Callahan Fails to Meet the Burden of Proof for Thought Field Therapy Claims

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Callahan's response evades the key issues raised by merely restating and elaborating upon what has already been said, providing citations that are out of context and irrelevant to the issues at hand, and misrepresenting what was actually said by his critics and me and the authors of articles he cites. He spends paragraphs refuting "straw men." He provides additional anecdotes, which offer no convincing evidence for his claims. His critics have expressed concern that Callahan and Thought Field Therapy (TFT) proponents will cite his response article, as published in the Journal of Clinical Psychology, to promote TFT, as TFT proponents have repeatedly done for the non-peer-reviewed earlier issue devoted to TFT. Callahan has been given an unprecedented opportunity to present his work in a reputable journal without prior peer review and has failed to meet the burden of proof for his claims, thus undermining his own claim, that his work has been rejected solely as a result of bias against innovation. © 2004 Wiley Periodicals, Inc. J Clin Psychol 61: 251–255, 2005.

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Callahan's response contains so many irrelevancies and misrepresentations of what I wrote that knowing where to begin is difficult. Given the space allotted to me for this rejoinder, rather than attempt a full, point-by-point rebuttal, I will identify his major errors and address the key points raised.

Callahan misrepresents the position I took on short-term heart rate variability (HRV) as disparagement of short-term HRV per se. He conflates the validity of HRV itself with the use of sound methodology and appropriate measures. My criticism concerned how short-term HRV was used in the Thought Field Therapy (TFT) studies (Callahan, 2001a,

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2001b; Pignotti & Steinberg, 2001) in which we chose to use a time-domain measure of HRV, standard deviation of normal-to-normal (SDNN). Kline (2001) pointed out the necessity of including frequency domain analysis, which would provide a specific breakdown of the non-frequency-specific time-domain measure, SDNN, in order to determine whether increases were legitimate (that is, whether they were mediated by the parasympathetic nervous system). Callahan's (2001c) rebuttal to Kline that he sent cases of frequency domain analysis, pre and post TFT, to an expert, who declared them to be legitimate is inadequate. I assisted Callahan in compiling this report and know that he selected those few cases (including some of my own data) that presented optimal results rather than presenting all the actual HRV data on frequency domain analysis from our *Journal of Clinical Psychology* studies. The expert never saw the complete data. Research with proper sampling methods is required to determine whether the increases in SDNN were legitimate.

The article I previously cited (Kautzner & Hnatkova, 1995; also cited by Herbert & Gaudiano, 2001) is indeed relevant to this discussion, because the authors recommended frequency domain analysis for short-term HRV with the limited use of certain other time-domain measures, but not SDNN, which they recommended for the 24-hour test. Thus, if we were going to use SDNN as our main measure of HRV, we should have used 24-hour HRV testing, for which norms, cutoff points, and adequate studies on stability exist. Callahan failed to see the relevance of this citation because he misunderstood the point I was making.

Additionally, he quoted Bigger, Fleiss, Rolnitzky, and Steinman (1993) as stating that short-term HRV results are "remarkably similar to those calculated over 24 hours" (p. 927) but dropped the context the authors were referring to: frequency domain, not time domain measures such as SDNN. None of the studies he cited used SDNN with short-term HRV as a measure of treatment efficacy. His references to short-term HRV as capable of diagnosing neuropathy and predicting sudden cardiac death are completely irrelevant to the issue of whether or not pre- and post-short-term HRV testing, inappropriately using only SDNN, under uncontrolled conditions is a valid measure to provide evidence for TFT's efficacy.

In his article in this volume, Callahan concedes that his generalization from the Bilchick and colleagues study (2002) was speculative. However, he fails to make this point clear on the website and newsletter articles I have previously referred to; instead he uses the study to make unwarranted and misleading claims about TFT, for example, that "it suggests that the treatment can generate about a 160% decrease in the chance of death" (Callahan, 2004, p. 2).

Callahan's statement in this issue that "Pignotti reiterates the accusation that I believe HRV is an index of health" creates a straw man: I am not criticizing HRV as one index of health; I am concurring with Kline's (2001) criticism of Callahan's notion of HRV as the context-free sine qua non of better mental and physical health and of Callahan's unwarranted conclusions about the relation of TFT and HRV to health.

Callahan missed my point in regard to his inappropriate comparison of the study performed on cognitive-behavioral therapy (CBT) (Carney et al., 2000) with his TFT case reports. The CBT study used 24-hour HRV and did not select for successful cases, as the TFT reports did. In addition, the population studied consisted of depressed patients who had coronary heart disease (CHD). In further rebuttal to his inaccurate portrayal of this study as indicating poor results for CBT, it is worth mentioning that the study did show significant change in root mean square of successive differences (rMSSD), an HRV measure that reflects parasympathetic change, and showed a finding of significant change in heart rate. The participants in the study who received CBT also had a significant favorable change in the Beck Depression Inventory, a well-recognized assessment measure for depression, and that change provides evidence against the predictive validity of SDNN for depression outcomes in this population.

Furthermore, I stand by my statement that Callahan was incorrect to imply that CBT made SDNN worse in this study. Because the lower SDNN was not statistically significant, the statement that the CBT intervention had anything to do with this slight lowering of SDNN is not correct. The authors speculated on the permanent biological effects, not because of the nonsignificantly lower mean after CBT, as Callahan misrepresents, but rather, because of the fact that all except one of the HRV measures failed to normalize completely to the level of nondepressed control group patients who had CHD. Callahan failed to mention that they offered another, more optimistic possibility: that these measures could gradually improve over time and stated that "CBT may have a beneficial effect, on a risk factor for mortality in depressed patients with coronary heart disease" (Carney et al., p. 639).

