

## About Acupuncture and Electroacupuncture

To the Editor:

I read carefully the outstanding article “Mechanisms of Acupuncture-Electroacupuncture on Persistent Pain” by Zhang *et al.*,<sup>1</sup> published in this journal in February. It is very detailed and shows several mechanisms of action of acupuncture and electroacupuncture: peripheral, spinal, supraspinal, and central and also their relations with bioactive chemicals involved in attenuation or control of pain, such as opioids, serotonin, norepinephrine, amino acids, cytokines among others. The article is really a lesson in acupuncture.

However, being an acupuncturist doctor for the past 27 yr and very curious about the real difference between manual acupuncture and electroacupuncture, I feel obliged to disagree with the author’s statement in page 489: “On the basis of that evidence, we hypothesize that electroacupuncture is superior to manual acupuncture...” although I understand that they do minimize the statement by complementing “... but further investigation is warranted to confirm this premise.”

Most of the articles the author cited to confirm this statement do not really compare real manual acupuncture with real electroacupuncture to hypothesize this fact. Only Schliessbach *et al.*<sup>2</sup> compared both procedures in 45 healthy volunteers to assess Pressure Pain Detection Thresholds. These authors state that electroacupuncture produces a higher Pressure Pain Detection Thresholds elevation than does manual acupuncture during needle application, but by the time of needle withdrawal, the two stimulation modalities no longer differ significantly.

In Berman *et al.*<sup>3</sup> and Vas *et al.*,<sup>4</sup> the authors compare electroacupuncture *versus* nonpenetrating needles connected with a mock transeletrical stimulation. Sangdee *et al.*<sup>5</sup> compare real electroacupuncture *versus* patch electrodes connected to dummy mode of stimulation (and not real manual acupuncture). Also, Mavrommatis *et al.*<sup>6</sup> compared real electroacupuncture plus drug *versus* retractable needles and simulated electrostimulation plus drug *versus* drug alone.

When Zhang *et al.*<sup>1</sup> cite Scharf *et al.*<sup>7</sup> as an example where manual acupuncture could not differ to sham, I must remember that “minimal acupuncture” and “non-classical points” are no longer examples of inert therapies, as observed by others.<sup>8,9</sup>

So, regarding the importance of the article by Zhang *et al.*,<sup>1</sup> there is no answer, at the moment, for the question: “is electroacupuncture better than manual acupuncture”? There is a tendency, among modern acupuncturists, to consider this a truism, but there is no scientific evidence to back up this statement. More research is necessary, specially designed to respond that question.

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## Competing Interests

The author declares no competing interests.

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## In Reply:

We thank Dr. Silva for his comments on the relative effects of electroacupuncture and manual acupuncture.<sup>1</sup> We agree that, while there is no conclusive evidence to show that electroacupuncture is superior to manual in pain management, scattered evidence suggests that the former might be superior to the latter. In a study<sup>2</sup> with 20 patients, electroacupuncture was superior to manual acupuncture in relieving pain in patients with tennis elbow. In Dr. Manheimer’s<sup>3</sup> meta-analysis of 16 trials of acupuncture

for osteoarthritis, sensitivity analysis suggested that electroacupuncture might be associated with better outcomes. Furthermore, indirect comparison between electroacupuncture and manual acupuncture also indicates the same tendency. For example, in a study by Dr. Berman *et al.*,<sup>4</sup> both electroacupuncture and manual acupuncture significantly relieved knee osteoarthritis pain between weeks 14 and 26 compared to needle insertion at sham points and nonpenetrating mock electrical stimulation, whereas in a study by Dr. Witt *et al.*<sup>5</sup> manual acupuncture significantly improved pain at 8 weeks but not 26 weeks compared to superficial needling at nonacupuncture points. Although these data are preliminary, they suggest that electroacupuncture might be more effective than manual acupuncture for managing pain. However, more studies that directly compare the effects of these types of acupuncture on pain, and take into consideration pain severity, acupuncture point location (local *vs.* distant), treatment “dosage,” and follow-up period, are necessary.

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### Competing Interests

The authors declare no competing interests.

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## The Potency of Different Propofol Formulations

*To the Editor:*

With great interest I read the study by Le Guen *et al.*<sup>1</sup> on the comparison of the potency of different propofol formulations that was published in February issue of *ANESTHESIOLOGY*. The authors compared the dose of Diprivan® (AstraZeneca, Cheshire, United Kingdom), Propoven® (Fresenius-Kabi AG, Bad Homburg, Germany), and Lipuro® (B-Braun, Melshungen AG, Germany) alone or in combination with lidocaine, which was necessary to achieve induction of general anesthesia, measured by a bispectral index (BIS)–controlled closed-loop system. I have, however, some concerns about the methodology that may undermine the clinical validity of the authors’ conclusions.

The most reliable way to compare pharmacologic potency of different drug formulations is a crossover study with healthy individuals either in a single center or with unified laboratory assessments. Otherwise, interindividual variations in pharmacodynamics might reduce validity of the findings substantially. The authors themselves criticize other studies for “ignoring high interindividual variability of the dose–effect relationship.” Yet, they chose to conduct a multicenter study and included patients ranging from American Society of Anesthesiologists I to III. The resulting interindividual variability in both BIS and propofol sensitivity are confounding factors that influence the closed-loop system. Another point of concern is the dose measurement in multiple centers, which also suffers from a very low sample size. Especially Propoven® with saline was measured only in four patients. Any results based on this sample size are prone to high statistical variability.

Concerning data handling, the authors do not report whether data from patients who had not reached induction at 360 s (which can be seen in figure 2 of the original article) were used for the analysis. Because the primary study outcome was “the dose of propofol given alone or associated with lidocaine until the moment of induction,” this information seems quite relevant.

A patient’s BIS is prone to artifacts,<sup>2,3</sup> and those can directly influence propofol dose administered by a BIS-controlled closed-loop system. Notably, “gentle manual assistance if SpO<sub>2</sub> decreased below 92%” as described in the methods will influence the BIS, and the authors did not state how often and in which group this measure was applied. In addition, since pain delays the time until induction, the effects of formulation potency and pain-induced induction delay cannot be separated in the analysis. In conclusion, I am not convinced that the data presented by Le Guen *et al.*<sup>1</sup> demonstrate clinically relevant differences in potency between propofol formulations.

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