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Background: *Psychotherapy*, a journal of the American Psychological Association (http://www.apa.org/journals/pst), published a paper by David Feinstein, "Energy Psychology: A Review of the Preliminary Evidence," in its June 2008 issue (*45*(2), 199-213.). Two commentaries highly critical of that paper were received by the journal, peer-reviewed, and accepted for publication. The journal allowed Dr. Feinstein to submit a response to these commentaries. This "rejoinder" follows. All three pieces were published in the June 2009 issue of the journal. Please note that the following may not exactly replicate the final copy-edited version. It is not the "copy of record."

Facts, Paradigms, and Anomalies in the Acceptance of Energy Psychology: A Rejoinder to McCaslin's (2009) and Pignotti and Thyer's (2009) Comments on Feinstein (2008a)

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Abstract

Allegations of selection bias and other departures from critical thinking in Feinstein (2008a), found in the Pignotti and Thyer and the McCaslin commentaries (2009, this issue), are addressed. Inaccuracies and bias in the reviewers' comments are also examined. The exchange is shown to reflect a paradigmatic clash within the professional community, with energy psychology having become a lightning rod for this controversy. While postulated "subtle energies" and "energy fields" are entangled in this debate, the most salient paradigm problem for energy psychology may simply be that accumulating reports of its speed and power have not been explained using established clinical models.

The Pignotti and Thayer and the McCaslin commentaries (this issue) on Feinstein (2008a) attempt to discredit the evidence presented regarding the efficacy of energy psychology. While offering some provocative observations, the commentators also introduce various inaccuracies and distortions, including allegations of selection bias and other deceptiveness on my part. I will begin by addressing false allegations and other misstatements, examine the efficacy issues, and finally review the paradigm clash that fuels the passionate discourse around energy psychology.

Selection Bias. Pignotti and Thyer (this issue) claim "selective bias" (p. 258) largely because the paper did not include two studies, both randomized controlled trials (RCTs), by Waite and Holder (2003) and by Pignotti (2005b). McCaslin (this issue) contends that the paper did "a disservice to readers" (p. 252) by not mentioning the Waite and Holder study. Both the Waite and Holder and the Pignotti studies were actually reviewed in earlier, widely circulated drafts of my paper, but later deleted for reasons discussed below. What is puzzling about the commentators' position, however, is that the two studies, had they been included, would have actually *supported* the claim that tapping on the body is effective as a treatment of emotional symptoms:

- In Pignotti's (2005b) study, 33 subjects tapped a set of acupuncture points recommended in a prescribed Thought Field Therapy (TFT) protocol done in the suggested sequence; 33 tapped acupuncture points used in TFT in a random sequence. Both groups showed equal (and remarkable) pre- to post-treatment improvement after single brief sessions: "97% of the 66 participants reported a complete elimination of all subjective emotional distress" (Pignotti, 2005b, p. 38).
- Waite and Holder (2003) tested three tapping conditions and a no-treatment control condition on 119 college students with self-reported fear of heights. One of the tapping conditions utilized a variation of a manualized Emotional Freedom Techniques (EFT) protocol; one used this protocol but substituted random points on the arm for the standard EFT points; and one used this protocol while having subjects tap on a doll. Relevant background is that using the forefinger stimulates an acupuncture point (Large Intestine 1) that is sometimes used in the treatment of "mental restlessness" (Ross, 1995, p. 306) and the arm contains numerous acupuncture points, although the researchers clearly had not conceived of the doll or arm conditions as potentially activating treatment points. In any case, the three tapping conditions all resulted in significant reductions in self-reported fear ($p < .003$, .001, and .001, respectively). The placebo group did not ($p = .255$).

Pignotti (2005b) attributed non-specific therapeutic factors such as expectancy, social-demand characteristics, and allegiance effects (her subjects were participants in a TFT training program) and other artifacts to the 97% self-reported success rate that was her primary empirical finding, though she presents no evidence that non-specific factors could lead to a 97% success rate. Waite and Holder (2003) concluded that while their study "establishes that certain techniques used by EFT may be useful in the treatment of fear," the positive outcomes "appear unrelated to the unique features of EFT" (p. 25), specifically tapping on acupuncture points. They instead attribute the reported effectiveness of EFT to "characteristics it shares with more traditional therapies" (p. 25), speculating on the influences of exposure, distraction, demand characteristics, relaxation, and an auxiliary breathing technique used with the tapping.

