Reply to "Comment on 'Performance of different synchronization measures in real data: A case study on electroencephalographic signals' "

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We agree with the Comment by Duckrow and Albano [Phys. Rev. E **67**, 063901 (2003)] that mutual information, estimated with an optimized algorithm, can be a useful tool for studying synchronization in real data. However, we point out that the improvement they found is mainly due to an interesting but nonstandard embedding technique used, and not so much due to the algorithm used for the estimation of mutual information itself. We also address the issue of stationarity of electroencephalographic (EEG) data.

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In the past years, several synchronization measures have been proposed. The objective of our previous study [1] was to contrast their performance in real datasets and compare them to standard approaches. In spite of their different definitions and implementation details, all synchronization measures showed qualitatively similar results, difficult to be guessed beforehand by visual inspection of the data. The only measure that did not agree with the others was mutual information (MI). Furthermore, nonlinear measures had a larger sensitivity in comparison with the linear ones.

The authors of the preceding Comment [2] reanalyzed the data presented in Ref. [1] using an estimation of MI based on the Fraser-Swinney algorithm [3]. In contrast with our previous results, they found MI to rank the three datasets consistently with the other synchronization measures. The main problem in our previous study [1] was that MI was not robust, i.e., its results depended strongly on parameters such as embedding dimension, time delay, and resolution in amplitude space. Depending on these parameters, the ranking changed. For the most plausible choices, the ranking disagreed with the other methods. Our conclusion was that this is due to the fact that the datasets were short, and the state space was very sparsely sampled for high embedding dimensions. Therefore, the algorithm was more sensitive to random fluctuations than to real structures in the data.

In order to test the dependence on embedding parameters and resolution explicitly, we had used in Ref. [1] an algorithm of correlation type, where the number of neighbors is counted for fixed neighborhood sizes. Instead, the authors of [2] used the Fraser-Swinney algorithm Ref. [3] which uses an adaptive binning, where bins are recursively subdivided until they are populated uniformly or until each two-dimensional bin contains $O(1)$ points.

Besides using the Fraser-Swinney algorithm, the authors of the comment propose a nonstandard, but indeed interesting embedding technique. From a d-dimensional delay vector, they produce a scalar by "interleaving" the binary digits [2]. Together with the adaptive binning, this implies that for high embedding dimensions only the first few components of the delay vector are relevant, and of these components only one binary digit. Thus, the convergence of the results of Ref. [2] for $d \to \infty$ is trivial. Moreover, the troublesome regions of small δ of Fig. 8 in Ref. [1] cannot be reached,[1] and in all cases example B shows the highest MI, as expected. Such an embedding constitutes the main advantage of the estimation proposed in Ref. [2]. We verified this by using the embedding of Ref. [2] together with the fixed distance correlation method of Ref. [1]. The results are shown in Fig. 1 where we see a pattern very similar to the one shown in Fig. 2 of [2].

Indeed, we believe now that neither the Fraser-Swinney algorithm nor the correlation method of [1] is optimal for estimating MI. The most precise method seems to be the one

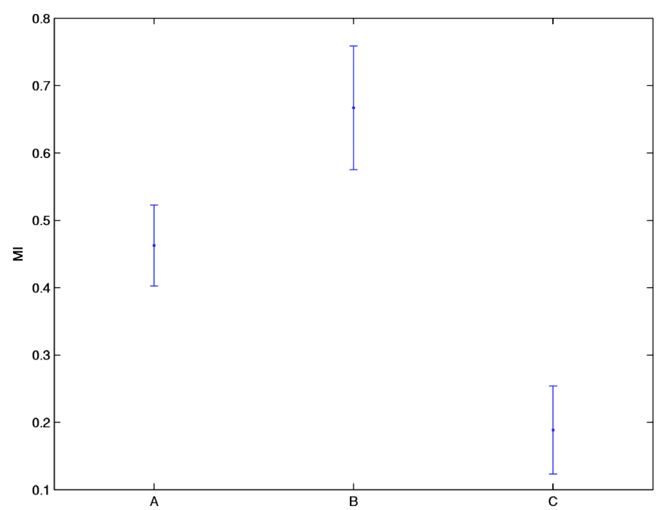

FIG. 1. Grand averages ± 1 standard deviation for mutual information for examples A, B, and C. The averaging is done over all embedding dimensions from 1 to 10 and embedding delays from 1 to 30. For all embeddings, we use 512 vectors. The algorithm uses fixed neighborhood size $\delta = 0.2$.

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[1]In passing, we remark that by mistake the MI values in Fig. 8 of Ref. [1] are divided by $\ln_2 \delta$ and were calculated using the Euclidean norm rather than the correct maximum norm, but results are qualitatively the same.

based on kth nearest neighbor distances. It is adaptive, uses small datasets in an optimal way, has small finite size corrections, and is simpler to implement than the Fraser-Swinney algorithm. Finally, it is easy to be extended to higher dimensions. Details will be presented elsewhere.

As a second issue, we agree with the comments on nonstationarity of EEG data and the limitations that it imposes in their analysis. Nonstationarity in the synchronization patterns is very difficult to estimate beforehand. Indeed, stationarity of the individual data sets does not guarantee that the synchronization pattern will also be stationary. The nonstationarity pointed out in Ref. [2] (Fig. 1) was also seen in Ref. [4], where the same data were analyzed with a technique (''event synchronization'') that is geared at high time resolution. But on the other hand, this nonstationarity also means that it is difficult to compare in detail the analysis of Ref. [1] (which used the entire time series of 1000 points) with that of Ref. [2] which used only the first 512 delay vectors.

In summary, we agree with the authors of Ref. [2] that MI, estimated from an optimized algorithm, can be a very useful tool in studying synchronization phenomena.

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