International Journal of Neuroscience, Early Online, 1–2, 2012
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ISSN: 0020-7454 print / 1543-5245 online
DOI: 10.3109/00207454.2011.648764

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Response to Letter to the Editor Concerning "Electromagnetic Hypersensitivity: Evidence for a Novel Neurological Syndrome"

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The authors' letter provided us an opportunity to highlight elements of our work that we believe are essential to understanding its importance. We followed an empirical approach and demonstrated a cause–effect relationship ($p < 0.05$) under conditions that permitted us to infer the existence of electromagnetic hypersensitivity (EHS), a novel neurological syndrome. Simply put, we asked the straightforward question: "Can exposure to an electromagnetic field (EMF) produce human disease?" We employed a research design that allowed the outcome of interest (development of physical symptoms) to be separated from the most likely source of bias (the subject's own awareness of the presence or absence of EMFs), and we answered the question using scientifically sound methodology.

The important issue at this point is not *whether* EMF can produce symptoms (we empirically demonstrated that it can) but rather why this effect historically has been difficult to detect. It occurred to us that EHS had remained elusive because of the way it was studied. The experiments designed to detect EHS had been based on the assumption that if it existed, it was a linear phenomenon, whereas EHS is actually a nonlinear phenomenon. For example, the experiments performed and/or reviewed by the authors all employed linear experimental and statistical designs [1–4], which are known to be inefficient for detecting nonlinear deterministic activity. If EMFs created disease in precisely the same way with every person (e.g., induction of migraine headaches), then a cause–effect relationship between EMFs and disease would easily be detectable using linear methods. The tradeoff for capitalizing on the sensitivity of linear methods is the likelihood of a false-negative result if the determinism in the system under study is nonlinear. The explanation that the studies [1–4] were negative because their designs were based on a dynamically incorrect model had not been considered prior to our work. Our study was designed to detect whether EHS was a linear or a nonlinear phenomenon, and we were successful in showing a link between acute EMF exposure and somatic responses ($p < 0.05$). This finding—taken together with the unfailingly negative results of the linear studies—is good evidence that EHS is a nonlinear phenomenon, as we suspected.

Historically, the failure to recognize the dynamical complexity of EHS prompted researchers to conclude that the best way to explain the phenomenon was to blame it on a neurotic character flaw of the individuals who suffered from it [4]. In contrast, we recognized the dynamical complexity of EHS and designed our study accordingly. In response to EMF presentation, our subject developed various symptoms, including headache, muscle twitches, a sense of unease, and palpitations. The symptoms were comparable to those the subject described as arising from environmental EMFs, were related to EMF exposure in a pattern unlikely to be due to chance ($p < 0.05$), and were unrelated to the subject's own impression of whether the EMFs were present or absent. In short, we proved in a scientific fashion that EMFs caused disease in this subject and did so by means of a process governed by nonlinear laws. We believe that future researchers will need to tackle the problem of EHS with these findings in mind.

The authors described our protocol as a "case report." We would like to correct this misstatement by underscoring the point that our paper described a scientific trial, using the subject as her own control, to test the straightforward question of whether EMFs could cause disease. They also raised several minor points.

Received 8 December 2011.

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They objected to our use of "pronounced;" it meant "symptoms greater than mild," which we feel is clear from the context. Second, we find the concerns raised regarding "0.03" vs. "0.07" to be irrelevant for two reasons. First, this issue derives from arbitrary changes made in the structure of our data. This sort of post-hoc analysis with various iterations of the data would be expected to produce results with a variable—yet still comparable—range of p values. Further, in larger context, a 93% chance of a real correlation between EMFs and disease (assuming $p = 0.07$) and a 97% chance (assuming $p = 0.03$) have materially identical implications—that future EHS studies should allow for the possibility of a nonlinear relation between EMFs and somatic responses.

The third issue raised was our decision to allow the subject to describe her symptoms in her own words. We maintain that a patient-centered approach is essential for understanding a nonlinear disease system. For example, a mild electric current causes local pain (symptom) only while the current is applied (stimulus), with symptoms ceasing momentarily upon discontinuation of the current. This exactly reproducible pattern is characteristic of a linear system. In contrast, the symptoms of EHS vary in terms of physical location in the body, may linger after the stimulus is turned off, appear to depend not only upon the field strength but also upon changes of the field ("pulse" vs. "continuous"), and are quite likely to be amplified by other factors, including the subject's emotional response to suffering. Studying a stimulus–response relationship in such a system requires careful control of multiple elements and a more nuanced approach when determining the presence of a response. For these reasons, we chose to evaluate the symptom severity spectrum based on a method that would—for most investigators—be easy to reproduce following the review of the subject's descriptions ("none", "mild", and "more than mild"), believing we might find a correlation between more severe symptoms and certain types of EMF exposure. We did.

Finally, the authors have been well funded by sources for whom general acceptance of an association between environmental EMFs and human disease would be financially disadvantageous. This coupled with their consistent record of pursuing predictably negative results by using a linear model for EHS creates at least the appearance of an important financial conflict of interest.

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