While both studies raise interesting questions about the mechanisms and best protocols for tapping treatments, their findings support rather than contradict the hypothesis that tapping on the body while attuning to a problem has efficacy as a treatment approach. In the first study, even though the investigator's write-up emphasizes that it was a comparison study rather than a study

of efficacy, the two tapping procedures nonetheless each resulted in "a complete elimination of all subjective emotional distress" in 97% of the participants. In the second study, three tapping variations resulted in highly significant reductions of fear while the group that did not use tapping did not show improvement. It should be noted that as therapists who utilize energy psychology have developed into a professional organization approaching 1,000 members representing a variety of approaches and strategies (http://energypsych.org), strict adherence to the original tapping protocols is considered by most practitioners to be unnecessary, a development that would be consistent with the findings of both studies.

Excluding Studies *Supporting* the Efficacy of Energy Psychology. While earlier drafts of my paper did discuss these studies, when it came time to submit the paper for journal consideration, length had become a concern, and I omitted discussion of both articles, planning to refer to each in subsequent work on the mechanisms and procedures of energy psychology. I felt the papers had much more bearing on those questions (and that was a primary focus of the authors in discussing their findings) than on the efficacy of the technique, where they lent only marginal evidence due to design issues. Excluding them was not, as the commentators suggest, an attempt to select only studies that support the efficacy of energy psychology since the data from both studies do support the efficacy of tapping while activating an emotional concern.

A third article, by Carbonell (1995), mentioned by Pignotti and Thyer as an omission, was also included in earlier drafts of the paper. Carbonell used either a TFT tapping protocol or a similar protocol that tapped on points not used in TFT with 49 acrophobia subjects. While both groups improved, significantly greater improvement was found in those who tapped on the TFT points. This study again lends evidence for the efficacy of energy psychology. It too was deleted from the final draft due to space limitations and design flaws combined with the judgment that it also was more appropriate for a subsequent paper on mechanisms and procedures (all three studies address questions such as whether tapping *anywhere* on the body, not just on acupuncture points, can enhance the speed and effectiveness of exposure protocols).

The five non-refereed papers favorable toward energy psychology, published in the special 2001 issue of the *Journal of Clinical Psychology* and referred to by Pignotti and Thyer as a case of uneven coverage on my part, all had shortcomings that were detailed in commentaries in the same journal issue. None of them produced data that decisively supported or in any way refuted the efficacy of energy psychology. The single study from that issue mentioned in my paper, as an example of an "uncontrolled outcome study" (and singled out by Pignotti and Thyer as another instance of selection bias), was presented in the context that "factors independent of the intervention being investigated may have been active ingredients in the observed improvements" (Feinstein, 2008a, p. 204).

Omission of Support for a Major Assertion. McCaslin (this issue) states that I provided "no citation" for the assertion that the stimulation of acupuncture points is "believed to send signals to the amygdala and other brain centers and reduce hyperarousal" (McCaslin, this issue, p. 253). However, a study conducted at Harvard Medical School supporting this assertion was cited on p. 211 of the paper: "MRI studies have, in fact, shown that stimulating certain

acupuncture points decreases activation signals in areas of the amygdala and other brain structures involved with fear (Hui et al., 2000)."

Claims of "Probably Efficacious" Treatments. My paper presented 17 studies, including 6 uncontrolled studies and 11 reported to be RCTs (though one of these, as Pignotti and Thyer point out, falls short of that designation). Every study I could find, published and unpublished, from systematic clinical observation to RCTs, including Pignotti (2005b), Waite and Holder (2003), and Carbonell (1995), lends support for the efficacy of tapping while mentally attuning to an emotional difficulty. Despite the design flaws found in some of the studies, the preponderance of evidence shows energy psychology interventions to be efficacious.

