May 24, 2005

Dear Dr. Saiers,

RE: AMMAN AND WAHL

In preparing our reply to Amman and Wahl (A&W), we have encountered some matters which appear to fall into categories where you have previously intervened editorially. We would like to bring them to your attention and obtain some guidance and/or rulings before going further. We are first bringing these matters to your attention informally, rather than through the GRL online submission system. But if it is your preference not to deal with anything raised herein other than through the GRL online submission system, please advise and we will proceed that way.

- Much of the A&W submission pertains to issues not raised in our GRL paper. It is difficult to reply to such comments while confining attention to the material in our GRL paper.
- We are troubled what we see as a "pyramid scheme" in which the key arguments and findings as stated in the A&W summary and abstract are themselves unsupported in the paper but instead are based on an unpublished submission by Wahl and Ammann to *Climatic Change*, which has not been accepted. These findings are stated in paragraph 8, levered up somewhat into the summary paragraph (para. 9) and then into the main conclusions of the Abstract; hence they are central to the conclusions made by the Comment. We are hardly in a position to respond to unpublished findings. To make matters worse, the conclusions in question all pertain to issues presented in MM05 (EE), but not in our GRL article. It's almost as though the GRL article is simply used to provide a platform for discussing these other topics. Within the four corners of a GRL response, it is obviously very difficult, if not impossible, for us to defend our EE results against criticisms arising from a separate, unpublished article. Our view is that any points which rely on unpublished arguments submitted to another journal about points made in EE should be excluded and **we request** that this be done prior to our submitting a Reply, so that we can focus on the GRL-related topics. In this case, it would involve the exclusion of para. 8, sentences 2,3 and 5 of the summary paragraph (para. 9) and the exclusion of the last 2 sentences of the Abstract (which would then be replaced by a claim, such as sentence 1 of para 9 which is actually argued in the running text and which relates to our GRL article.
- In your editorial response to our first Reply to Ritson, you stated that you would deal with concerns about being misquoted at an editorial level by requiring revision of the Comment and that such points did not need to be considered in a Reply. This was an important but minor issue in Ritson, but is a big problem in Ammann and Wahl. We have no idea how to cope with the numerous misquotations and mischaracterizations in the limited scope of a Reply and we doubt you would wish us to get involved in this sort of commentary. We have parsed the A&W submission and, as you will see, the problems are pervasive. We apologize for the length of the attachment but we want to spell out our concerns thoroughly now so we don't go off on a tangent in our reply. If you asked A&W to use or demonstrate actual quotations from our articles, and preferably from our GRL article, it would certainly reduce the problem. **We**

request that you advise us whether you prefer to deal editorially with misquotation issues so that we can avoid these matters in a Reply and, if so, which items we can avoid.

- There are many points where A&W present calculations or results as if they were novel when they are, in fact, already given in our own papers. The issue is not simply that we are not cited but that A&W phrase their paper so as to insinuate oversights or omissions on our part.
- There are several assertions made by A&W for which no argument or source is provided and which appear to be merely unsupported speculations.
- There are numerous instances where key terms are not defined. We can deal with these in our response but you should look at them to see if the statements are simply extraneous and ought to be taken out.
- In important cases, several of these issues interact.

A full listing of problems is attached as Schedule A.

Our overall impression is that when the "pyramid scheme" assertions in paragraphs 8 and 9 are set aside, the next most important point in the A&W article is that the hockey stick shaped PC series [of the bristlecones] occurs in PC series of different order depending on the standardization – the PC1 under MBH98, the PC4 and PC2 under other circumstances. This is not a new observation: we made equivalent points in connection with principal components calculations using a covariance matrix (PC4) and using a correlation matrix (PC2). Since Huybers brought up the exact same issue we have made a detailed reply on this matter already (though we are certainly able to do so again if needed).

Ammann and Wahl are at best making a portion of the argument that Huybers already spelled out with greater clarity and depth. But at the moment, to the extent that they have made any comment on our GRL material, it is overlaid with a lot of extraneous, unsupported and inaccurate material, so we are seeking advice on how to focus our response.

We apologize for the trouble, but think that it is better to deal with this earlier rather than later. Thank you for your attention,

Yours truly

Stephen McIntyre and Ross McKitrick

SCHEDULE A

MATTERS RAISED THAT ARE UNRELATED TO GRL PAPER

1. A&W state (Abstract) that

> "**McIntyre and McKitrick [2005a]** suggest that the procedure applied to North American tree ring records led to a systematic bias in the famous "hockey stick" series of Northern Hemisphere temperature". They go on to claim: "this claim is unfounded, and that a proper standardization, independent of the reference period applied, leads to essentially the same [NH temperature reconstruction] result."

