
The Search for the Holy Grail: Heart Rate Variability and Thought Field Therapy



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Thought Field Therapy (TFT) is one of several unusual psychotherapies that have witnessed rapid growth over the past few years, despite the absence of scientific support. Promoters of TFT frequently cite changes in heart rate variability (HRV) as evidence of TFT's effects. Pignotti and Steinberg (2001) present reports of 39 cases in which HRV was assessed prior to and immediately following TFT. Serious methodological shortcomings preclude interpretation of these data with respect to either the efficacy of TFT or the clinical utility of HRV. Ethical concerns are raised about the aggressive promotion of TFT and the misuse of HRV. © 2001 John Wiley & Sons, Inc. *J Clin Psychol* 57: 1207–1214, 2001.

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Although Callahan Techniques®-Thought Field Therapy (TFT) was initially developed over 20 years ago, it has only recently attracted widespread attention among practicing psychotherapists. TFT is a kind of psychological acupuncture in which therapists instruct their clients to tap on different body parts while thinking about their problems to relieve psychological distress. Currently, Callahan claims that over 5,000 mental health professionals have been trained in the unorthodox techniques. TFT is part of a larger rise in the so-called “power,” “energy,” or “neoteric” therapies over the last few years. These therapies share several features: extraordinary claims of effectiveness for a wide range of

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psychological problems that greatly outstrip their empirical support, claims of unusually rapid treatment effects, unusual theoretical bases, and aggressive marketing and dissemination efforts to practitioners through proprietary workshops (Herbert et al., 2000; Lohr, Montgomery, Lilienfeld, & Tolin, 1999; Rosen, Lohr, McNally, & Herbert, 1998).

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 (Gaudio & Herbert, 2000; Hooke, 1998; Swenson, 1999). Recently, Callahan and his followers have focused a great deal of attention on heart rate variability (HRV) as a measure of TFT's putative effects (Callahan & Callahan, 2000). They have not, however, provided a clear rationale for why HRV is a more appropriate measure than standard diagnostic and symptom assessments, and have not published research linking HRV to these other measures. Despite the absence of data, TFT and its numerous "energy" therapy offshoots, such as Emotional Freedom Techniques (Craig, 1997) and Touch and Breathe (Diepold, 2000), are now being aggressively marketed and appear to be growing in popularity. In this context, good data on the efficacy of TFT and the reliability, validity, and utility of HRV are welcomed. Unfortunately, the paper by Pignotti and Steinberg sheds little new light on these issues.

Pignotti and Steinberg's Purpose?

It is not clear if Pignotti and Steinberg's article is intended to be about the utility of HRV as a measure of psychopathology in clinical practice or about the effectiveness of TFT as a therapeutic modality. The initial paragraph suggests the former: "The purpose of this article is not to provide a controlled study, but rather to show how HRV can be used as a tool to measure treatment outcomes in a clinical practice setting" (p. 1194). The subsequent paragraph, however, introduces TFT, and TFT is the only treatment technique utilized in the case descriptions that follow. The specific link between HRV and TFT is never articulated in the article. In other words, if the central focus of the article is truly intended to be on HRV as an outcome measure, then it is not clear why a novel, unconventional intervention like TFT was chosen over established treatments. On the other hand, if the article is really about TFT, then it is not clear why a new and controversial assessment measure like HRV is highlighted as the outcome measure, rather than accepted, standard measures of psychopathology.

To understand this confusion, one must look beyond the present article to other writings on the use of HRV as a measure of the effects of TFT (e.g., Callahan, 2000; Hagglund, 1999; Pignotti, 1999). Without exception, these articles and Internet postings are not written by psychophysicists 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 the promotion of TFT, rather than the evaluation of HRV per se.

Either way, the present article falls dreadfully short at demonstrating the efficacy of TFT or the efficacy of using HRV as a treatment outcome measure. The authors selected 39 cases from their private psychotherapy practices, including four cases from training seminars on TFT. The cases represent a bewildering array of psychiatric syndromes, medical problems, and problems of everyday life, including, among others, depression, various anxiety syndromes, eating disorders, body image disturbance, attention deficit hyperactivity disorder, learning disabilities, chronic fatigue, bereavement, joint pains,

headaches, asthma, and atrial fibrillation. Each patient underwent a 5-minute HRV assessment and a session of TFT followed by another 5-minute HRV assessment. Subjective ratings of distress (SUD; Wolpe 1969) were also obtained on a 1–10-point scale before and after TFT for most cases. Although no summary statistics are provided, case descriptions suggest that most patients reported an improvement in symptoms, a decrease in subjective distress, and a normalization of HRV immediately following treatment. The authors conclude that these case studies support not only the effectiveness of TFT, but also the utility of HRV as an outcome measure in clinical practice.