While the reviewers did point to a number of design problems, their comments sometimes obscured rather than sharpened the relevant issues. McCaslin, for instance, discussing the Elder et al. (2007) study, states: "In an e-mail, the author stated that participants were allowed to exit and re-enter the study if they didn't show up for the 12-week check-in (C. Elder, personal communication, July 2, 2008). If, for some reason, a stable base of participants cannot be maintained, or is allowed to come and go as they please, that fact should be disclosed in the published data. In this case, the fact was not disclosed" (McCaslin, this issue, p. 251). This assertion surprised me. On inquiry, Elder responded: "This is complete nonsense. What was stated was that there had been a participant who missed the 3 month data collection, but did provide 6 month data" (C. Elder, personal communication, February 20, 2009).

Some of McCaslin's other criticisms of the studies reviewed in my paper were more cogent. He elaborates on several intricate points, such as additional ways Wells et al. might have countered for therapist allegiance, but he then generalizes from these relatively tangential observations into a sweeping dismissal of all the efficacy data on energy psychology. While I can only admire a few of his most adroit comments, neither McCaslin nor Pignotti and Thyer effectively refute my assertions that both the Wells et al. and the Elder et al. studies establish the examined protocols as "probably efficacious" for the conditions specified. Before dismissing them, it certainly should be noted that:

- While the difference between the treatment and the control conditions in the Elder et al. study ($p < .09$) did not quite reach statistical significance, the finding that is relevant for establishing the efficacy of TAT (Tapas Acupuncture Technique) as a treatment for weight loss maintenance is not whether it was statistically superior to an established treatment (the control condition, in this case, was a weight loss support group). Both treatments were significantly more effective in helping participants maintain weight loss ($p < .034$ for the support group and .000 for TAT) than the third treatment condition, which controls for placebo effects, regression to the mean, and other artifacts. TAT, in fact, resulted in "virtually no weight regain" (Elder et al., 2007, p. 78). The TAT group was also significantly superior to the support group with the subset of participants who reported a previous history of recurrent unsuccessful weight loss, a population of special clinical interest to Kaiser Permanente, the sponsor of the study. So Elder et al. not only found evidence for the efficacy of TAT in

maintaining weight loss but also found TAT superior to a support group in maintaining weight loss with a targeted population.

- McCaslin's most fundamental criticism of the Wells et al. (2003) study is that the comparison condition, diaphragmatic breathing, was neither a wait-list group nor a group that (as described by the authors of the study) utilized an established phobia treatment. However, the comparison condition, deep breathing is, as McCaslin notes, "commonly believed to control anxiety" (p. 252). Imaginal exposure, which is a well-established treatment for phobias, was also part of the protocol for both groups. The Wells study authors should have noted that they were comparing EFT to a method (exposure combined with diaphragmatic breathing) that is commonly utilized in the treatment of anxiety. McCaslin also suggests that the investigators might have further controlled for placebo effect and participant expectations "by asking the participants about potential biases beforehand and documenting their responses" and asserts that "this was not done" (p. 252). However, the investigators did exactly that, finding that t tests "showed no significant difference between the mean confidence level that any treatment would work for those later included in the EFT condition . . . as compared to those later included in the DB condition" (Wells et al, 2003, p. 951).

A partial replication of the Wells study (Baker & Siegel, 2005, presented at a conference and reported in my paper but as yet unpublished) used a no-treatment control group, along with a Rogerian-like counseling comparison condition, to control for placebo and regression to the mean. This investigation supports the findings of the Wells study, with three pre-/post- outcome measures of EFT vs. counseling reaching the .001, .001, and .002 levels of significance.

Mechanisms. McCaslin suggests that any such observed benefits of energy psychology treatments are "attributable to well-known cognitive and behavioral techniques which are included with the energy manipulation" (p. 249). He calls for dismantling studies to isolate the effects of tapping. Wells et al. (2003) *is* a dismantling study in that identical protocols were used, with the only difference being the use of tapping or diaphragmatic breathing. The tapping treatment produced significantly stronger outcomes. In Pignotti (2005b), also a dismantling study, 97% of the participants showed improvement after tapping using varying protocols. Neither finding (nor any other finding in any study I am aware of) supports McCaslin's assertion.

