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Dear Professor Gadgil

I have now heard from the referees about your joint paper with Yadumani and N V Joshi entitled *Coherent rainfall zones of the Indian region*. On the basis of these reports I am prepared to accept a somewhat modified version of your paper. As you can see from the copies of the reports Referee A makes no specific comments but I am afraid that Referee B does raise a number of points in his somewhat rambling comments. I have tried to pinpoint the main concerns he has and suggest you consider these and, if necessary, comment on them in your text. It seems to me that he is totally against areal averaging and this influences his ideas. However, perhaps you could consider his points about broad versus narrow bands, time versus frequency domains, and definitions of terms. I also enclose an Agreement form which should accompany two copies of your revised paper.

Yours sincerely

Brian D Giles

Dr Brian D Giles

Review of **Coherent Rainfall Zones of the Indian Region** by Gadgil, Yadumani, and Joshi (GYJ)

Introduction

The entire paper is done in the time domain. And before commenting let me say I have nothing against this provided there is a rationale behind it. For example, for many years I worked in seismic exploration for oil. The spectrum of seismic traces which make up a seismic section are "broad band signals", that is the spectrum has a broad level of high energy over a broad band of frequencies, sometimes upwards of an octave. The center frequency of this band depends on the source of energy used and its width also often depends on the source used.

I've also done a lot of work on "narrow band signals" in various fields of geophysics, my PhD in fact. Now by definition a narrow band signal has considerable predictive value, much more so than broadband. Now whether one is working on broad band or narrow band signals one is not dealing with "random noise". And if the signals are broad band one works primarily in the time domain (deconvolution, etc).

One objection to this paper and to some of the ones it references is that the writers apparently believe rainfall has non-random components. And yet, GYJ reference a paper by Wigley et al. (1984) which plays a prominent role in their analysis. Now I have looked over this paper and Wigley et al. (1984) keep talking about "signal to noise ratio". It finally dawned on me that they regard the "trend" as a "signal" and all the natural variability as "noise". This is the position taken by the recent IPCC report to the United Nations, and it is an insane position because no one in the IEEE regards a trend as a signal (the world has been turned upside down by these people). But Wigley et al. are not as explicit as the IPCC report so I called in a colleague and without letting on what I thought Wigley meant ask him what he thought Wigley meant. He replied that Wigley et al. regarded the trends in the data as the "signal". So, I think I'm on pretty firm ground. This Wigley paper was a forerunner of what later appeared in the IPCC Report explicity, that is, all variability in climate is noise and they are searching for a Green House "signal".

Comments

Although I'd worked as a signal processing engineer for many, many years in the oil and electronic industries I never had heard of "Principle Component

in this paper and I therefore look forward to follow-up studies based on these

Analysis" until I became involved with meteorologists a few years ago. I knew this didn't have anything to do with signal processing (or I would have heard of it) and finally traced it back to a report by Edward Lorenz in 1956 at MIT. This, as Lorenz says, came out of psychology and was used by the USA Army to try and measure psychology profiles of inducted soldiers. Needless to say such data are very, very, very fuzzy. I eventually found a colleague who had used it in mineral exploration. He had taken Landsat satellite data and applied Principle Components to get some idea of lineation patterns that might be related to zones of mineralization. He jokingly said such data was so fuzzy that Landsat images were called "chicken scratches".

Now there is nothing fuzzy about monthly or annual values of air temperature or rain gauge data. The data are given to two fractional digits of accuracy. So, why is one regarding it as fuzzy? And if one does not regard the data as being completely random (and I don't think GYJ do regard it as such) then one has a signal *or signals* of some sort. And the *only way* to dig into this is to start computing spectra in the frequency domain. If it is broad band one has poor predicibility as one does in the oil industry. If narrow band one has pretty good predictibility. But GYJ never get to the frequency domain. I recently had reason to read the paper of Dyer (1975) that GYJ reference. This paper is virtually worthless. And it was not until 1978 that Dyer joined forces with P.D. Tyson and they found South African rainfall data had narrow band signals in it by *using spectrum analysis.* The subsequent work is spelled out in Chapter 3 of Tyson (1986).