Methodological Weaknesses

It is difficult to know where to begin to evaluate this study, as it is so devoid of methodological rigor that the resulting data are simply uninterpretable. The most fundamental flaw is the lack of any attempt to control for experimental confounds. These include statistical regression (i.e., the tendency for extreme scores on any measure to become less extreme upon reassessment), effects due to the mere passage of time, nonstandardized methods and procedures, expectancy or “placebo” effects, and experimenter demand characteristics. Controls for such factors are not academic methodological niceties, but rather are absolutely essential to our ability to interpret treatment outcome data, particularly for novel procedures that are promoted as extraordinarily powerful and have little or no published evidence of efficacy (Herbert, 2000). Both within-group and between-group methods have been developed to control for such effects, and are widely used in outcome studies in medicine and psychology (Kazdin, 1998).

It is likely that the changes observed among the patients treated with TFT have nothing to do with balancing “energy meridians,” and everything to do with the experimental artifacts described above. Furthermore, in addition to the obvious possibility of “placebo” effects, 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 please the therapist and terminate the tapping procedure.

There are many other problems with Pignotti and Steinberg’s report. The extensive list of case studies presented with very little individual detail is completely insufficient to support the claims of the authors. It would have been much more informative if single-case experimental designs were employed to help reduce potential confounds. Also, standard symptom measures (e.g., self-report symptom inventories, structured interviews, etc.) were not administered. Furthermore, participants were so heterogeneous that generalizations to any particular group are not possible. In short, this collection of case reports will not convince any thoughtful scholar or clinician that the observed changes have anything at all to do with TFT.

HRV Measurement

But what about HRV? Even if the study design does not permit conclusions about the effects of TFT, might these data support the utility of HRV as an outcome measure in clinical practice, as the authors suggest? Although HRV may hold promise as a biological marker for certain forms of psychopathology (Cohen, Matar, Kaplan, & Kotler, 1999), the use of HRV in this report is not consistent with the scientific literature on the topic and provides little useful data on the procedure.

The authors provide a brief but unsophisticated review of the HRV literature from both cardiology and psychiatry. They correctly note that HRV is an index of the variation in heart beat intervals relating to autonomic regulation, and that low HRV has been shown to be associated with various cardiac conditions and to be a risk factor for general morbidity and mortality (Kleiger, Miller, & Bigger, 1987). However, Pignotti and Steinberg inaccurately state that "HRV has been used increasingly as a measure for psychotherapy outcomes" (p. 1194), and then cite Cohen, Kotler, Matar, Kaplan, Miodownik, & Cassuto (1999) to support this claim. Cohen et al. state that HRV has the *potential* of providing a promising measure of clinical improvement if future research can confirm its utility for this use. The treatment outcome use of HRV to date has been largely restricted to psychopharmacology research (e.g., Roose et al., 1998).

Unfortunately, Pignotti and Steinberg's review reflects only a superficial appreciation of the issues involved in the measurement of HRV and the relevance of these measurements to psychopathology. First, regarding HRV measurement, the distinction between time domain analysis and the generally preferred frequency domain analysis (also known as power spectrum density; PSD) is not discussed. From PSD is computed a high-frequency (HF) measure, thought to reflect parasympathetic activity, and a low frequency (LF) measure, thought to indicate sympathetic activity (Cerutti, Bianchi, & Mainardi, 1995). Although the HF component is fairly well established as a measure of parasympathetic activity, the degree to which the LF component reflects sympathetic activity remains controversial (Cohen et al., 1999). Pignotti and Steinberg used time domain analysis measures (i.e., SDNN); however, PSD is the more widely used method of assessing HRV in clinical research. Furthermore, the authors noted that they had to use the 24-hour test norms for comparison because 5-minute norms do not exist. Kautzner and Hnatkova (1995) state that the "length of recording significantly influences the results of quantitative HRV assessment" (p. 119), and that important differences exist between the 5-minute and 24-hour tests. Finally, the use of the "HeartScanner" (Biocom Technologies, 2000) to assess HRV may be questionable. The manual presents no data on the reliability or accuracy of the instrument. Most importantly, the HeartScanner uses a photoplethysmograph (PPG) instrument comprised of a "finger sensor" to record beat-to-beat changes in peripheral blood flow, instead of the universally accepted electrocardiograph (ECG) methods. It is possible that movements by the subject could increase noise in the HRV recording using a PPG, 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 posttreatment assessment, independent of their ECG function. Interestingly, Kennedy (1995) provides an extensive list of commercial manufacturers of HRV instruments, all of which use some form of ECG recording.