Their "2005a" refers to our GRL paper. In the text itself (para. 2), no such connection is drawn since these points are not made in our GRL paper. Instead, in the paper itself A&W attribute the claims to McIntyre and McKitrick [2005b], our E&E paper, as follows:

> "**MM05b** [our bold] also conclude that the ITRDB data transformation then leads to biases in the hemispheric temperature reconstruction. They present calculations based on an alternative method, which leads to results that do not exhibit a "hockey stick" shape in the first PCs. Here we show that this result is an artifact of using only centered, but not standardized, data combined with an unchanged PC rejection criterion that does not properly reflect the modification."

The topics were discussed in our E&E paper. Hence the citation to our GRL paper in the Abstract is wrong, as evidenced by its inconsistency with the text.

2. A&W (para. 1) state: "Useful Northern Hemisphere temperature reconstructions for past centuries have been available since at least 1993 [Bradley and Jones, 1993]. Since 1998, a large number of reconstructions have been published reflecting widening proxy networks and improvements in reconstruction techniques [see summary by Jones and Mann, 2004]." We do not discuss the history of temperature reconstructions in our GRL article and do not agree with this characterization of the literature. For example, the first IPCC report in 1990 included a temperature reconstruction which the IPCC presumably considered "useful". Whether recent efforts constitute "improvements in reconstruction techniques" is somewhat in the eye of the beholder. We are not convinced that they do. In any case, dealing with all such multiproxy studies is well beyond the scope of a Reply. These sentences have nothing to do with our GRL article.

3. The A&W Abstract states that "a proper standardization, independent of the reference period applied, leads to essentially the same result." This claim is only applicable against claims in MM05b (EE), not GRL.

RELIANCE ON UNPUBLISHED MATERIAL UNDER REVIEW ELSEWHERE

4. A&W (para. 8) cite their own unpublished submission to *Climatic Change* as evidence for key statements in paragraph 8. There is no other support or argument for these claims other than citation to a paper only just entering the review process. We ourselves are particularly sensitive to the use of unpublished submissions as supposed support. Jones and Mann [2004] in *Reviews of Geophysics*, cited a paper in submission to *Climatic Change*, which criticized some of our earlier work. This submission was also cited in the MBH Corrigendum in *Nature* to support an assertion of robustness of results to the matters disclosed therein. This submission was subsequently rejected by *Climatic Change*, but not before the claims were circulated. They were, for instance, quoted on the Environment Canada website as a supposed refutation of our work. *Wahl and Ammann, submitted*, has been submitted to *Climatic Change.* It is premature at this point to assume that it will be accepted,

5. The claims based on unpublished material still under review elsewhere are not minor for the paper's rhetorical force: they are carried forward without any further support into the summary paragraph (para. 9) with even stronger language, and then are highlighted in the Abstract – a situation which resembles a sort of "pyramid scheme". We can hardly enter into detailed discussion of these claims on the available record. Further, the points made in para. 8 and carried forward into the summary and abstract do not even pertain to our GRL article, but to our EE article. We think that these claims should be deleted until there is a record which permits us to respond to them.

MISREPRESENTATIONS AND MISQUOTATIONS

6. A&W (para 2) state that "MM claim that the standardization approach chosen by MBH biases the ITRDB information towards a "hockey stick" shape". This is not what we said. In our GRL Abstract, we stated:

> Their method, when tested on persistent red noise, nearly always produces a hockey stick shaped first principal component (PC1) and overstates the first eigenvalue. In the controversial 15th century period, the MBH98 method effectively selects only one species (bristlecone pine) into the critical North American PC1, making it implausible to describe it as the "dominant pattern of variance".

Our points are clearly summarized and stated and do not require paraphrases that change what we said. These paraphrases are a recurring problem. A&W should quote the exact claim that they purport to rebut or at least re-state it without changing its meaning.

7. A&W (para. 2) stated: "Recently, McIntyre and McKitrick [2005a and 2005b, subsequently called MM collectively, or MM05a and MM05b individually] have criticized a particular aspect of this reconstruction [the PC algorithm]". While it is true

that we criticized the PC algorithm of MBH98, this sentence misrepresents our articles by implying an overly narrow focus. We dealt with many issues additional to the PC algorithm. For example, MM05a (GRL) examined statistical skill and RE benchmarking, while MM05b (EE) discussed the robustness of NH temperature reconstructions to various methodological permutations and to presence/absence of bristlecone pines, validity of bristlecone and Gaspe cedars as temperature proxies and numerous instances of inaccurate disclosure and misrepresentation in MBH98. The A&W summarization in para. 2 is highly selective and thereby misleading.