There is, nonetheless, wide agreement among clinicians who have informed themselves about energy psychology that the approach utilizes many established clinical principles. As my paper asserts several times, energy psychology *is* an exposure treatment. Its mechanisms of action would seem to parallel those of other exposure treatments. The debatable element is whether adding the stimulation of acupuncture points or other areas of the skin to an exposure protocol markedly increases the speed and effectiveness of that protocol. While more definitive research is clearly needed, the studies I reviewed provide preliminary evidence for that claim.

Inappropriately Citing the Division 12 Criteria. Regarding claims that a treatment has met APA Division 12 criteria, Pignotti and Thyer (this issue) state that "it is not the prerogative of an individual" to make this determination, but rather that "designating a treatment as empirically supported is a function of a Division 12-appointed committee of psychologists" (p.

260). They cite Division 12's "Website on Research-Supported Psychological Treatments" (http://www.psychology.sunysb.edu/eklonsky-/division12/index.html) in making this assertion. This website, accessed on January 29, 2009, as well as previously, makes no mention of it being the exclusive right of Division 12 to determine which therapies meet the Division 12 criteria. A purpose of such published criteria, in fact, would seem to be to allow members of the profession to apply the criteria to new therapies and to provide evidence for any determinations being proposed that can be evaluated by others—which is precisely what has occurred here. Pignotti and Thyer, nonetheless, contend that my paper makes a determination that is properly only Division 12's to claim. The source they cite, however, does not address this issue, nor does anything else I have found. And my paper is careful to not imply anything but what it states, clearly disclosing, after presenting its conclusions regarding the significance of the Wells et al. and Elder et al. findings, that "Division 12 has not yet evaluated either study in published reports" (Feinstein, 2008a, p. 212).

Conflict of Interest. Regarding disclosure of possible conflicts of interest, I agree that I probably should have provided a footnote indicating that I have written books and articles favorable about the subject being reviewed and that I offer classes on the topics of those publications. However, I felt that citing these books and articles in the references, along with listing subtitles that clearly advocate an energy approach ("Rapid Interventions for Lasting Change" and "Clinical Strengths of a Complementary Paradigm") signaled to the reader my predispositions as the author, as did the tone of the writing. I also did note on the journal's disclosure form "a significant financial interest" in that "I provide clinical services using this approach, have written three books on the topic, and consult and speak on the topic."

Commentator Bias. Both the Pignotti (2005b) and the Waite and Holder (2003) studies came to conclusions that I and others contend are not consistent with their empirical findings. After analyzing the latter, for instance, Baker and Carrington (2005) summarized: "Waite and Holder's . . . conclusions unfortunately do not follow from their data" (para. 10). It is relevant to the discussion of bias to note that both studies were published in *The Scientific Review of Mental Health Practice,* a journal closely and openly affiliated with the Commission for Scientific Medicine and Mental Health (CSMMH). The CSMMH website (http://www.csmmh.org) describes its purpose as "the scientific examination of unproven alternative medicine and mental health therapies," and its top banner is "Curing the Ills of Alternative Medicine and Questionable Mental Health Practices" (retrieved February 3, 2009). The leadership listed on the CSMMH website includes the founders of publications such as *The Skeptical Inquirer* and *Quackwatch.* The single acknowledgement in McCaslin's commentary is to the editor of *The Scientific Review of Mental Health Practice*. Pignotti, the first author of the other commentary, has published two articles in that journal and has written more than half a dozen other pieces that are critical of TFT (e.g., Pignotti, 2005a), disclosing in some of them that she had been one of "TFT's most enthusiastic proponents" (Pignotti, 2007, p. 394) before becoming disaffected with its founder and his approach.

This background should not be held as relevant for weighing the merits of the reviewers' comments about my paper. I hope I have adequately addressed the major objections on their own

terms. But this background is highly relevant for understanding the intensity of the quarter-century debate around energy psychology.

