

Effect of Vibration Anesthesia on Injection-Related Pain: A Prospective Crossover Study [Response to Letter]

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Dear editor

We thank Drs. Xu and Gao for their thoughtful review of our article, “Effect of Vibration Anesthesia on Injection-Related Pain: A Prospective Crossover Study” and welcome the opportunity to respond.

First, we agree that larger randomized crossover designs are generally preferred. However, we believe the substantial reduction in pain observed across multiple injection types supports the potential clinical utility of vibration anesthesia. Our study was intentionally designed as a pragmatic proof-of-concept investigation intended to evaluate whether vibration anesthesia could meaningfully reduce pain during common needle-based procedures. All participants received the control condition first, followed by the DigiVibe-assisted procedure, to avoid potential contamination of baseline pain assessments by prior exposure to vibration stimulation.

As with any non-randomized crossover design, expectancy, learning, and period effects cannot be completely excluded. However, we note that the magnitude and consistency of the observed reductions were substantial across all procedure types, including subcutaneous injections where every participant reported lower pain scores with DigiVibe. While a randomized sequence may further strengthen future studies, we believe the observed effect sizes are unlikely to be explained solely by order effects.

Regarding carryover effects, the procedures evaluated were brief, minimally invasive interventions, and a five-minute interval was selected based on the transient nature of the nociceptive stimulus. Nevertheless, we agree that formal assessment of carryover effects would strengthen future investigations.

Second, we agree that pain perception is influenced by multiple psychological and physiological factors, including anxiety and expectation. However, the primary objective of this study was to evaluate whether vibration anesthesia improves the patient-reported pain experience during needle-based procedures, rather than determine the specific mechanisms responsible for any observed benefit. The enrollment criterion of self-reported needle fear was intentionally selected to reflect a clinically relevant population that commonly experiences heightened discomfort during injections. From a patient-centered perspective, reduction in perceived pain remains clinically meaningful regardless of whether the effect is mediated through nociceptive modulation, reduced anxiety, distraction, or a combination of these mechanisms. Although anxiety was not formally measured, participants nevertheless reported substantially lower pain scores when vibration anesthesia was used.

Third, the needle-gauge subgroup analyses were intended as exploratory observations and not as definitive evidence of differential efficacy across needle sizes. We agree that formal interaction testing would be required to establish whether treatment effects differ significantly between gauge groups.

With respect to the reported rank-biserial correlation coefficients, zero-difference pairs were handled according to standard Wilcoxon signed-rank procedures and were excluded from the ranking process.^{1,2} For non-zero differences, observations were ranked based on the absolute magnitude of the paired difference, and tied absolute differences were

assigned the average of the corresponding ranks. The rank-biserial correlation coefficients were then derived from the resulting positive and negative rank sums. We agree that including additional details regarding the treatment of ties and zero differences would have improved transparency and reproducibility.

We also thank the authors for identifying the typographical error in Table 1. The reported mean height of 308 inches is incorrect and resulted from a transcription error during manuscript preparation. The correct value for mean height is 65.2 inches. This error does not affect any statistical analyses, study outcomes, or conclusions.

Fourth, although the consistency of the observed response is notable, we do not believe it is unexpected given the nature of the study population and intervention. Participants were specifically selected based on self-reported needle aversion, and the crossover design allowed each participant to serve as their own control, minimizing inter-subject variability.

Finally, we agree that vaccine hesitancy, healthcare utilization, and treatment adherence were not directly measured in this study. Our discussion intended to highlight potential clinical implications based on the established relationship between needle-related pain and avoidance behaviors reported in the literature,^{3–5} rather than claim that these outcomes were demonstrated by our trial.

In conclusion, we thank Drs. Xu and Gao for their constructive comments. We agree that randomized studies incorporating sequence randomization and larger populations would further strengthen the evidence base. At the same time, the results of the present study demonstrated statistically significant reductions in patient-reported pain during fingersticks, subcutaneous injections, and intramuscular injections when DigiVibe was used, with no safety concerns identified.

Disclosure

The authors report no conflicts of interest in this communication.

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