiger, et al., (1991) Shows That HRV is Not Influenced at all by Placebo

The Kleiger, et al., (1991) study showed that HRV is not influenced at all by placebo. Herbert and Gaudiano argue that the Kleiger, et al., (1991) study is not relevant to psychotherapy studies, because "the researchers did not lead the subjects to believe that the pill would produce any specific beneficial effects." (Herbert and Guadiano, 2001, p. 1211). Herbert and Gaudiano misunderstand placebo effects, here: For a treatment result to be properly considered a placebo effect, it is not necessary for the subject to be told or led to believe that the pill or other treatment will produce some specified effect. People are culturally conditioned to believe that if a doctor gives them a pill, that taking the pill will cause them to "feel better", or to otherwise show some kind of improvement, whether the doctor explicitly says so, or not.

Although the subjects in the Kleiger, et al., (1991) study were healthy, (i.e., showing no major health problems), they each wore Holter monitors for 24 hours both at the beginning of their participation in the study, and at the end. Subjects could thus easily infer that the pills given had something to do with improving their heart, even if they had no idea what the term "heart rate variability" meant. The pill, along with the inferences gleaned from having researchers hook up a Holter monitor, was sufficient enough to have caused a placebo effect, if HRV indeed responds to the placebo effect. The researchers did not need to explicitly tell the subjects that the pill would produce any specific response. It's enough only that the subjects believed that the pills would produce improvements of some kind, i.e., that it was an "active" treatment, (even though these subjects were already healthy).

Those improvements need not have been specified in advance, in order for any reported benefit to be properly considered a placebo response. It's common in drug efficacy studies for subjects in a placebo group, (who obviously don't know that they are receiving an inert substance), to sometimes report any number of very specific results, including beneficial and/or harmful "side effects" to the "drug", which they were never told by researchers to anticipate.

The beliefs of the subject receiving a treatment are the crucial facts to determining whether or not an effect in the subject of an inert substance or treatment is a placebo effect. In an earlier article, Gaudiano wrote, "Callahan claims that placebo is not a factor because many people do not believe that the treatment will work. Callahan demonstrates a misconception of placebo here, because it is only necessary for the person to believe that they are receiving some sort of 'active' treatment' (which they are told by the therapist that they are receiving) for placebo effects to be produced-not that they necessarily believe the treatment will work." (Gaudiano, 2000; emphasis added).

But that's just the point: In TFT treatments, many people don't even believe that they are getting any kind of "active" treatment, at all. In fact, people get significant, and in many documented cases, unprecedented improvements, even if they actively "disbelieve", and are militantly skeptical. Many openly skeptical people not only do not believe that they are getting an active treatment when they receive a TFT treatment, they often believe that by merely "tapping", they are not getting any kind of genuine "treatment", at all: They sometimes even think it's just a joke, or a game. How can Gaudiano call it a placebo effect, if the client does not even believe that he or she is receiving any kind of "real" treatment, at all? Gaudiano must still explain the fact that even openly skeptical people who do not believe that the TFT treatment they are receiving is anything more than a "joke", and not any kind of "active" treatment at all, still nevertheless get results similar to those who "believe in" TFT.

Without that element of belief on the client's part that the TFT treatment is some kind of "active" treatment, (even if the client believes that TFT works for reasons other that what Callahan and his colleagues believe it does), any significant improvement immediately after a TFT treatment cannot be a "placebo" effect. If the client believes that the "tapping" is "just a placebo", then there is a logical contradiction in calling any beneficial, significant results of the treatment a "placebo effect", if the client significantly improves immediately after a TFT treatment. For something to be a placebo response, it is necessary for a significant result to occur, or for the client to believe that a significant result has occurred; and for that result to be attributed to the treatment, and for that result to have not actually in fact been actively caused by that treatment.

Doctors don't tell their patients, "Take two placebos and call me in the morning". Patients don't walk into a doctor's office and say, "Can you write me a prescription for placebos?" By definition, if a patient believes that the doctor gave him or her a prescription for something that's "just a placebo", then that lack of belief that he or she is receiving an "active" treatment means that any benefit received cannot be considered a placebo effect, for that patient in that context. A placebo effect is any effect of a treatment or substance that is in fact "inactive", that the patient/client mistakenly believes is an "active" treatment; i.e., a treatment that the patient/client attributes his or her treatment results to. If there's no attribution, there's therefore by definition no placebo response. TFT clients very often believe that they have received no "genuine" (i.e., "active") treatment at all.