8. A&W (para 2) state that "Because the North American records play an important role in the reconstruction of the 15th century, **MM05b** [EE – our bold] also conclude that the ITRDB data transformation then leads to biases in the hemispheric temperature reconstruction for that time". Again, this is a misquotation of what we said in MM05 [EE]. While we discuss "bias" in connection with the PC methodology in GRL, we discuss "robustness" and "skill" in connection with NH temperature reconstructions. These nuances matter and there is no need for misquotation. The Abstract to that article stated:

> The differences between the results of *McIntyre and McKitrick [2003]* and *Mann et. al. [1998]* can be reconciled by only two series: the Gaspé cedar ring width series and the first principal component (PC1) from the North American tree ring network. We show that in each case MBH98 methodology differed from what was stated in print and the differences resulted in lower early 15th century index values. In the case of the North American PC1, MBH98 modified the PC algorithm so that the calculation was no longer centered, but claimed that the calculation was "conventional". The modification caused the PC1 to be dominated by a subset of bristlecone pine ring width series which are widely doubted to be reliable temperature proxies. In the case of the Gaspé cedars, MBH98 did not use archived data, but made an extrapolation, unique within the corpus of over 350 series, and misrepresented the start date of the series. The recent Corrigendum by Mann et al. denied that these differences between the stated methods and actual methods have any effect, a claim we show is false. We also refute the various arguments by Mann et al. purporting to salvage their reconstruction, including their claims of robustness and statistical skill.

9. A&W (para 2) state that "They [MM05b (EE)] present calculations based on an alternative method that leads to results that do not exhibit a hockey stick shape in the first PCs". We agree that we presented PC calculations based on a covariance matrix in which the first 3 PCs do not have a hockey stick shape. But it is misleading to state that we presented these calculations as an "***alternative*** method". We presented them as the calculations that result from applying the PC methodology stated to have been used in MBH98 – a "conventional" PC calculation. For these calculations, we used the default settings (covariance matrix) on a standard algorithm in R (or S); the results in Matlab would be the same. This method was not conceived by us out of thin air as an "alternative" but as the implementation of the methodology actually described in MBH98. The A&W characterization is thus misleading on this point.

10. A&W (para. 2) state that they will "show that this result is an artifact of using only centered, but not standardized, data combined with an unchanged PC rejection criterion that does not properly reflect the modification." The assertion that we did not use "standardized" data is false. ITRDB chronologies are already standardized to dimensionless units. Indeed Huybers' Comment makes this very point (para. 6): "NOAMER records are standardized chronologies [Cook and Kairiukstis, 1990]".

11. A&W (para 3) state that "MM emphasize that the "hockey stick" shape is introduced because the standardization is performed relative to a subsection rather than the full series." Again, this is a misquotation, which makes an important change in emphasis. We state that the standardization is severely biased towards selection of hockey sticks. For the NOAMER network, we do not state that the PC method "***introduced***" the hockey stick shape. We point out quite clearly that a subset of bristlecone ring width series has a hockey stick shape; we do not argue that the PC "introduced" this shape. We do argue that the MBH98 PC method ***promoted*** a pattern, which is in the PC4 under the stated "conventional" methodology using a covariance matrix, to the PC1, that it attributed a huge eigenvalue to this subset (38%), making Mann et al. think that it was the "dominant component of variance", when it was actually a controversial local effect particular to bristlecone pines. This misquotation is very important as it affects much of the A&W analysis.

12. A&W state (para. 4) that "In the MM analysis, the "hockey stick" PC appears in the PC4 - yet the authors choose to retain only the first two PCs.". This is misleading and insinuates deception on issues where we took care to be transparent. In MM05a, we explicitly report (para. 12) that the hockey stick shape of the bristlecones appears in the PC4. A&W not only fail to credit this observation to us, but present the existence of a hockey stick shape in the PC4 as a novelty, as though we had concealed it. Their statement that "the authors choose to retain only the first two PCs" is false. The issue of PC retention simply doesn't arise in our GRL article, since no NH reconstruction is carried out in that article. In our EE article, where the issue comes up, we reported on a number of methodological permutations, including calculations in which we retained 5 PCs in the network involved. The statement implies that the location of the hockey stick shape should determine PC retention policy. They provide no evidence on this (implausible) criterion yet imply we are remiss for not applying it.

