Atmos. Chem. Phys. Discuss., 12, C5781–C5784, 2012
www.atmos-chem-phys-discuss.net/12/C5781/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "A new El Niño-Southern Oscillation forecasting tool based on Southern Oscillation Index" *by* C. A. Varotsos and C. Tzanis

Anonymous Referee #2

Received and published: 15 August 2012

Reviewer 2: A new forecasting tool of the El Niño Southern Oscillation, by C. A. Varostos and C. Tzanis

General comments:

Reviewer 1 has offered an excellent discussion of this manuscript and has thereby shortened my review considerably. I strongly recommend that one read that review and take careful heed of its content before reading my review. One issue noted by Reviewer 1 is the lack of any comparison between the utility of this forecast method and any other forecast method, which is a severe flaw in this study. Generally, before any forecast methodology is implemented or even presented to the public in the weather or climate literature, that technique has been tested in real time for months or years and the forecast skill compared with that of currently used forecast techniques. Not only does

this study fail in this most basic analysis, there is not even any quantitative discussion of the expected uncertainty in the forecasts due to data quality, temporal resolution, etc. If the authors wish to be taken seriously in the forecasting community, they should assume responsibility for these "dirty details" themselves rather than leaving them to others.

The conceptual explanations given in this manuscript are sparse. The reader is referred to an expensive, hard-to-obtain (three weeks for a major institution to find a copy!) book (P. A. Varotsos, N. V. Sarlis and S. Skordis 2011) for the most important explanations, including any physical justification. The added mathematical detail in this book is welcome, but the conceptual information is scattered and, to me at least, incomplete, often referring to previous articles. In any case, the authors of this manuscript should not expect the reader of their paper to wade through earlier publications, getting bits and pieces of the methodology from each, in order to apply their method. Nowhere in this manuscript is a description of how the windowing is applied; the index i, corresponding to the window length, does not appear in any of their equations. This omission occurs even though the size of the window is paramount in their estimation of how long in advance the SOI can be predicted. If the authors cannot explain why "an increasing trend in a time series corresponds to negative ΔS and vice versa," perhaps they could, and certainly should, include more utilitarian details about the windowing.

Despite the many shortcomings of this manuscript, I am happy to see it properly discussed in ACPD. The issue is that a parallel literature on ENSO has arisen in the geoscience and physics literatures (i.e., American Met. Soc., Tellus, Q. J. Royal Met. Soc., etc. vs. Physica D, PRL, etc.), and each side often dismisses the other either for not being realistic enough, pure enough, imaginative enough, or whatever. I hope that the presence of this manuscript in the joint literature will instigate some communication between the currently disjoint sets of research. In this regard, the authors of the current manuscript are to be commended for including substantially more references to the geophysics ENSO literature than was present in their original version. Further,

novel methods of ENSO forecasting are always welcome provided they are properly scrutinized; the more promising the method, the stronger should be the scrutiny. We all live with this.

Specific Comments:

Pg. 2: El Niño is sometimes described as being quasi-periodic, even though there is little evidence for this (see Fig. 1 of Newman et al. 2009, J. Climate). In fact, there is quite a lot of evidence that sea surface temperatures in the global tropical strip, including El Niño events, are dynamically described on the seasonal timescale as a multivariate linear stochastic differential equation with a non-normal linear operator (e.g., Penland and Sardeshmukh 1995, J. Climate; Penland 1996, Physica D). At finer, i.e., monthly, resolutions, resolved nonlinearities may play some role (e.g., Kondrashov et al. 2005, J. Climate), but state-of-the-art general circulation models have only recently become competitive with linear statistical models in forecasting the pattern, amplitude and temporal evolution of tropical SSTs (Saha et al. 2006, J. Climate). These linear stochastic models do not ignore nonlinearities; the stochasticity results from rapidly varying, unresolved chaos. (Ruelle's book, Chance and Chaos, gives a delightful discussion.) Note also that not everyone ascribes to this description of El Niño, but the issue is far from being settled in favor of a nonlinear system with resolved nonlinearities.

Pg. 4: The normalized intensity p_k does have the properties of a probability (or, more specifically, a normed measure), but this probability is not to be interpreted in the frequentist sense usually ascribed to probability. A better discussion of this quantity would be very helpful to the reader (see also comments by Reviewer 1). A better explanation of p_k might also lead to a more comprehensible explanation of how to interpret ΔS physically, rather than as a simple indication of a trend, and elucidate the "intuitive" description on Pg. 9.

Pg. 5: The statement that 84 months is the longest period of El Niño is just wrong. Look again at Fig. 1 of Newman et al. (2009). A better justification for this choice might

be provided ex post facto, in that the authors' best results seemed to occur for windows of about two years.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 17443, 2012.