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Corrigendum To: Is Tapping on Acupuncture Points an Active Ingredient in Emotional Freedom Techniques: A Systematic Review and Meta-analysis of Comparative Studies

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Abstract

We published a meta-analysis of component trials of Emotional Freedom Techniques (EFT) in this journal. EFT is an evidence-based method, validated in over 100 clinical trials, which uses fingertip tapping on acupressure points in conjunction with techniques from exposure and cognitive therapy. The meta-analysis examined six studies in which an active control such as diaphragmatic breathing (DB) or sham acupoints was used in place of tapping on actual acupoints. The purpose of the meta-analysis was to determine whether tapping was an inert or an active ingredient in EFTs observed treatment effects. Subsequent to publication, errors in the statistical analysis were identified, primarily incorrect standard deviation (SD) values, and our methodological approach was questioned by others. We therefore had the meta-analysis re-run by an independent senior statistician who compared pre- to follow-up results to determine the sustained effects of treatment. The cumulative fixed effects Hedge's *g*-value was found to be 0.73, 95% confidence interval: 0.42, 1.04, $p < 0.0001$. The corresponding random effects Hedge's *g*-value is 0.74, 95% confidence interval: 0.34, 1.13, $p < 0.0001$. We also reviewed and clarified our methodology. In conclusion, despite computational errors in our original publication, the present revised analysis supports the original conclusion that the acupressure component of the EFT protocol is an active ingredient that contributes to the method's favourable health effects.

Corrigendum To: Is Tapping on Acupuncture Points an Active Ingredient in Emotional Freedom Techniques: A Systematic Review and Meta-analysis of Comparative Studies

Emotional Freedom Techniques (EFT) has been the subject of over 100 clinical trials. Over 40 review papers and meta-analyses have summarized the research demonstrating its efficacy for anxiety, depression, PTSD, phobias, pain, and a variety of physical conditions. EFT combines elements of cognitive and behavioural therapies with the novel ingredient of acupressure, in the form of fingertip tapping on acupuncture points.

The aim of the original paper was to determine if acupoint tapping was an active or inert ingredient (Church, Stapleton, Yang, & Gallo, 2018). Since EFT also includes elements drawn from conventional psychotherapy techniques, it is possible that its measured effects are entirely due to these, with tapping merely serving as a placebo, distraction, or inert ingredient.

A literature search found six studies in which tapping was replaced by an active control, such as diaphragmatic breathing (DB) or tapping on sham acupoints. Three of these met the identified quality control standards and were selected for analysis. The meta-analysis found a moderate treatment effect size for tapping over and above the control conditions. We concluded that EFTs acupressure component was an active ingredient and that the positive treatment outcomes were not due solely to placebo, the nonspecific effects of any therapy, or EFTs non-acupressure components.

After publication, we were contacted by Glen Spielmans, Ph.D. of Metropolitan University in Saint Paul, MN, who indicated that there might be computational errors in our paper. He and colleagues wrote a paper criticizing 5 elements of our methodology (Spielmans et al., 2020). Here we examine those claims one by one, and provide a re-analysis of our original paper.

Revision to Table 3

We requested that the original statistician, who is independent and has no therapeutic allegiance to the EFT method, re-analyze the data, but she was not available. We therefore requested a fresh analysis from a senior statistician, the third author above, who is also independent and without therapeutic allegiance. He determined that Table 3 in the original paper included several incorrect entries, primarily incorrect standard deviations, and pointed out that it only presented results for the full EFT (including tapping) condition, and not the control condition used in the 3 studies of interest (i.e. EFT without tapping but with an active control).

In addition, the original table 3 presented a mix of results from pre- to post-assessment and pre- to follow-up assessment. Of note, the Fox (2013) paper included in the analysis did not provide pre- to post-assessment results. Moreover, as the principal question of interest is whether EFT with tapping results in sustained treatment effects, as compared to EFT without tapping, he compared the results in the revised Table 3 (below) with a focus on pre- to follow-up results to determine the sustained effects of treatment.

