

Letter to the Editor: Electromagnetic Hypersensitivity

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McCarty et al. [1] present the results from two experiments which assessed whether exposure to an electric field could trigger symptoms in a participant who reported having “electromagnetic hypersensitivity.” For reasons that are not stated, the authors used several ways of categorizing these symptoms. First, they report that the participant herself described her symptoms as present or absent, and that she also sometimes used the qualifiers “mild” or “strong.” The authors then grouped some of these categories together, in order to analyze the symptoms according to whether they were “none,” “mild,” or “more than mild.” However, it appears to have been the participant’s decision to use these descriptors; the authors do not seem to have instructed her in advance to use any predetermined method to rate her symptoms. As such, was using this rating system in this manner in the original analytic strategy or was it decided post hoc?

Elsewhere in the text, the authors note that “the subject consistently reported pronounced symptoms that occurred during the field intervals, particularly in intervals 7, 13, 14, 15 and 18.” These five intervals do not correspond with the “mild/unqualified/strong” categories used by the participant (as presented in Table 3a). It is unclear what “pronounced” means in this context, why the authors feel that this was particularly true for those five intervals, why this does not map on onto the participant’s method for rating her symptoms, or why this additional way of categorizing the symptoms was not analyzed.

The use of multiple ways to grade the participant’s symptoms is perplexing. Had the authors restricted themselves to analyzing symptoms as merely “present” or “absent,” a different set of results would have emerged. Using Fisher’s exact test with these data, a significant effect would have been observed for the first experiment comparing symptom presence in pulsed versus sham conditions, but only at $p = .03$. In the second

experiment comparing symptoms in sham, continuous, and pulsed conditions, this effect would not have been replicated ($p = .07$).

Care is required in evaluating case studies in this field. Studies in which participants do not respond to stimuli that are conventionally thought to be harmless are difficult to publish, leading to publication bias. In some cases, participants have been found to produce impressive results on first testing, but are unable to replicate their performance when tested further (e.g., [2]). For McCarty et al., while the participant may well warrant further investigation, we question whether the results reported are sufficiently compelling to prove the case they advance. It is also plausible that they reflect a statistical artifact resulting from the way the data were analyzed. We would urge the authors, and the participant, to seek independent replication of their results.

In conclusion, while the results of McCarty et al. are intriguing, more research is necessary before we should revise the conclusions of the recent systematic review which considered all controlled trials in this literature and failed to find sufficient evidence to support the concept of “electromagnetic hypersensitivity” [3].

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