A Clash of Paradigms. Energy psychology presents the mental health field with a paradigm that is derived from health and mental health practices from other cultures, often quite unfamiliar or foreign to the Western mind. This paradigm, which holds that subtle energies and energy fields play a critical role in health and illness, has come into increasing conflict with conventional constructs as alternative medicine has been gaining prominence. The resulting paradigmatic conflict is unfolding within a much larger arena than just energy psychology.

As the CSMMH website correctly notes, alternative medicine and mental health therapies "have become increasingly popular in the United States and the world" (retrieved February 1, 2009). In 1997, in fact, an estimated 629 million visits were made to practitioners of alternative and complementary medicine in the U.S. at an out-of-pocket cost of $27 billion (Eisenberg et al., 1998). In contrast, Americans made only 386 million visits to their family doctors that year. Meanwhile, a more recent study by the National Center for Complementary and Alternative Medicine (2008) showed that the percentage of Americans seeking alternative medicine treatments slowly edged upward in the period between 2002 and 2007. The EFT newsletter alone currently has 430,000 active subscribers (G. Craig, personal communication, February 12, 2009). The stakes in this paradigm debate are substantial.

While the scientific community has been slow to investigate most alternative medicine practices, this is changing (Institute of Medicine of the National Academies, 2005). Organizations such as CSMMH, and its various affiliated publications can, in my opinion, provide a vital service by attempting to hold new entries into this rapidly expanding and largely unregulated arena to high scientific standards. The salient debate here, however, is not about the legitimate debunking of charlatans or well-marketed ineffectual therapies. It is, rather, in the false negatives, the dismissal of legitimate innovation, that may occur when healthy skepticism crosses the line into what has been referred to as "pseudoskepticism" (Truzzi, 1987).

Dismissing Anomalous Findings. Pseudoskepticism is most commonly seen in scientific discussions when observations that do not conform to conventional paradigms are dismissed in the guise of critical thinking. As Kuhn (1996) has shown, anomalous observations serve as the engine in a paradigm's evolution (or its replacement by one with greater scope and precision), yet members of a profession tend to circle the wagons to block the impact anomalous findings may have on conventional formulations. In Kuhn's words, when "confronted by even severe and prolonged anomalies, [scientists] do not renounce the paradigm" (p. 77).

An analysis of how anomalous information is typically dismissed identifies underlying assumptions that skew the ways data is interpreted (Carter, 2007). For instance, assuming that the *possibility* of extraneous influences in an experiment *explains* unexpected findings is a way of discounting anomalous observations. Pignotti (2005b) observed a 97% success rate for two variations of tapping. Waite and Holder (2003) reported .001, .001, and .003 pre- /post improvement probabilities in three variations of tapping. Wells et al. (2003) found exposure/ tapping to be superior to exposure/diaphragmatic breathing on four measures ($p < .005$, .005, 02, and .02, respectively). While it is possible that these findings could be explained by non-specific

therapeutic factors, a more parsimonious (though outside-the-paradigm) conclusion would be that percussion using the fingers while mentally activating a fearful stimulus or other emotional problem, even during a single brief session, reduces arousal to the stimulus. Parsimony involves not only striving for the simplest available explanations in interpreting data; it also requires that the explanations used reasonably account for all the data. But again and again, the authors of both commentaries strain—extending to McCaslin's inaccurate portrayal of Elder's personal communication—to find explanations that are consistent with their worldviews rather than consistent with observations that do not support those worldviews.

A familiar maxim applied to discredit anomalous observations is "The plural of anecdote is not data" (used by Pignotti & Thyer, this issue, p. 259), but this ignores the fact that in the early developmental phases of a clinical breakthrough, all the evidence is anecdotal. While energy psychology is no longer in an early developmental phase, and more substantial empirical evidence has accumulated and continues to accumulate, literally thousands of favorable case outcomes have been reported by practitioners of varied clinical backgrounds and theoretical orientations (sources described in Feinstein, 2008a). This constitutes a different order of evidence than the reports of a method's originator or protégés. Again, while it is possible that expectancy effects, other non-specific factors, and financial interests by promoters—the explanations posited by Pignotti and Thyer (this issue)—have induced a mass hysteria toward rapidly overcoming long-standing emotional problems in thousands of individuals, it is more reasonable to consider that the large body of anecdotal evidence claiming improvement using tapping/exposure protocols may have *some* bearing on the efficacy of the method.

