

**Walther Replies:** The Comment [1] addresses a possible correlation of the solar neutrino flux as measured by the Homestake experiment and the Mount Wilson magnetograph data. My paper [2] examines the correlation of the neutrino flux with the sunspot number as most of the published work on the neutrino-solar cycle correlation refers to these two data sets; see the references in [2,3]. However, it is pointed out in [2] that similar problems exist with correlation studies involving other indicators of solar activity. In addition, the statistical analyses in [4–6], which serve as a basis for the Comment, raise another issue that is related to the one described in [2]. I will briefly describe the bearing of these two issues on the analyses in the Comment and in [4–6].

The highly significant results given in [6] and referred to in the Comment were obtained by optimizing the Spearman correlation statistic over an unknown time delay. While it is physically appropriate to consider such a time delay, it is clear (and can easily be confirmed by simulations) that such a procedure will produce significant estimates that are quite exaggerated. In fact, the problem with this procedure becomes already visible by looking at Fig. 2 in [6]. Besides the highly significant correlation for a time delay of about 1.4 yr, one sees a significant correlation (at the 1% level) for *negative* time delays of about  $-0.5$  and  $-0.8$  yr. The resulting implication of a surface magnetic response that precedes processes in the solar interior does not make any physical sense. The authors acknowledged in [6] that this procedure might give exaggerated results and tried to improve upon this method by using a certain centroid of the results instead. However, there seems to be no mathematical justification for this new procedure: Using simulations as in [2] one finds the same problems as described there. One also needs to keep in mind that the range of time delays in Fig. 2 is apparently arbitrary. It is clear by looking at the figure that increasing the range will make the centroid results less significant.

I certainly agree with the authors that suggestive trends and speculations need to be made known for science to advance. But any further claim that the effect does really exist must be supported by evidence. The regression figures for various latitudes in the Comment are provided without any estimates of how likely it is that these outcomes could be due to chance. References [4–6] do provide significance estimates, but they are overstated as explained in [2] and in the previous paragraph; this was acknowledged by the authors in the Comment. Devising an appropriate statistical procedure for this problem seems not straightforward, but would be helpful beyond the problem of properly assessing the evidence for the speculative trend described in the Comment.

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Guenter Walther\*

Department of Statistics  
390 Serra Mall  
Stanford University  
Stanford, California 94305

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\*Email address: walther@stat.stanford.edu

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