Consider the case where a subject is in fact receiving an inert substance, (without knowing that the substance is inert), and receives a significant improvement in symptoms similar to those in the group receiving an "active" drug, anyway. If this individual nevertheless does not believe that the inert substance was the cause, (or a significant factor) in his or her significant improvement, then strictly speaking, we should not call his or her response a placebo response. It might be more proper to perhaps call it a case of spontaneous remission, or to say the cause or causes are undetermined.

Consider that a doctor would have no reason to give an inert substance in the place of an active drug, to a patient who is in a coma, and call it a "placebo response", if the patient's condition improves considerably, immediately after he or she receives the inert substance. It would be more appropriate to label the improvement a spontaneous remission, or some other cause. The comatose patient cannot attribute his or her improvement to the inert substance, since he or she is in a coma. This just illustrates why without some kind of positive belief, you cannot properly label something a placebo response.

It should be kept in mind that the above discussion of placebo is in the context of actual clinical practice and individual case studies. The nature and meaning of "placebo" is complex, and there are other aspects of the concept "placebo" that do not apply at all to the current discussion.

While considering the alleged role of placebo responses in TFT treatments, consider the following statement by Callahan:

> "Because of the skepticism with which some clients and psychologists view Thought Field Therapy, I've often said that we don't get our fair share of so-called placebo cures (not that we really need them!). We are constantly working against negative expectations rather than being the beneficiaries of positive ones. Even if the placebo effect exists, a precondition for placebo success is a deep belief in the therapeutic technique by the client and/or the doctor (preferably both). But many people approach TFT with a militant disbelief that the technique could ever be effective. Despite the intensity of this skepticism, TFT still works." (Callahan and Trubo, 2001, p. 203).

To clarify a misconception of some critics, "placebo" tapping treatments are of course, still possible, when testing TFT, and Callahan and his colleagues have never said otherwise. However, to qualify as a placebo result, the client/subject must believe that the "fake" algorithm, used as a control, in fact caused their actual improvements.

"Placebo Controls are Not Always Necessary"

TFT critics often argue that control groups are necessary, before claims of treatment efficacy can be properly validated. In a book of essays on the theory and mechanisms of placebo, the essay, "Placebo Controls Are Not Always Necessary", (Finkel, 1985), argues that in some cases, no control group is necessary. In some cases, you can rely on "historical controls, which involve(s) comparison of the results from a new intervention with prior experience obtained in a comparable group of patients receiving no therapy or a known effective regimen…It is also appropriate when the effect of a therapy is self-evident". (Finkel, 1985, p. 419).

The studies by Callahan and his colleagues published in the October 2001 JCLP show HRV improvements within mere minutes, in case after case, by amounts unprecedented in medical or psychological literature. The entirety of medical and psychological literature that looks at HRV as an outcome measure for a psychological intervention constitutes historical controls for the studies by Callahan and his colleagues. Cowen (1990), and Carney (2000) are especially relevant as historical controls. Critics are welcome to compare the reported results of Callahan and his colleagues with these studies, which can serve as "historical controls", if the critics don't feel that the reported results, unprecedented in medical or psychological literature, make TFT's efficacy self-evident.

The Client Does the Tapping, Not the Therapist

On page 1209 of Herbert and Guadiano's article, they write, "…the very nature of the TFT protocol suggests that experimenter demand effects are likely to be operational because the therapist continues tapping until patients 'self-report of no trace of emotional or physical distress' (p. 1197). Patients are therefore subjected to strong social pressure to report a reduction in SUD to the therapist and terminate the tapping procedure".

First, it is just not plausible that alleged "strong social pressure to report a reduction in SUD to the therapist…" explains the improvements within minutes of the client's HRV, by amounts unprecedented in medical or psychological literature. Second, Herbert and Guadiano mistakenly imply that the therapist is doing the tapping. In fact, the client is doing the tapping. On page 1197, Pignotti and Steinberg state: "3. When it is determined which treatment points to use, the subject is then instructed to stimulate these points by tapping them five to seven times on each point in the specific, set sequence". (Pignotti and Steinberg, 2001).

Lohr Also Conflates Phenomenon With Theories, and Misunderstands the Claimed Relationship Between HRV and Emotional Health

Let's consider the article by Jeffrey M. Lohr, "Sakai, et al., Is Not An Adequate Demonstration of TFT Effectiveness", (Lohr, 2001). Many of the same errors in critical reasoning portrayed by McNally, Kline, and Herbert and Gaudiano also crop up in Lohr's article. Lohr, like the others, challenges the theory that supposedly explains how and why TFT is efficacious. None of the articles in JCLP by Callahan or his colleagues even purports to argue a theory to explain how and why TFT actually works. They merely offer demonstrations that TFT is efficacious. Therefore, all of Lohr's comments about TFT theory are quite irrelevant.