Another paper GYJ reference is Gregory (1989). In this regard I reviewed a paper by Mitra and Dutta (1991) some months ago where they analyzed the ten regional time series published by Gregory (1989) and found two narrow band signals in them. I naturally recommended publication because I had analyzed these 10 series in 1989 and also found the two signals. Using regional data is poor practice as can be seen by reading Currie and O'Brien (1992) and the many papers they reference. But, at least Mitra and Dutta (1991) had elucidated a very important fact. The variability in these India regional series is narrow band, not broad band. This was important, but not important enough for me to publish because one should analyze each record individually, and the India data on an individual basis were not available. In contrast, the Gregory (1989) paper was not useful except for the 10 regional series published which led Mitra and Dutta (1992) to important results.

Summary

The GYJ paper is heavily weighted in two directions. The first is along the lines of Wigley et al. (1984) and this paper is a discussion on how trends are signals. This is one of the more worthless papers ever published and I don't think GYJ realize that they are really being had here. Wigley considers all natural variability in climate data to be "random noise" and he is after the Green House "signal", a trend. I don't think GYJ consider all natural variability random noise (see **Final Remarks**. And so I'm writing this review in a kindly manner)

The second focus is on dividing India into sectors. They apparently think variation in monsoon rainfall is characterized by two spatial scales, the scale of 10 zones devised by Gregory (1989) and another of 30 zones found by them. But as said earlier the only useful thing to come out of the Gregory (1989) paper are the 10 regional series published by him which resulted in the important findings of Mitra and Dutta (1991), albeit I object to any areal averaging of records at all.

Final Remarks

I don't think this paper should be published. If it is published it should be reduced by about 75% because it is enormous. And if it is published GYJ should be required to reference the one Mitra et al. (1991) paper that has appeared and say why they think it bad and their approach better. They should likewise be required to reference say, Currie and O'Brien (1992), and say why they think their conclusions on rainfall in the USA are no better than those of Mitra et al. (1991). My impression is that GYJ do not, as Wigley does, regard all natural variability in rainfall as random noise and that their job is to detect the Green House signal. If my impression is right then why don't they start learning something about spectrum analysis and see whether the nonrandom variations in rainfall are broadband or narrow band?

One thing that bothers me about this paper, and a great many other papers, is the use of vague phrases over and over and over. The most common is "interannual variability". What does this mean? It apparently means periods beyond one year. Well, one frequently reads of ENSO having a broad band period of 3 to 8 years. Is this what they mean? If not, then the next vague phrase I read over and over and over is "decadal", i.e., 10 years or longer I gather.

Even in the world of seismic exploration for oil where one has poor predictibility and the seismic sections are turned over to geologists for interpre-

tation one is not allowed vague phrases. The geologist wants to know what source of energy was used (dynamite in the old days and more controlled sources later). He wants to know where the peak energy is approximately for reasons I won't go into. In other words one is not allowed vague descriptions.

Epilogue

I'm led finally to a personal story I shall remember to my dying day. After leaving the oil industry and getting mixed up in atmospheric science (the worst slum ever created by the human mind in my opinion) I attended informal meetings; and as usual "Principle Component Analysis" was a principle topic. A high level Administrator from a USA Government Agency headed the meeting and on the way to the bathroom I remarked I had never heard of "Principle Component Analysis" until I left the oil industry. This Administrator misunderstood me completely. He told me in effect "well now you are in the big leagues of basic research supported by a big agency of the Federal Government, not in Hicksville, Texas with a hick oil company". Hicksville, Texas happened to be the consolidated reseach laboratories of the 4th largest oil company on earth with many hundreds of the best chemical, petroleum, electrical and signal processing engineers money could buy. The real hicks, of course, were this Administrator and people at this meeting.

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REFEREE'S COMMENTS (for Author)

Coherent rainfall zones of the Indian region

Gadgil S. et al

The Indian rainfall data set is one of the great treasures of our limited climate data base. It is also one of the most extensively analyzed regional rainfall data sets. In this well written paper, the authors describe a technique for reducing the dimensionality of the data set while retaining the most important information on interannual variability.

The methodology used for defining homogeneous rainfall regions is of general interest and can be applied to other regions as well. The statistical results as described in this paper are of sufficient interest to warrant publication. The physical implications of the results are not addressed in this paper and I therefore look forward to follow-up studies based on these statistical results.

I found few errors in my reading of the paper (Wigley misspelled on pages 20, 21; some inconsistency on the references to the number of zones on p. 26). I found the review of results from the scientific "grey literature" of India to be of interest.