HRV and Psychopathology

Pignotti and Steinberg also cite research suggesting that low HRV may be associated with depressive and anxiety syndromes. In terms of psychopathology, the authors overstate the extant research literature. The empirical research to date has only provided support for the general finding that HRV is lower in individuals with particular anxiety disorders when compared with controls, including panic (Klien, Cnaani, Harel, Braun, & Ben-Haim, 1995) and posttraumatic stress (Cohen et al., 1997). Because anxiety and stress are known to be associated with increased physiological arousal, it is not surprising that persons with anxiety disorders will differ from nonanxious controls on measures reflecting increased sympathetic and/or decreased parasympathetic activity (Cohen et al., 1999). As Pignotti and Steinberg correctly note, studies of HRV between depressed individuals

and controls have produced inconsistent findings (Yeragani, Balon, Pohl, & Ramesh, 1995). Most importantly, it should be noted that psychotropic medications with anticholinergic effects actually decrease HRV, while still being associated with reductions in symptomatology (Roose et al., 1998). 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.

Alternate Explanations

Conflicting results have resulted from the use of differing data collection methods and standard, reliable HRV values are not yet available for psychiatric populations. Most importantly, HRV is affected by a wide range of factors, including age, gender, body position, rate of breathing, smoking, hour of the day, medications (Cohen et al., 1999), and even sexual intercourse (Brody, Veit, & Rau, 2000). In other words, HRV is highly sensitive to a variety of measurement artifacts, and must be assessed under standardized conditions to yield reliable data. Interestingly, clients in Pignotti and Steinberg's case studies are described as resting in an upright position when HRV measurements were taken; most psychophysicologists 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). In fact, little explicit information is provided in the paper as to how exactly HRV measurements were recorded. All the research we reviewed concerning HRV included detailed and technical descriptions of the recording procedures used. If the goal of Pignotti and Steinberg's article is to provide support for the use of HRV, a detailed description of testing methods and procedures for measuring HRV would be necessary. The ambiguity surrounding how HRV measurements were taken in the case studies preclude replication by independent researchers.

As discussed previously, it is possible—even likely—that the changes in HRV were not due to TFT, but to experimental artifacts or incorrect, unstandardized assessment procedures. Specifically, as individuals acclimate to novel or stressful environments they tend to relax and breathe more slowly, and these changes will be reflected in HRV measurements if not controlled for in the recording. Therefore, the observed changes in HRV could simply have occurred from the passage of time during the session. Rate of respiration is an important determinant of respiratory sinus arrhythmia, and decreasing respiration will increase parasympathetic activity and therefore increase HRV (Hirsch & Bishop, 1981). This effect underscores the need for standardized assessment conditions for HRV. The present case reports cannot rule out the possibility that any changes in HRV were due simply to an acclimation response between the two HRV assessments.

The only reference Pignotti and Steinberg make to any explanation other than TFT for the HRV changes is to dismiss the possibility of placebo effects, stating that "it has been established that HRV does *not* respond to the placebo effect" (p. 1195). For example, the authors cite a study by Kleiger et al. (1991) in which HRV did not change in normal volunteers following administration of a sugar pill. The Kleiger et al. study, however, does not address placebo effects in psychotherapy trials, and is irrelevant to the present circumstance. Kleiger et al. studied normal subjects who had no medical or psychological problem for which they expected to be treated. Moreover, the researchers did not lead the subjects to believe that the pill would produce any specific beneficial effects. The other studies cited to attest to HRV's "immunity" to placebo in psychotherapy pertain to research related to a motion-sickness patch (e.g., Casadei, Conway, Forfar, & Sleight, 1996). 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). 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. The need for appropriate experimental controls cannot be dismissed.

Conclusions and Implications

If the authors are truly interested in the utility of HRV as an outcome measure for psychopathology, it is not clear why they did not study this issue directly. Why not evaluate the validity of HRV in relation to established measures for particular populations? What about the reliability of various HRV assessment methods with psychiatric patients? It would seem that a serious investigation of HRV as an outcome measure would follow well-established psychometric methods. Unfortunately, and contrary to the authors' claims, the present study provides no useful data on the reliability, validity, or utility of HRV in clinical practice.

In conclusion, the unsystematic collection of case reports presented by Pignotti and Steinberg does not provide convincing support for the utility of HRV in general clinical practice nor for the efficacy of TFT specifically. The authors' claims to the contrary are not supported by the data they present. Furthermore, this article raises serious ethical questions concerning the use of a highly unusual and unconventional treatment procedure with seriously disturbed and/or vulnerable persons, particularly when established, empirically supported treatments are available. The 39 people treated by Pignotti and Steinberg included children as young as six, and persons experiencing suicidal ideation, clinical depression, and other serious concerns. When treating potentially life-threatening problems for which effective treatments are available, we believe that the use of experimental approaches such as TFT should be confined to research protocols that have been approved by appropriate institutional review boards, with informed consent procedures. Unfortunately, it is our fear that the publishing of Pignotti and Steinberg's report may ultimately be used unjustifiably to continue to promote the use of TFT without any solid empirical demonstration of its efficacy after 20 years of development.

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