As the other authors do, Lohr badly misconstrues Sakai, et al., (2001), ["Thought Field Therapy Clinical Applications: Utilization In An HMO In Behavioral Medicine and Behavioral Health Services"], when he writes, "The authors should be much more specific about the way in which HRV is sine qua non of inducing emotional health with TFT." He echoes Kline, here, in misrepresenting the actual conclusion drawn about HRV and TFT. The simple fact that HRV very often improves to such a degree after proper TFT treatments are given is indeed evidence that something significant is occurring. As stated earlier in greater detail, inappropriately low HRV is a marker of psychiatric disorder, (of some kind), [Cohen, Matar, Kaplan, and Kotler, (1999), referred to earlier]. TFT dramatically improves very low HRV within minutes by amounts unprecedented in medical and psychological literature; and at the same time significant improvements in subjective units of distress, (SUD) also occur. Sakai, et al., are NOT arguing that the HRV/psychiatric disorder relationship is a one-to-one, invariant, context-free relationship. Thus, when Lohr uses the phrase "…HRV is sine qua non of inducing emotional health with TFT", this is a straw man position.

Lohr muddies the waters by talking about efficacy research and effectiveness research, and complains about "the small number and methodologically flawed efficacy studies" for TFT. One might wonder: Why doesn't he just try it out for himself? Simply find a couple of subjects who have both some kind of significant psychological problem, and also have very low HRV. Take a "pre-treatment" HRV, then follow with the appropriate TFT algorithm(s), and then take a "post-treatment" HRV. If one has access to the equipment, this would be easy for any critic to do. While not definitive, with a simple demonstration Lohr would see for himself that the basic claims for TFT, as amazing as they may sound, are at least credible.

The Wade Study (1990); and the Figley and Carbonell Study (1999)

On page 1230, Lohr cites an unpublished doctoral dissertation by Wade, (1990). Lohr points out that the Wade study showed very little effect on overall self-concept after a TFT phobia treatment. Lohr mistakenly implies that the Wade study was intended as an efficacy study of TFT. Wade wrote, under the heading, "Scope of the Study": "The question that this study addresses is whether a change in self-concept will result from treatment of a phobia through the use of the Callahan Technique. It is not the purpose of the study to evaluate the effectiveness of the technique". (Wade, 1990, p. 9).

While it is certainly interesting to evaluate how treatment for a phobia affects one's overall self-concept, keep in mind that Callahan has never claimed that eliminating a phobia will alone and by itself improve one's overall self-concept. It is a straw man, to imply otherwise. Callahan's only claim for the phobia algorithms is that they will eliminate a specific phobia.

Lohr also criticizes the study by Figley and Carbonell (1999), for lack of "formal statistical tests", and "no control group". [See the section above, "Placebo Control Groups Are Not Always Necessary"]. Furthermore, if reported improvements are substantial enough, we do not need statistical analysis to tell us that the results are indeed significant.

Use of SUD Ratings To Measure Treatment Efficacy: Lohr Cites a Previous Study He Co-Authored, But a Careful Reading of that Study Shows that it Contradicts the Very Point He is Making Here!

Professor Lohr, on page 1232, argues that "researchers…have identified several reasons why SUD ratings cannot be used as measures of treatment efficacy for EMDR, [Eye Movement Desensitization and Reprocessing] and the same criticism can be applied with equal force to their use in TFT". One of the articles he refers to is one that he co-authored, (Lohr, Kleinknecht, Conley, Dal Cerro, Schmidt, & Sonntag, [1992]), that in fact does not say, as Lohr claims it does, in his JCLP article, (page 1232) that "SUD ratings cannot be used as measures of treatment efficacy". In fact, it says the opposite. Contrast that statement with this statement, from Lohr, et al., 1992, page 164: Under the heading, "Future Research", stating what future EMDR research should include, Lohr, et al., (1992) state, "These assessments should include the verbal report of subjective processes (such as SUD-type ratings of emotional distress), ratings of the content of the traumatic imagery, and psycho-physiologic indices." (Lohr, et al., 1992; Emphasis added). That same article also says, about SUD ratings, that "It would have been more desirable to add standardized measures…" (Lohr, et al., 1992, p. 163; Emphasis added), along with SUD ratings, in the EMDR sessions. It does NOT say that SUD ratings cannot or should not be used. Further, on page 163 of that article, Lohr, et al., say, "the study would have been further enhanced by systematic measurement of heart rate, and the inclusion of other psycho-physiologic measures…" (Emphasis added).