INSERT TABLE 3 HERE

As seen in the revised Table 3, across the 3 studies and 6 outcomes evaluated (from pre- to follow-up assessment), positive effect sizes (ES) associated with the full EFT with tapping regimen were consistently observed. These ESs ranged from small (2 outcomes: ES = 0.33, 0.35), to medium (2 outcomes: ES = 0.52, 0.56), to large (2 outcomes: ES = 1.13, 1.71).

Considering the effect sizes obtained from the pre- to follow-up assessments for all 6 outcomes evaluated, the cumulative fixed effects Hedge's g -value is 0.73, 95% confidence interval: 0.42, 1.04, $p < 0.0001$. The corresponding random effects Hedge's g -value is 0.74, 95% confidence interval: 0.34, 1.13, $p < 0.0001$. These revised and corrected numbers provide slightly stronger evidence for acupressure's effects than those in the original paper.

Methodological Procedures

Spielmanns et al. (2020), list the following methodological criticisms of our meta-analysis:

“Specifically, a) two included studies did not include participants with documented mental health problems, b) two included studies did not specifically isolate the effect of acupoint tapping, c) clear rationales for selected measures were not provided, d) comparison groups were not bona fide therapies, e) researcher and therapist allegiances were not controlled, and f) selection of included studies may have been biased.”

Here is our response to each of these points in turn.

a) Two included studies did not include participants with documented mental health problems.

The quality standards used to identify studies for inclusion in our meta-analysis are the Meta-analysis Reporting Standards (MARS) criteria described by the American Psychological Association (APA) reporting standards group (APA Publications and Communications Board Working Group on Journal Article Reporting Standards, 2008).

The MARS criteria require the use of “validated” assessment tools with defined “psychometric and biometric properties.” They do not require the diagnosis or documentation of a mental health condition. The three studies we included in the primary analysis met this criterion.

Our meta-analysis focused on a single narrow question: Does the tapping component of EFT contribute to the effectiveness of the technique?

There are several other meta-analyses of EFT that address mental health diagnoses such as anxiety, depression, and PTSD. EFTs efficacy for these conditions can be determined by referencing these studies. The fact that these quite different questions were not addressed by our meta-analysis does not compromise the results.

b) Two included studies did not specifically isolate the effect of acupoint tapping.

Spielmanns et al. (2020) correctly state that two of the included studies omitted both EFTs “Setup Statement” as well as the tapping portion of the protocol. This design characteristic is stated in both studies (Fox 2013, Wells et al., 2003). Spielmanns et al. (2020) therefore conclude that because the Setup Statement as well as acupoint tapping was omitted, these two studies failed to isolate the acupoint tapping portion of the EFT protocol.

This criticism rests on an inadequate understanding of both the manualized EFT protocol, and the explicit intent of the Setup Statement. This misunderstanding is quite understandable given that none of the authors in Spielmans et al. (2020) have been trained in EFT.

The Setup Statement is typically spoken aloud or silently at the beginning of an EFT treatment. It is followed by the “Reminder Phrase.” The purpose of the Setup Statement and Reminder Phrase is to keep the client focused on the source of emotional distress as each acupressure point is stimulated.

In his control group, Fox (2013) omitted both the tapping and the Setup Statement. However, participants still focused mentally on the triggering emotion, being given the instruction to “observe how the emotion felt in their body.” Wells states that in the control group, “participants were instructed to ‘tune into’ or focus on their fear throughout the treatment (exactly as in EFT) by repeating a similar Reminder Phrase (e.g., ‘This fear of spiders’) between each breath.”

