

## Peer Review My Foot!

Dear Editor,

We received your letter and the comments of the reviewers concerning our paper "Effects of Sunspot and Solar Wind Activity on the Periodicity of Human and Animal Rhythms." We thank the reviewers for their attempts to understand it. Unfortunately, they failed.

We write in the hope that making our experience public can serve as a cautionary tale for those among your readers who are trying to do truly original work. The benefits of "peer review" are entirely dependent on the presumption that the reviewers are, in fact, peers—expert in the work under consideration. Our work, unfortunately, lies outside of the scientific realms inhabited by your ignorant and narrow-minded reviewers. One of them clearly doesn't even know what "stochastic" really means.

Although our paper was severely criticized by the reviewers, no two reviewers had the same criticisms. One found it "imaginative but unsubstantiated." The second, contradicting the first, claimed it was "unoriginal, pedestrian, plodding, and pointless." He mentioned, without saying why, that the work was eerily similar to that presented in a talk he'd recently heard in Novosibirsk. He declaimed that what was "new in the paper wasn't true, and what was true had been anticipated by Aschoff and Pittendrigh." What nonsense! Aschoff and Pittendrigh never addressed the implications of biological rhythms and photoperiodism for Mars colonization. The reviewer suggested we submit the paper to "a more specialized journal." However, yours *is* the more specialized journal; we submitted it to you on the advice of the editor of a *less* specialized journal. The third reviewer's main requirement was that we redo our analyses using the Student-Newman-Keuls test instead of the Bonferroni test. Only the third reviewer revealed any appreciation for the work's importance. He recognized it as "an unusual approach to previously unconsidered questions of some potential interest" but wondered

"Where will it lead?" It is, of course, difficult to say. It may, however, have immense implications for the future. My colleague, Professor Nikolai Ivanovich Lobachevsky, the renowned author of "Understanding Biological Rhythms: What Goes Around, Comes Around," deserves the credit. He has repeatedly steered members of the Chelm Institute in important new directions. These have, I might add, all led to new insights and practical applications, especially in footwear and floor coverings.

You said "two of the three reviewers found substantive deficiencies in rationale, design, validation of results, interpretation, and presentation." You hinted that if we revised the manuscript so as to fully satisfy the reviewers—change this, change that, change everything—then maybe, just *maybe*, you'd reconsider and send it back to the same sclerotic reviewers who mauled it in the first place. Satisfy the reviewers, indeed! If two out of three reviewers suggest something, is that the yardstick? Should majority rule reign in science? Why did the third reviewer not suggest it? Are all reviewers' opinions to be given equal weight? And always more than the authors'? How many scientists of the day thought that what Madame Curie was doing with her pot of pitchblende would lead anywhere? Have not authors the right to publish their results, especially when obtained barehanded in winter in Murmansk? Isn't it your duty as editor to recognize and support novel and original work and to foster its acceptance rather than merely reinforce prevailing opinion?

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### *Editor's response:*

Thank you for your letter. I knew Madame Curie. Marie Curie was a friend of mine (just a friend). You're no Madame Curie. As for Professor Lobachevsky: I don't know.