Although he was talking about EMDR, think about it: Cohen, et al., (1999) have already established that there is a psycho-physiologic relationship between low HRV and psychiatric disorders. The articles in JCLP written by Callahan and his colleagues do indeed add a "psycho-physiologic measure" along with SUD ratings, to their demonstrations; namely, HRV. Lohr, et al., (1992) on the same page, (p.163), go on to say, "Evidence of reductions in psycho-physiologic indices of fear would have carried great weight". (The EMDR studies were focusing on Post Traumatic Stress Disorder). Lohr's previous article does not lend support at all to the point he's trying to make here about TFT demonstrations and SUD. In fact, it contradicts the very point he is making. His point about using SUD ratings in TFT efficacy demonstrations is muddled and poorly thought out. If he wishes to cite a study that supports his claim that "SUD ratings cannot be used as measures of treatment efficacy", he'll have to continue looking for such a study, (or conduct a study looking at that very question), because the Lohr, et al., (1992) study does not support that conclusion; in fact, it contradicts it.

Rogers, et al., (1999) Also Shows the Reliability of SUD Ratings as a Measurement of Treatment Efficacy, Contrary to Lohr's Interpretation of Rogers, et al.

Lohr also cites Rogers, et al., (1999) to support his contention that "SUD ratings cannot be used to measure treatment efficacy…" (Lohr, 2001, p. 1232). Actually, Rogers, et al., (1999) in fact lends support to the use of SUD ratings as a psychotherapy outcome measure. In the Rogers, et al., study, Vietnam veterans diagnosed with PTSD were receiving a single session of EMDR treatment for their own self-identified most traumatic combat-related memory.

On page 126, Rogers, et al., (1999) states, "the greater within-session SUD decreases observed for the EMDR group were accompanied by greater decreases in self-monitored severity of intrusiveness", (p.126). This is contrary to Lohr's claim that Rogers, et al., "have identified several reasons why SUD ratings cannot be used as measures of treatment efficacy…" (Lohr, 2001, p. 1232). In fact, the study lends support to the notion that SUD ratings can be a reliable outcome measure. While it would be mistaken to interpret Rogers, et al., (1999) as definitive proof that SUD ratings are the sine qua non of psychotherapy treatment outcome measures, Rogers, et al., (1999) certainly does not support Lohr's contention, at all. It in fact shows a correlation between SUD ratings and self-monitored "severity of intrusive recollections, nightmares, and flashbacks related to the memory…" (p. 122). Contrary to Lohr's implication, Rogers, et al., (1999), does not show that "SUD ratings cannot be used as measures of treatment efficacy".

It's true that the Rogers, et al., study discusses a previous study which showed the relationship between SUD ratings and another outcome measure to be more ambiguous, but the Rogers, et al., study also discusses the likely artifacts in that previous study which would explain that ambiguity, and what Rogers, et al., did differently in their own study, to avoid that artifact.

If Lohr, then, wants to argue or imply that SUD ratings have been clearly shown to NOT be reliable as a psychotherapy treatment outcome measure, neither of the two studies he cites to substantiate his argument, Lohr, et al., (1992), nor Rogers, et al., (1999) in fact substantiate his claim.

Lohr's Four Questions

Lohr asks the proponents of new treatments to answer, "Does your treatment work better than no treatment?" (p1232, Lohr, 2001) For TFT, the evidence is strongly favorable.

Lohr also asks if treatment works better than a placebo. The evidence presented by Cowen, Kogan, Burr, Hendershot, and Buchanan, (1990) showed that even after five weeks of biofeedback training, with, by far, much greater time and opportunity for positive expectations and many other possible factors to come into play, there was still only an 8% improvement in HRV. It is therefore difficult to see how TFT treatment results within minutes showing HRV improvements, often well over 100%, could reasonably be written off as mere "placebo" effects. The evidence strongly supports the idea that TFT has been shown to work better than the most plausible placebo, (i.e., Cowen, et al., 1990).

"Does your treatment work better than standard treatments?" No other treatment has ever been shown to improve HRV so significantly regardless of time. TFT results are accomplished within mere minutes.