The mental focus on the emotion in the Fox (2013) study, and the use of the Reminder Phrase in the Wells et al. (2003) study, can both reasonably have been expected to maintain client focus on the presenting problem. *The EFT Manual* (3rd edition, Church, 2013, page 148) recommends that a client: “say any words that keep you focused on the problem, whether they’re the perfect words or not.” The EFT protocol thus explicitly states that the mental focus on the issue is key, not the particular words used, or whether they’re explicitly characterized as a Setup Statement or Reminder Phrase.

Because this mental focus on the problem was maintained in both the Wells and Fox studies, we do not believe that the omission of the Setup Statement made a material difference to the results of those two studies or to the conclusions of our meta-analysis.

c) Clear rationales for selected measures were not provided

Spielmans et al (2020) refer to the “unclear rationale regarding selection of measures,” and note that of 25 measures, only 6 were selected for analysis. In our paper, we provide a clear definition of why these particular measures were selected: they were “items representative of psychological distress.”

Since the populations in the included studies were heterogeneous, with complaints ranging from frozen shoulder to phobias, we chose measures that could be pooled for statistical analysis. Assessments of psychological distress were available for those studies that met the inclusion criteria. We thus provided a clear rationale for the measures chosen.

d) Comparison groups were not bona fide therapies.

The comparison conditions typically omitted the tapping component of EFT in favor of a substitute activity, such as diaphragmatic breathing or acupressure on sham points. We made no claim that these substitute activities were bona fide therapies.

There are a number of studies comparing EFT to bona fide therapies such as EMDR, CBT, and NET. To establish the efficacy of EFT relative to these other methods, these studies provide a useful guide. However, it was not the purpose of our meta-analysis to compare EFT to another therapy. Instead, the research question was entirely different: to evaluate the relative contribution of acupressure tapping to the EFT protocol.

Further, Spielmans et al. (2020) appear to misunderstand the nature of a comparative study. In such a study, as defined by Wampold and Imel (2015), a single component of a therapy is omitted, and a substitute activity is introduced. This controls for any placebo or expectancy component of the observed change. For this reason, we regard comparative studies as superior to dismantling studies, in which a single element is omitted, because these fail to control for the expectancy effect of that particular element.

Introducing all the many components of a bona fide therapy, for instance CBT or EMDR, as Spielmans et al. (2020) advocate, instead of a substitute activity, would invalidate a comparative study, because more than one substitute component would be introduced, confounding the study from the design phase on.

CBT for instance has both cognitive and behavioral components, and these two categories can be further broken down into at least five sub-components (Pompoli et al., 2018). EMDR has eight steps, and if it had been used as a substitute activity in the studies in our meta-analysis, the substitution of eight separate activities for the single one (tapping) would have confounded the results, because it would be impossible to determine the relative contribution of each of the eight to the observed effects.

Instead, in the studies included in our meta-analysis, only a single component such as diaphragmatic breathing or sham acupressure was substituted for tapping. This controlled for expectancy effects and the placebo response, and is the design most appropriate to isolating the effect of acupressure tapping.

The criticism that comparison groups were not bona fide therapies represents a misunderstanding of the design of component studies. Spielmans et al. (2020) in fact frequently mischaracterize them as "dismantling" studies, apparently unaware of the substantial differences between the two methodologies.

e) Researcher and therapist allegiances were not controlled

In limitations section of the Discussion portion of our meta-analysis, we clearly state that in one of the three studies analyzed, therapist allegiance was controlled for, while the other two studies, "did not report any attempts to control for therapist allegiance." Spielmans et al. (2020) state that we acknowledged this limitation.

In the limitations section we further state that in certain number of the studies analyzed, "The principal investigators were also proponents of EFT, a source of potential bias."

That researcher and therapist allegiances were not controlled in every study is clearly stated in our paper and does not affect our conclusions.

f) Selection of included studies may have been biased

Spielmans et al. (2020) state that a weakness of our meta-analysis is that the sample sizes in the studies analyzed were small. The standards of the APA Division 12 Task Force on Empirically Validated Treatments, used for determining which studies were included in our primary analysis, require a sample size sufficient to "detect statistically significant effects" ($p < 0.05$ or better; Chambless & Hollon, 1996). This criterion was met for those studies included in the meta-analysis.

