
C O M M E N T

Regarding the October 2001 *Journal of Clinical Psychology* Special Issue on Thought Field Therapy: Retraction of Conclusions in the Article "Heart Rate Variability as an Outcome Measure for Thought Field Therapy in Clinical Practice"



Monica Pignotti
Independent Scholar

This article is a retraction of the conclusions drawn in a previous article, published as part of a special October 2001 issue of the *Journal of Clinical Psychology* on Thought Field Therapy (TFT). I decided to write this retraction after reconsidering a number of issues raised in the critiques of the articles. Additionally, subsequent misinterpretations of the literature on heart rate variability (HRV) by Roger Callahan, which led to further questioning of his premises and claims regarding TFT and HRV as represented in the articles, are discussed. I conclude that the burden of proof is on TFT proponents to demonstrate its efficacy and that well-designed controlled studies using standardized assessment measures and long-term follow-up must be performed to allow the scientific community to take claims made for TFT seriously. © 2004 Wiley Periodicals, Inc. *J Clin Psychol* 61: 361–365, 2005.

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I thank the authors of the Thought Field Therapy (TFT) critiques of the *Journal of Clinical Psychology* October 2001 issue: James D. Herbert, Brandon A. Gaudiano, John P. Kline, Jeffrey M. Lohr, and Richard J. McNally. Though all of them did not directly critique my article, their responses were highly relevant to the understanding I have gained on these matters. I also thank James Herbert for giving me his comments on an earlier draft of this article. Additionally, I would like to thank James Herbert, Scott Lilienfeld, and Richard McNally for generously taking the time to have some very helpful discussions with me regarding their point of view on Thought Field Therapy (TFT). I also wish to thank my coauthor, Mark Steinberg, who has accepted our differences of opinion on TFT and remained my colleague and friend.

Correspondence concerning this article should be addressed to: Monica Pignotti, 3563 South Bascom Avenue #1, Campbell, CA, 95008; e-mail: pignotti@worldnet.att.net.

After considerable reflection, I have decided to retract the conclusions I drew about Thought Field Therapy (TFT) and heart rate variability (HRV) in an article I coauthored (Pignotti & Steinberg, 2001) for this journal. That article was part of a special issue of the *Journal of Clinical Psychology* devoted to TFT, in which non-peer-reviewed articles were published along with critiques (Beutler, 2001). It should also be noted that my coauthor, Mark Steinberg, does not agree with my current position and stands by the conclusions stated in our article. I now agree with most of the major points made in the critiques of the articles relevant to mine (Herbert & Gaudiano, 2001; Kline, 2001; Lohr, 2001; McNally, 2001). These critiques pointed out flaws that included selection of only successful cases; focus on a diversity of problems; failure to use a control group; failure to control for placebo effect, demand characteristics, and regression to the mean; lack of valid assessment measures; use of the Subjective Units of Distress Scale (SUD) as the only measure of efficacy other than HRV; inappropriate use of an out-of-context physiological measure (HRV); and lack of a credible theory.

This change in position resulted from my doubts and concerns in observing that Callahan, in the years that followed the publication of this issue, was making increasingly extraordinary and unwarranted claims in his promotion of TFT, particularly in regard to HRV. For example, he misinterprets and overgeneralizes a study by Bilchick and associates (2002) to apply to his results with TFT although in the abstract the authors clearly stated that the study applied to a limited population, patients who had congestive heart failure. Bilchick and colleagues found in their study that for this population, each increase of 10% in standard deviation of normal-to-normal (SDNN) (a time-domain measure of HRV) conferred a 20% decrease in risk of mortality. Callahan, in three separate articles (2002, 2003, 2004), misinterpreted this study to conclude that because, as measured by 5-minute HRV tests under uncontrolled conditions, people had changes in SDNN pre and post TFT treatment that were frequently 100% or greater, therefore TFT could prevent heart problems and decrease risk of mortality by the percentages reported by Bilchick and coworkers. This claim is evidence that Callahan is continuing to make the same erroneous assumptions that he made in his *Journal of Clinical Psychology* articles, pointed out by Kline (2001), that "HRV stands in both one-to-one and context free relations to better health. According to Callahan, HRV is the sine qua non of better mental and physical health" (p. 1190). Bilchick and colleagues were appropriately making highly context-bound conclusions, yet Callahan uses these data to draw context-free conclusions to promote TFT.

In the same articles (Callahan, 2002, 2003) and in a subsequent promotional flyer for TFT trainings (Bray, 2004), Callahan presents a bar graph in which he makes this same form of error of inappropriate context dropping. The graph reports changes in SDNN, as reported in the HRV literature, related to biofeedback, smoking cessation, cognitive therapy, the drug sertraline, and TFT (Callahan cites no references to specific studies in his report; he is referring to studies cited in Callahan, 2001a). In the promotional flyer above the graph is the caption "TFT is the most effective therapy ever!" The graph displays an 80% increase in the SDNN of selected putatively "successful"¹ cases of people who were treated for depression with TFT, comparing the increase to much lower increases and non-statistically significant decreases reported in the literature. The problem with this comparison is that the other studies were done under much different conditions and with 24-hour HRV testing. This is not in any way a legitimate comparison,

¹"Successful" is in quotation marks because Callahan used no standardized measures for depression. He used only the SUD and HRV and did not follow up to determine whether the results were maintained.

drops the context of the conditions under which the studies were done, and thus is highly misleading to potential mental health consumers reading the website or the promotions.