Does treatment work through the processes one claims? This issue is irrelevant since theory was not presented in these articles. In science, theory is always modified, rejected or accepted on the basis of facts. Callahan and his colleagues, in these JCLP articles, are not making any arguments for the process by which TFT works. Remember, it is poor critical reasoning to conflate the questions of whether a specific phenomenon is true, and why and how, if true, it works. Questions such as how does TFT work, and why, are excellent topics for future articles.

Further, on the question whether TFT works better that standard treatments, consider the Carney, et al., (2001) study, earlier discussed, in which, after up to 16 sessions of Cognitive Behavioral Therapy, HRV was shown to have actually declined by about 4%, while Callahan and his colleagues show improvements in HRV averaging over 150% (in Callahan 2001a), after just one single, brief TFT treatment.

The "Context of Discovery" vs. the "Context of Justification"; or, the "Thinker" vs. the "Prover"

Lohr refers, on page 1232, to the distinction between the "context of discovery" and the "context of justification". The author Robert Anton Wilson (1986, p.3) calls this distinction, (and not just in the context of science, but in all areas of inquiry), "the thinker" and "the prover". The "thinker", according to Wilson, is interested in learning what is actually true about the world, and will change or modify his or her theories to fit the known, demonstrated facts. The "prover" starts out with certain theories or opinions, and is intent on "PROVING" those theories or opinions true, regardless of the facts and evidence! (Political "spin-doctors" are "provers", and so are most Young Earth Creationists.)

Lohr writes, "The report by Sakai, et al., [2001] represents research conducted explicitly in the context of justification." This statement by Lohr begs the question, "Justification of what, exactly?" Results are demonstrated, and theories or arguments are justified. Sakai, et al., does not try to justify any particular theory, but rather show simple, easy to replicate results. Lohr and some of the other TFT critics in JCLP may be what Wilson called "provers"; they seem to be intent upon PROVING that TFT is bunk, regardless of the evidence!

On page 1233, Lohr demands extraordinary evidence for TFT's extraordinary claims. Sakai, et al., provide a demonstration that something quite significant occurs, with proper TFT treatments. Lohr then demands controlled studies. I have previously, above, explained why we can rule out various specific "procedural artifacts and nonspecific factors" that other TFT critics have previously suggested. Callahan has explained why control groups are not relevant for a high success therapy (Callahan 2001c, page 1265). See also the section above, "Placebo Control Groups Are No Always Necessary", as well as, "Why Dramatic Claims Are Easier To Prove (Or Refute!) Than More Modest Claims", and recall the hypothetical "healing the blind" treatment above, which explains why control groups are not necessary, to know for certain whether an extraordinary change has occurred, or not.

Rosner and "Healing by Magic"

Let's consider Rita Rosner's article, on page 1241, (Rosner, 2001). Rosner is criticizing the article, "Thought Field Therapy-Soothing the Bad Moments of Kosovo", by Johnson, Shala, Sejdijaj, Odell, and Dabishevici, (2001). Rosner concludes on page 1244 that "…the article provides an anecdote about healing by magic". A major complaint is that supposedly the specific situation in which TFT was administered in Kosovo generated very powerful positive expectancy of healing within the minds of those being treated. Let's suppose that positive expectancy was a factor. To account for ALL of the results reported by Johnson, et al., Rosner would have to assume that Johnson and his colleagues had more charisma, to cause such incredible positive expectancy, than the greatest charismatic religious "faith-healer" practicing today, whoever that greatest "faith healer" might be.

Does Rosner seriously not see anything remarkable about a therapy reported to have such results? A more reasonable response from Rosner would have been to see if she could replicate these results. She could simply find someone who has been diagnosed with PTSD, try at least several TFT algorithms, and just see if she gets results similar to what Johnson, et al., report. That would be a lot more reasonable and interesting, than merely dismissing Johnson, et al.'s report as "magic". Those unfamiliar with the power of TFT may understandably believe it is magic. As science fiction writer Arthur C. Clarke has observed: "Any sufficiently developed technology is indistinguishable from magic", (Clarke, 1962).