Spielmanns et al. (2020) fail to state what sample size might have been satisfactory to them, so it is impossible to determine the magnitude of that number. This points to the value of clear published standards such as those of the APA Division 12 Task Force, which provide consistent criteria against which to measure studies.

Another misconception present in the Spielmanns et al. (2020) paper, as well as much journalism and lay understanding of science, is the conflation of the size of the statistical effect with the number of participants in a study. They state that: "...it is dangerous at best to draw conclusions based on three studies across a total of 89 participants."

In the media, studies with large *N*s, such as epidemiological studies with tens of thousands of participants, are frequently referred to as "large," even if the observed effects are statistically small. One of many such required an *N* of 61,433 followed over 18 years to find 156 cases of stomach cancer (Larsson, Bergkvist, & Wolk, 2006). The reason that such large populations are required for analysis is because the effect size is tiny.

When the effect of a change is large, only a small number of participants are required to demonstrate it. An example is a hypothetical study asking the research question of whether amputation of a leg in diabetics with peripheral neuropathy results in a leg length discrepancy. Assessing fewer than 10 participants, very high statistical significance is obtained. Large *N*s are not necessary to demonstrate an effect if the size of that effect is large.

This is why the APA Division 12 standards used for quality control in our meta-analysis use the criterion of "statistically significant effects," and not absolute numbers of participants. The authors of the APA Division 12 standards do not regard conclusions drawn from such analyses as "dangerous" (Chambless & Hollon, 1996); they in fact regard them as grounds for empirical validation of a therapy.

The possibility of publication bias raised by Spielmanns et al. (2020) is a widespread problem in science. We address it in the Discussion section of our meta-analysis, along with a brief description of our thorough search for "file drawer studies" or "grey literature" that might have skewed the results.

While publication bias is a legitimate concern in any field of investigation, it can at best be acknowledged and addressed. Our meta-analysis is no more able to solve this universal scientific quandary than any other study, and it does not affect our conclusions.

Data Analysis

Under the heading "Flawed Data Analysis," Spielmanns et al. (2020) attempt to replicate the calculations made in our meta-analysis.

As stated above, an independent statistician re-evaluated our original results. Table 3 above shows positive effect sizes associated with the full EFT + tapping regimen ranging from small (2 outcomes: $ES = 0.33, 0.35$), to medium (2 outcomes: $ES = 0.52, 0.56$), to large (2 outcomes: $ES = 1.13, 1.71$). Considering the effect sizes obtained from the pre- to follow-up assessments for all 6 outcomes evaluated, the cumulative fixed effects Hedge's *g*-value is 0.73, 95% confidence interval: 0.42, 1.04, $p < 0.0001$. The corresponding random effects Hedge's *g*-value is 0.74, 95% confidence interval: 0.34, 1.13, $p < 0.0001$.

Although a moot point, it is unclear as to how Spielmans et al. (2020) attempted to replicate our original meta-analysis by using data from our original table 4 (which does not include means or standard deviations), and/or by using data from our original table 3 (which included a few incorrect entries from the source publications of the three component studies). While they provide effect sizes in their Table 2, they do not provide the underlying detail showing the steps by which these calculations were arrived at.

Discussion and Conclusions

At the end of their paper, Spielmans et al. (2020) state that, “the current evidence base is far too weak to claim that acupoint tapping is a specific, beneficial ingredient in psychotherapy.” Over 100 clinical trials of EFT have appeared in peer-reviewed journals. Over 40 review papers and meta-analyses have been published. A database of EFT research is available at Research.EFTuniverse.com.

We have been able to identify only two other therapies for which there are more than the six dismantling or component studies we found for EFT; these are CBT and EMDR (Jeffries & Davis 2013; Kaczurkin & Foa 2015). The breadth and extent of the published literature for EFT, including isolating the relative contribution made by its various components, provides an evidence base matched by only a few other therapies.

