Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC2
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Comment on gmd-2022-106

Anonymous Referee #2

Referee comment on "Empirical Assessment of Normalized Information Flow for Quantifying Causal Contributions" by Chin-Hsien Cheng and Simon Redfern, Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2022-106-RC2, 2022

The paper is sort of a continuation of the methodological work by Liang about information flow. Is starts with the concept of causal sensitivity, based on partial derivatives of an effect variable Y as dependent on a causal variable X, time, and other variables, and also potentially containing noise.

Full comprehension of the framework is not warranted without referring to Liang's papers, and the reader is left alone why the ratio of a partial derivative of Y to the total time derivative of X should bear the word "causal". Also, notation-wise, the total derivative of Y with respect to time would be a sum of several partial derivatives of Y wrt all variables (X1, X2, ...) times the total derivative of them:

dY/dt = partial Y / partial X1 * dX1/dt + partial Y / partial X2 + ... + partial Y / partial t

so it one wants to single out the part which is due to X1 (say), eq. (1) in line 81 would simply be

nCS = | partial Y / partial X1 | - or otherwise, the reviewer doesn't understand the notation \partial Y(X)) / partial t

but the main problem is why this would be called "causal" sensitivity?

Also, in applications in the Earth Sciences where time series of observations are available, how do you calculate the partial derivatives from them if you are ignorant of the underlying processes?

The connection to IF is presented only indirectly - by stating the hypothesis that nCS is approximately equal to |nIF|. If there is no alternative way to calculate nIF, how would you be able to test this hypothesis? Where is the independent definition of nIF ?

Later, one special case is considered - linear models (very unlikely to work for complex systems) where IF has a representation through covariance values. In addition, it seems that the authors believe in a decomposition of the normalization factor into a simple sum of IF, a noise component and a "self-dependence" term. Why should it be that simple? And how would you be able to discern the three terms, given only the time series of X and Y? The surprising answer is in eq. (8) to (10) which show that the self-dependence of Y is dependent on X, and the noise term (contribution of other variables) is also dependent on X. How is that possible? What are the assumptions about the phase space structure, stationarity etc. which go into that?

The reviewer also notes that md3 = md2 whenever the sign in the absolute bracket of eq. (13) is positive, and =2 md1 - md2 in the opposite case, so in which sense is md3 anything new once you have md1 and md2?

The "empirical tests" chapter lists no less than 8 artificially generated time series ("designed mock-up data sets") without, however, providing any details. The curious reader might want to reproduce the numerical results shown in the Figures, but there is no clue how to do that. What exactly did you choose as (say) "1D example with fluctuating self-dependency noise-contribution and a sinlge causal direction"? One has to refer to the supplement (not referenced to in lines 181-190 of the manuscript) to find answers to these questions - however, also this is difficult since mathematical notation is wrong (example: what does the sum "_2 ^nt 1/nt" mean? The summation index (n) can't be the upper limit of the sum itself, and if the user has to choose nt first, i.e. nt is a constant for the sum, the latter is juts (nt -1)/nt, which hardly makes sense? Fundamentally, if you have the partial derivative of X1(Y1) explicitly given as a time-varying function, you simply can't require that the partial derivative of Y1(X1) would be exactly zero, contradicting the inverse function theorem. What is going on here?

In the previous chapter, dependence on other variables was considered as "noise", and there was the self-dependence term. But now, in l. 177, self-dependent terms are suddenly also noise, adding to the confusion.

The reviewer was fully lost when there was talk about the "1:2:3" ratio for 1D examples where X1, X2, X3, Y1, Y2 and Y3 are occurring. How is that a "1D" example?

The figures in the results section are not illuminating to the reviewer. It seems like one has to recognize a 21-units time lag from Fig. 3; however, even if one happens to know which panels have to be compared here, there are 1000 time steps shown, so the 21-units lag would only make a difference of 2.1%, you would need a magnifier to see anything here (the text in l. 187 talks about a 21% effect, which seem to indicate that there were somehow windows of length 100 analyzed each time, but this is mentioned nowhere else and is not visible in the Figures).

Eq. (20) seems to be a differential equation for Xadj, but not even the units fit here (unless both X and t are dimensionless, which wouldn't be case in any applications). Admittedly, the reviewer didn't even get the "25-75% split" mentioned in l. 249.

The only way to come thtough the material presented is by going through the (uncommented!) Matlab scripts provided as fileshare by the authors. Do you really expext this from a reviewer, let alone a "normal" reader?

As the reviewer doesn't see an easy fix to render the paper comprehensible., there are no detailed comments to the text (there would be many!). Still, there could be some interesting ideas, not the least since causal inference approaches (like Granger causality, CCM, PCMCI etc.) are quite fashionable in recent years in the Earth Sciences, and the concept of "higher order dependency" might be interesting. However, in its current form, the concepts are not communicated in a way that would render the paper acceptable for publication.