"Echo Attributions"? An Example of an "Echo Attribution" by TFT Critics

In the article on page 1245, by Gerald M. Rosen and Gerald C. Davison, (Rosen and Davison, 2001), the authors are worried about what they call "echo attributions". The irony is, in these JCLP articles, we see an example of TFT critics relying on an "echo attribution" in their criticisms of TFT. Herbert and Gaudiano refer on page 1208 to the "three independent reviews of TFT", when one of those "independent reviews" was their own article, published in a non-peer reviewed journal, (namely, Skeptical Inquirer magazine), [Gaudiano and Herbert, 2000]. At best, Rosen and Davison have seriously overstated the alleged danger of "echo attributions" by TFT advocates. The entire October 2001 JCLP issue, including the articles highly critical of TFT, was non-peer reviewed before publication. However, Rosen and Davison do not seem concerned about "echo attributions" by TFT critics. What if TFT critics in JCLP cite their own non-peer reviewed articles from this issue, as evidence that The Journal of Clinical Psychology has supposedly thoroughly refuted the basic claims of Thought Field Therapy, and then quote their own, non-peer reviewed words from that article, to support that claim?

On page 1249 of their article, Rosen and Davison write: "It also is of concern that future restrictions on continuing education credits for TFT workshops, or sanctions resulting from clinical use of the method, will be countered by citing the present articles, perhaps ignoring that they were not peer reviewed." Again, Rosen and Davison ignore the possibility that TFT critics could conceivably cite the JCLP articles highly critical of TFT, to state Boards and professional organizations, relying on the prestige of the name of the Journal of Clinical Psychology, ignoring that those critical articles were not peer-reviewed, either. Instead of citing a prestigious journal, to decide whether or not the claims for TFT are credible, the rational thing to do is to just replicate the TFT protocol, and observe the results for one's self.

The Arizona Board

Rosen and Davison also briefly comment on the 1999 controversy in Arizona, where the State Board of Psychologist examiners sanctioned a TFT-trained psychologist. Rosen and Davison give a misleading impression of the implications of the Arizona Board's Order, in the case. For a detailed rebuttal to the critics' comments about the controversy, read: "The Arizona Board: The Difference Between a Legal Inquiry Vs. A Scientific Inquiry"; (Barger, 2002).

Rosen and Davison Also Make the "Peer Review" Objection

Let's put Rosen and Davison's complaint about "lack of peer review" for TFT's basic claims into perspective. With a list of all of TFT's algorithms, and precise instructions on when and how to apply them, any psychologist, indeed any layperson, can perform simple demonstrations with TFT, and see for themselves whether or not they get any significant or otherwise interesting results, on themselves or on openly skeptical friends or colleagues. All of the TFT algorithms, with complete instructions, are in the book, Tapping the Healer Within: Using Thought Field Therapy To Instantly Conquer Your Fears, Anxieties, and Emotional Distress, by Callahan and Trubo, (2001). Thus, any critic could quickly see for him or herself whether or not the basic claims of TFT are at least credible. I trust that most psychologists who are interested in learning whether these claims are at least credible are also quite capable of applying good critical reasoning.

For example, any psychologist interested in seeing whether or not TFT's claims are credible can try a TFT algorithm on a skeptical colleague, or on him or herself, and see what results they get. Anyone who wants to "look through the telescope" for themselves can see that something quite significant occurs when, and only when proper TFT treatments are used.

Callahan and his colleagues, in their JCLP articles, have demonstrated that TFT significantly improves very low HRV, and lowers inappropriately high HRV, within minutes, in many cases to a degree no other intervention in medical or psychological literature has ever been shown to do. Any critic can simply pick up a book, read off the appropriate algorithm, and do his or her own simple demonstrations with TFT. The fact that Callahan's claims are not considered "mainstream" does not make them untrue. Scientific truth is objective; the truth or falsehood of Callahan's claims are determined by easily repeatable demonstration, not by taking a poll of clinical psychologists, or looking to the rulings of a legal body. Neither Galileo nor Darwin needed opinion polls or full acceptance by their colleagues, to make their fascinating but controversial discoveries true.

CONCLUSION

We've seen that some critics commonly mistakenly conflate the phenomenon of TFT with theories purporting to explain how and why it works. The critics suggested several different counter-explanations for the results reported by Callahan and his colleagues, but each of these counter-explanations has been shown to not be plausible.

The difference between proof and persuasion has been presented. The results achieved by Callahan and his colleagues can be replicated by anyone who cares to do so. The fact that some critics are still not persuaded doesn't mean that TFT's efficacy hasn't been proven. As we've shown, many of the arguments presented against TFT turn out to have been straw men, and this article has corrected the numerous misunderstandings and misrepresentations of the actual claims made for TFT. Other researchers should be encouraged to attempt to replicate the results of TFT. The efficacy of Thought Field Therapy has been proven.

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