

**REPLY TO RITSON:**

**“COMMENT ON “HOCKEY STICKS, PRINCIPAL COMPONENTS AND  
SPURIOUS SIGNIFICANCE” BY S. MCINTYRE AND R. MCKITRICK**

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Ritson's Comment [Ritson, 2005] exhibits pervasive misunderstandings and misrepresentations of both our article [McIntyre and McKittrick 2005a], ("MM05a" herein) and Mann *et al.* [1998] ("MBH98"), making his analysis pointless and incorrect.

Ritson incorrectly concluded that the purpose of principal components (PC) analysis in MBH98 was to simplify the "complexity" of the data set to obtain a "history". He then carried out a detailed analysis that is completely irrelevant to MBH98 or to our article. If a PC analysis is carried out on a matrix with  $r$  columns, then obviously the matrix can be approximated using  $m < r$  PCs and their associated eigenvectors. The mean across the columns of the approximating matrix (denoted by Ritson  $_m Y$ ) will, in turn, approximate the simple average of the data values. Ritson does not reflect on why anyone would use such an elaborate procedure to "approximate" a simple average, when the average itself could be calculated easily and exactly if it had any climatological meaning and was desired by the researcher. Ritson incorrectly claimed that we used the  $_1 Y$  series in our analysis and carried out a detailed analysis of its properties compared to the  $_2 Y$  series. His claim is false and his analysis is meaningless. The  $_m Y$ 's are nowhere used in our analysis or in MBH98. Both use the individual PC series, which have very different properties than the  $_m Y$ 's.

Ritson incorrectly stated that alternate "centering conventions" are permissible in a principal components analysis and that the short-centering methodology of MBH98 was appropriate for the data. Both claims are incorrect. Preisendorfer [1988], the statistical reference in MBH98, explicitly precluded non-centered methods in a *principal components* methodology, as follows:

The first step in the PCA of [data set]  $Z$  is to center the values  $z[t,x]$  on their averages over the  $t$  series... If  $Z...$  is not rendered into  $t$ -centered form, then the result is analogous to non-centered covariance matrices and is denoted by  $S'$ . The statistical, physical and geometric properties of  $S'$  and  $S$  [the covariance matrix] are quite distinct. **PCA, by definition, works with variances i.e. squared anomalies about a mean.** (p. 26, emphasis added)

Accordingly, Ritson's examination of the effect of different centering "conventions" is not an analysis of alternative *principal components* methodologies.

Ritson then incorrectly claimed that:

When sufficient terms are retained, results become independent of centering convention... Due to the different centering conventions, a constant offset is to be expected. However a constant offset leaves unaffected all physically relevant quantities such as temperature anomalies and changes.

Allowing Ritson some terminological licence here in his use of "convention", we agree that the column means of the approximations (Ritson's  $_m Y$ 's) are, up to an offset, not affected by centering methodology. This is obvious, since it is the same original matrix that is being approximated up to an offset. We never said otherwise, since we never had occasion to refer to Ritson's  $_m Y$ 's. However, the PC series, the eigenvalues and the eigenvectors, which we did discuss, are dramatically affected. Ritson ignored our specific demonstrations of these matters in MM05a, where we showed that the first eigenvalue of the MBH98 method was dramatically inflated (38% of variance) relative to the first eigenvalue in a centered calculation using the covariance matrix, and that the PC1s differed dramatically. Aside from ignoring our discussion of this, Ritson ignored the

specific warning of *Preisendorfer [1988]*, quoted above, that the properties of PCs can change dramatically when the data are decentered. It is the PCs and not the  $mY$ 's that are used in MBH98 regression-inversion steps, where the PCs have a substantial impact on the MBH98 temperature reconstruction [*McIntyre and McKittrick, 2005*] (“MM05b” herein). Ritson’s entire discussion of the properties of the  $mY$ 's is irrelevant.

Ritson incorrectly stated that “all results and figures in the MM05 GRL derive solely from (m=1) PC1 analyses”. This is misleading and easily refuted. In MM05a, we specifically reported that a hockey stick shape of the MBH98 PC1, reflecting the bristlecones, could be observed in the centered (covariance) PC4. We obviously could not have made this observation without considering lower order PCs. Our article also specifically referred to MM05b, where we demonstrated the impact of PC methodology (and the bristlecones) on an MBH98-type temperature reconstruction. The reconstructions discussed in that article used up to 5 PCs from the North American network and never less than 2 PCs. We referred Ritson to this article in private correspondence.