After analyzing the comparison, I could no longer ignore these obvious misinterpretations of the literature, and it was apparent to me that there were major critical reasoning errors in Callahan's inferences about TFT and HRV. Thus, I began to have serious doubts about and subsequently reconsidered all the claims he had made regarding HRV and reread the critiques (Herbert & Gaudiano, 2001; Kline, 2001; Lohr, 2001; McNally, 2001) of our articles.

Critics (Herbert & Gaudiano, 2001; McNally, 2001) wondered why we used HRV instead of standard assessment measures. Callahan chose to use HRV because he believes it is a more difficult measure to influence than psychological assessment measures typically used and that TFT treatment is robust and changes HRV, whereas the more conventional forms of psychotherapy produce minimal results and do not affect HRV (Callahan & Callahan, 2000). Furthermore, Callahan believes that control groups are not needed in studies on HRV. He bases these assumptions on the premises (1) that HRV is immune to placebo effect, (2) that HRV is stable over time, and (3) that existing interventions have very little effect on SDNN (Callahan, 2001a, 2001b; Pignotti & Steinberg, 2001).

However, the study cited on the stability of SDNN (Kleiger et al., 1991; Van Hoogenhuyze et al., 1991; Bigger, Fleiss, Rolnitzky, & Steinman, 1992; Stein, Rich, Rottman, & Kleiger, 1995) uses the 24-hour test and includes no data on stability as measured by 5-minute HRV tests. Indeed, Herbert and Gaudiano (2001) point out that there are major differences between the 5-minute and 24-hour HRV tests (Kautzner & Hnatkova, 1995). Callahan (2001c) responds to Herbert and Gaudiano's criticism that there were no norms for the 5-minute test by stating that percentage change is being reported. However, his answer does not address the problem of the lack of existing literature to support the stability and test-retest reliability of 5-minute SDNN. With regard to placebo effect, the studies cited do not address its context in psychological treatments; thus, it makes sense to me that this possibility cannot be ruled out.

In addition to these problems, some of the TFT practitioners who owned the same HRV equipment that we had used for the studies (Biocom Technologies, 1998–1999) reported on the TFT listserve and in e-mails to me that when they ran 5-minute tests on themselves and then repeated the tests with no intervention, they had noted major changes in SDNN. I also performed an informal experiment on myself, running 5-minute HRV tests pre and post aerobic exercise, and repeatedly noticed changes of 100% or greater in SDNN much higher than reported in the existing literature. Callahan (personal communication) dismissed these reports as highly unusual. It has been my experience that he tends to ignore any new information that might challenge his existing beliefs regarding TFT.

All of these factors have led me to conclude that there were major flaws in the way HRV was employed in our studies, as well as in the inferences we drew from them. Therefore, these data cannot be considered probative and, thus, the conclusions are unwarranted. Until studies on TFT and HRV are made under controlled conditions and more modest, context-specific conclusions are drawn from any data that might support actual favorable change in HRV measurements from these controlled studies, the claims that Callahan and others are currently making for TFT and HRV cannot be considered valid.

The only other measure of efficacy used in our studies was the SUD Scale (Wolpe, 1969). However, that scale was originally meant to be used as a process variable to monitor the patient's emotional state during the treatment, rather than as a means to measure treatment efficacy (Lohr, 2001; Lohr et al., 1992). Although Callahan (2001c, 2004) has repeatedly defended his use of the SUD and stated that the client's self-report

has consistently been his standard for treatment efficacy, the use of the SUD is highly problematic because of the potential for introducing demand characteristics.² That is, therapist expectation for improvement could unintentionally be transferred to the patient and influence the self-report.

Although Callahan repeatedly asserts that there is no substitute for the client's self-report (Callahan & Callahan, 2000; Callahan & Trubo, 2001), I have reached a different conclusion after careful consideration: that there is no substitute for randomized controlled studies of TFT that use standardized assessment measures. Until such studies are performed and produce data to support Callahan's claims, I have to agree with McNally (2001) that mental health professionals cannot be expected to pay attention to the claims made for TFT by Callahan and its proponents.

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²Some of the audio and video demonstration tapes in which Callahan has been shown on TV treating people for psychological problems (Callahan, 1997), which he adduces as evidence in support of the power of TFT, make it evident that such demand characteristics as well as positive expectancy have indeed been introduced into the process. On one of the audiotapes included in his training packet as an example to trainees of how TFT should be administered (Callahan, 1994), Callahan can be heard questioning a woman being treated for depression on a radio show, who reports she has had no change in her SUD; he asks whether she is sure and then tells her that his proprietary TFT testing procedures have revealed that her SUD is much lower than she reported. He then explicitly suggests that she will probably feel these effects very soon, and the radio host remarks on how much better she looks.

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