McCaslin (this issue) goes much further than merely dismissing anecdotal reports, stating "nowhere in the history of psychology, medicine, anatomy, physiology, or biology is there any evidence that human beings have an energy field" (p. 253), failing to mention the abundant scientific evidence presented in Oschman's (2000) *Energy Medicine: The Scientific Basis* or Rubik's (2002) work on biofields, among many other sources. In addition, the experiment from *JAMA*, recounted by McCaslin in support of his statement, has been discredited from a statistical standpoint as "an exemplar of the misuse of science" (Cox, 2004, p. 75). Even other professional skeptics have described it as a case where conclusions drawn by fellow skeptics, journalists, the public, the paper's authors, and the editor of *JAMA* were erroneous, noting that the experiment "does not prove that the HEF [human energy field] does not exist" (Selby, 1998, para. 6).

A Dramatic and a Less Glaring Paradigm Problem. From its outset, energy psychology has posed a number of challenges to the psychotherapy field. It is a method whose explanatory accounts do not appear to conform to conventional models within psychology, and it has become a lightning rod in the paradigm clash between those who claim that subtle energies are a decisive agent in the action of alternative health practices and those who discount the existence or importance of such energies. Beyond this conspicuous paradigm clash, energy psychology has another paradigm problem in relationship to becoming accepted within mainstream clinical practice. The most troubling anomaly presented by energy psychology does not involve putative energy fields. It is the speed and power with which positive clinical results are reported for challenging conditions. The strong outcomes described by Pignotti (2005b) and Waite and Holder (2003) were based on extremely brief, single-session tapping treatments. From

early claims of the *Five Minute Phobia Cure* (Callahan, 1985) to the rapid responses observed in traumatized disaster survivors (Feinstein, 2008b), such reports have led to cognitive dissonance —or outright dismissal—in many conventionally trained clinicians. In the workshops I conduct, the speed with which dramatic changes apparently occur is often reported as being as perplexing as it is inspiring to therapists new to the method. Whether the active ingredient turns out to be acupuncture points, energy fields, some artifact of stimulating the surface of the skin, or a yet undetermined agent, the mechanisms leading to such rapid outcomes are not explained by traditional clinical paradigms.

Consequences. The issue is not just philosophical. The dissemination of energy psychology has been institutionally curbed since the APA censured the approach in a memo to its CE sponsors as not being a legitimate topic for psychology CE credits (Murray, 1999). For instance, of more than 160 presentations at the 2008 *Psychotherapy Networker* Symposium in Washington, D.C., organized by one of APA's major CE sponsors, the only clinical presentation that was identified in the program as specifically not eligible for APA CE credit was mine on "Energy Psychology in Disaster Relief." Of the other clinical programs, many of which were not evidence-based, all were eligible. According to Larry Stoler, Ph.D., a past President of the Association for Comprehensive Energy Psychology, the APA's position on energy psychology "has beyond question prevented significant numbers of psychologists from learning about energy psychology and has drastically slowed scholarly research by, in effect, branding energy psychology as illegitimate" (personal communication, February 25, 2009).

Meanwhile, reports from more than a dozen countries, coming not only from practitioners but also from independent local health care authorities whose responsibilities include identifying effective interventions, suggest strong favorable outcomes using energy psychology in the aftermath of natural and human-made disasters (Feinstein, 2008b). That psychologists are prevented from receiving CE credits for informing themselves about these developments does not serve science, clinical practice, or the APA's core objective of promoting "human welfare" (Article 1.1, Bylaws of the American Psychological Association, retrieved February 11, 2009, from http://www.apa.org/governance/bylaws/art1.html). Such exclusionary practices instead inhibit the distribution of knowledge about a potentially potent though anomalous breakthrough in the treatment of PTSD and other serious disorders at a time when the need for more effective treatments has never been more pressing.

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