Callahan's response to my criticism of our use of the Subjective Units of Distress Scale (SUD) as a measure of treatment efficacy is a reference to papers he and colleagues wrote in the 1950s in which he claims a SUD-like measure was used before Wolpe; therefore, he states, he does not feel constrained by conventional views on the SUD. Such a response appears to be nothing more than an argument from authority because he does not demonstrate how it is a rebuttal to the problems with the use of SUD measurements outlined in more recent literature pointed out by other critics and by me (e.g., Lohr, 2001).

Instead of addressing problems noted with demand characteristics of the SUD in the example of his radio show TFT treatment for depression (Callahan, 1994), he notes that the patient's facial color changes. He goes off on an anecdotal tangent about pre- and post-TFT treatment changes that occurred in a discredited laboratory test (Barrett, 2003). TFT treatment was performed on a patient who had flu, whose face reportedly became flushed during the treatment. Apparently, it did not occur to Callahan that there could be explanations other than the restoration of health and well-being for blushing during a TFT treatment.

Callahan asserts that the patient on the radio show did not initially report a lowering of her SUD because she said she did not understand. Careful listening to the tape reveals that initially she reported no change. Only after Callahan questioned her did she say that she felt "different" but did not know why. After she reported a two-point drop in the SUD the treatment was continued. Later, when she again reported no change in her SUD, it was Callahan who told her, "Remember you were reluctant to admit it before, because you didn't understand," after repeatedly questioning her self-report. When she continued to report an unchanged SUD level, he tested her SUD with TFT and informed her that his tests were indicating lowering of the SUD score. Clearly, this patient was incessantly questioned, challenged, and contradicted until she changed her self-report. It would appear that the cognitive dissonance alleged by Callahan was his own, not the patient's.

As for Callahan's assertion that TFT's results could not be attributable to placebo effect because TFT is effective for animals, we need to ask, As indicated by what type of assessment? It is usually pet owners who make the evaluation about behavioral changes, and as human beings they have beliefs and opinions, which they are fully capable of projecting onto their pets when deciding whether treatment is effective. Callahan provides no convincing rebuttal of the possibility that HRV could change in response to positive expectancy. To the contrary, Callahan (personal communication, 2000) once mentioned that one of his trainees, who was a devout Christian, experienced a change in HRV after engaging in prayer. Apparently, it did not occur to him that this anecdote is an excellent example that belief and positive expectancy can produce a change in HRV. Studies are required if we are to accept the assertion that HRV is placebo-effect free in the realm of psychology, in terms of positive belief and expectancy.

The anecdote about the raised HRV and T-cell count findings for the acquired immunodeficiency syndrome (AIDS) patient after TFT treatments appears to illustrate the post hoc ergo propter hoc (after this, therefore because of this) fallacy: there is no evidence that reported changes were caused by TFT. The fact that the patient was continuing to take AIDS drugs, which presumably have been proved to raise T-cell counts, was the most likely explanation. In fact, a report in Callahan's newsletter on this case (Hanson, 2002) reveals that the patient's T-cell count had already begun to rise, from 30 to 320, before he had started TFT. As for the reduction of side effects, a more parsimonious explanation is that the patient simply adjusted to the drugs and the side effects abated. To determine whether TFT raises T-cell counts or reduces side effects of AIDS drugs, randomized controlled studies would be required.

The larger issue is that Callahan and TFT proponents have failed to meet the burden of proof for TFT's efficacy. Simply invoking the technology of HRV and the alleged opinions of unnamed experts on selected samples of successful cases does not eliminate the need for well-designed studies, using valid psychological assessment measures and systematic follow-up of participants (not the sporadic, anecdotal follow-up he cites as reported in Pignotti & Steinberg, 2001, and in his anecdotes). The follow-up noted by Callahan in the Kosovo study (Johnson, Shala, Sejdijaj, Odell, & Dabishevci, 2001) was meaningless because there were no valid assessment measures used at any point during the study. Not even the SUD was used in this study; only a self-report of presence or absence of distress, which has highly questionable reliability, especially considering the cultural taboos on reporting emotional suffering noted by the authors, was used.

Given that Callahan charges trainees $100,000 for his proprietary trade secret Voice Technology (VT) (Robb, 2003) and has, for years, charged his own clients $3,000 for 5 hours of VT (Callahan, 1994), I am puzzled as to why funding of research to support the claims of his therapy would be a problem. If TFT is as good as proponents claim, its efficacy should be easy to demonstrate with valid psychological assessment measures.

Rosen and Davison (2001) express concern about Callahan and colleagues' misusing the non-peer-reviewed issue of the *Journal of Clinical Psychology* by citing the articles and neglecting to mention the lack of peer review and by posting specific quoted statements from the articles and attributing them to the journal, creating an "echo attribution." Since the publication of that issue, this misuse has occurred repeatedly (Callahan, 2002; Gaudiano, 2002; Robb, 2003), demonstrating that the concern is well founded. Similar concerns about whether the contents of Callahan's response in this issue will be cited in a similar manner are therefore warranted. To the title "Unprecedented changes in short-term heart rate variability due to thought field therapy" it would be all too easy for TFT proponents to add in their promotions, "as published in the *Journal of Clinical Psychology*."

The only valid conclusion that can be drawn from this series of articles is that Callahan and his colleagues, who were given an unprecedented opportunity to have their work published in a reputable journal without prior peer review, have failed to meet the burden of proof, thus suggesting good cause for the difficulty TFT proponents may have had in the past with peer-reviewed publication. Their difficulty would appear to have little to do with rejection of innovation and everything to do with their coming up short on valid evidence.

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