Given that Spielmans et al. (2020) believe that this evidence base is “far too weak,” it would again be helpful to readers of their critique to discover what number of studies would in fact satisfy them. When clear criteria are proposed, the discussion of a reasonable standard of proof becomes possible. When criticism of research is made, without the suggestion of any reasonable evidentiary standard, no benchmark is provided for the definitive evaluation of a therapy.

We welcome the process of scientific review, and appreciate the need for rigorous analysis. We are grateful to Spielmans et al. (2020) for the opportunity to elaborate our explanations of our methodology and extend our observations.

At the same time, it is worth remembering that the purpose of psychological and medical research is to identify therapies that relieve human suffering. Few studies are perfect, and analyses which provide reasonable guidance to clinicians are aligned with that aim.

In conclusion, despite computational errors in our original publication, the present revised analysis supports the original conclusion that the acupressure component of the EFT protocol is an active ingredient that contributes to favourable health effects, particularly when evaluated at follow-up assessment beyond the initial pre- and post-assessment time period. Our conclusions are consistent with each of the six individual studies selected for analysis, and with the extensive literature on the efficacy of EFT as a whole.

Table 3 (Revised). Effect Sizes for EFT + Tapping vs. EFT + Control for Those Studies Meeting APA Criteria

Author (Date)	Measure	Condition	N*	Pretest Mean (SD)	Posttest Mean (SD)	Follow-up Mean (SD)	Pre to Post**		Pre to Follow-up**	
							Hedge's <i>g</i>	95% C.I.	Hedge's <i>g</i>	95% C.I.
Church & Nelms (2016)	Anxiety	EFT + tapping	16	58 (9.0)	52 (7.0)	51 (6.0)	0.07	-0.59, 0.72	1.13	0.40, 1.83
		EFT w/DB	18	55 (7.0)	50 (6.0)	56 (7.0)				
	PST	EFT + tapping	16	56 (10.0)	50 (9.0)	50 (8.0)	-0.13	-0.79, 0.53	0.56	-0.11, 1.23
		EFT w/DB	18	54 (8.0)	47 (8.0)	53 (9.0)				
Fox (2013)	Anxiety	EFT + tapping	10	2.63 (0.66)	-----	2.34 (0.61)	-----	-----	0.35	-0.50, 1.20
		EFT, no tapping	10	2.71 (0.69)	-----	2.73 (0.96)				
	Negative Emotion	EFT + tapping	10	2.35 (0.49)	-----	2.04 (0.42)	-----	-----	0.52	-0.34, 1.38
		EFT, no tapping	10	2.33 (0.63)	-----	2.38 (0.85)				
Wells et al. (2003)	BAT	EFT + tapping	18	4.5 (2.5)	6.8 (1.7)	6.7 (1.6)	0.65	-0.01, 1.32	1.71	0.71, 2.71
		EFT w/DB	17	5.6 (2.5)	6.4 (1.8)	6.2 (1.6)				
	Fear	EFT + tapping	18	31.9 (7.3)	15.1 (9.7)	22.8 (7.5)	0.83	0.15, 1.51	0.33	-0.51, 1.17
		EFT w/DB	17	32.2 (10.6)	25.2 (12.2)	26.8 (15.6)				

*For Wells et al. (2003), the respective sample sizes for EFT/DB at 6-month follow-up were 12 and 9, respectively. With this reduced sample size, pre-test means (SD) were as follows: BAT: EFT + tapping: 4.2 (2.7); EFT w/DB: 6.1 (2.6); Fear: EFT + tapping: 30.9 (8.3); EFT w/DB: 30.9 (11.6). **Comparisons are full EFT condition versus control conditions. BAT: Behavioral Approach Task (BAT); PST: Positive symptom total; DB: Diaphragmatic breathing.

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