Ritson incorrectly claimed that the scale of our simulated PCs differed substantially from the scale of the North American PC1, presenting this as a supposed refutation of our article. This is false. Had Ritson checked the scale of the actual MBH98 NOAMER PC1, he would have seen that the scale of our simulated PC1 matches theirs precisely. Since the MBH98 algorithm subsequently re-scales the tree ring PC1 and applies the result in a regression step, the original scale is irrelevant anyway since remaining differences in scale merely change the regression coefficient.

Ritson incorrectly claimed that *Graybill and Idso [1993]* presented a “proxy-derived temperature history” using bristlecone pines. In fact, *Graybill and Idso [1993]* specifically denied that the anomalous 20<sup>th</sup> century growth of bristlecones could be attributed to climatic factors and hypothesized that the anomalous growth was due to CO<sub>2</sub> fertilization. We discussed this issue at considerable length in MM05b.

Ritson incorrectly claimed that, because the bristlecones had a different “pattern”, this mandates their inclusion as a temperature proxy. If the bristlecones have been contaminated as a temperature proxy, there is no statistical methodology that justifies their inclusion merely because they have a “different” pattern. If they are contaminated, they should not be used.

Ritson misleadingly pointed out that the simulated PC1s randomly pointed up or down and that the mean height of an ensemble of blades is 0 as though this were an original discovery on his part and something to be held against us. In Figure 1 of MM05a, we had previously showed that about half the simulated PC1s had an upside-up hockey stick shapes and half upside down. So of course the “mean height” is 0. However, this is irrelevant. MBH98 does not use “ensemble averages” of PC1s, but uses them one at a time. Used in this way, the blades do matter.

Ritson pointed out that the amplitude of his  ${}_2Y$  in simulations differed from the amplitude of the  ${}_2Y$  from the North American network. This is true, but, as noted above, the  ${}_2Y$  is nowhere used in any analysis. In fact, Ritson has inadvertently illustrated the power of the MBH98 method in mining for hockey stick shaped series. The reason for the difference in  ${}_2Y$  amplitudes is that simulated series with strong hockey stick shapes point both up and down, while in the NOAMER network, the strong hockey stick shapes are all

bristlecones pointing in the same direction. PC methodology disregards information on orientation and doesn't care whether the network series point up or down. It is an unattractive aspect of PC methods applied to proxy calculations that this information is discarded, and argues against use of PC analysis on tree ring networks in proxy studies. The non-centered MBH98 method intensifies this tendency and searches for hockey stick shapes so strongly that it not only obtains hockey stick shaped PC1s from a network containing the bristlecones, but even from networks of red noise. In *McIntyre and McKittrick [2005c]*, we show how even one or two hockey stick shaped bristlecone pine sites are sufficient to distort the decentered PCs from the North American network.

Ritson incorrectly claimed to have refuted our demonstration that the biased MBH98 method produced hockey stick shaped PC1s. The tendency of the MBH98 PC algorithm to produce hockey stick shaped PC1s has been independently confirmed by, inter alia, *von Storch and Zorita [2005]* and *Huybers [2005]*. Ritson's failure to replicate this result is due to his confused focus on the  $mY$ 's instead of the PCs.

Ritson incorrectly summarized our GRL article as *merely* claiming that "short-centering invalidated the MBH98 results." This is a fundamental misrepresentation. While we strongly criticized MBH98 short-centering, we emphasized the inter-relationship of the flawed methodology and the flawed bristlecone proxies. In fact, we originally identified the unique role of bristlecones in MBH98 by following the flawed methodology to see what it picked out in the controversial North American network. It is the interaction between contaminated proxies and flawed methodology that is essential, not *simply* the flawed methodology. However, once one is aware of the flaws in the proxies, it is obviously not acceptable merely to attempt to patch up the methodology

(whether through corrected PC methodology or otherwise), if the effect is merely to allow the flawed proxies to drive results some other way.

Our critique of MBH98 was not confined to the use of decentered PCs, but in any case, nothing in Ritson's comment overturns even that part of the argument.

## References:

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