13. A&W (para. 7) state that "the MM claim that a "hockey stick" outcome in the PCs is an artifact of the MBH standardization procedures is incorrect." Once again, this is a misquotation and a rather serious misquotation. Note the difference between our explicit and precise reference to ***PC1s*** and the A&W discussion of ***PCs***. We do not state that the hockey stick pattern in the **lower PCs** is an "artifact" of the standardization method. We go to some pains to show that bristlecone growth has a hockey stick shape and spend considerable time in MM05b questioning the validity of bristlecones as a temperature proxy. The existence of a hockey stick pattern in the PC4 under a covariance matrix (PC2 under a correlation matrix) is not an artifact of the "standardization" method; we argue on separate grounds that is may be an artifact of fertilization rather than a product of climatic

factors. What we argue is that its promotion into the **PC1** is an artifact of the PC method and that this error led to the incorrect interpretation of this pattern as the "dominant component of variance". The misquotation here transforms these arguments entirely.

FAILURE TO CREDIT DISCUSSIONS IN OUR GRL & EE PAPERS

14. A&W (para. 3) state that "In MM05a [GRL], the PCs of the ITRDB data are calculated using data with the long-term mean removed. However, MM05a do not perform a division by the standard deviation." In this case they are criticizing us for not doing a step in the GRL paper that has no bearing on that discussion (though we refer to it briefly as a side comment in para. 3). It *does* have bearing on the issues raised our E&E paper, where we discuss and apply the step as appropriate. While the terminology is slightly different, use of a correlation matrix equates to division by the standard deviation. Huybers understood this. Since A&W spend much of their Comment discussing MM05b [E&E] results and conclusions, it is misleading for them to subtly distinguish here between MM05a and MM05b so as to suggest an oversight in methodology. In our GRL paper we applied a "conventional" PC algorithm, as said to have been used in MBH98, to the data. Using a covariance matrix rather than a correlation matrix is hardly an oversight; it is an accurate implementation of the method under examination. If it was the intent of A&W to argue about what ought to be the preferred methodology, as Huybers did, they should have done so; instead they make an unwarranted and inaccurate insinuation of omission on our part.

15. A&W (para. 2) state that "in MBH, these data are transformed through a standardization followed by Principal Components (PC) Analysis". This transformation was not mentioned in the original article and cannot be attributed to that article; it was reported for the first time in MM05a, but we are not cited.

16. A&W (para. 3) state that: "MBH standardize relative to the 20th century (1902-80) mean and corresponding standard deviation (and subsequently apply an additional scaling to the variance of the 20th century trend; this step has no significant implications)." MBH itself contains no description of this procedure; it was discussed for the first time in MM05a (GRL), but A&W fail to cite us and imply it was known all along.

17. In MM05a, we stated (para. 4) that "PCs can be strongly affected by linear transformations of the raw data." A&W (para. 4), using language almost identical to ours, state that scaling, a linear transformation, "strongly affect the resultant PCs". They do not cite our virtually identical comments and imply that we had overlooked this effect.

18. A&W (para. 4) state that, if we had scaled the series, we would have "captured the hockey stick shape with only two PCs". In MM05b, discussed at length by A&W, we state the same thing as follows:

- If the data are transformed as in MBH98, but the principal components are calculated on the covariance matrix, rather than directly on the de-centered data,

> the results move about halfway from MBH to MM. If the data are not transformed (MM), but the principal components are calculated on the correlation matrix rather than the covariance matrix, the results move part way from MM to MBH, with bristlecone pine data moving up from the PC4 to influence the PC2. In no case other than MBH98 do the bristlecone series influence PC1, ruling out their interpretation as the ""dominant component of variance" *[Mann et al, 2004b]*

A&W fail to credit our priority on this point; also once again, they not only present exactly the same point as a novelty, but as a supposed oversight.

19. A&W (para 5, Figure 1b) draw attention to the upward trending PC4 in a PC calculation using a covariance matrix. In MM05a (para. 12), we previously stated that the "distinctive contribution of the bristlecones [the hockey stick] only appears in the PC4". Again, A&W fail to acknowledge our priority on the point, present the point as a novelty and imply that we overlooked it.

20. Similarly, A&W (para 6, Figure 1c) draw attention to a similar effect, this time using the PC2 from scaled data. As mentioned before, this calculation is identical to our previously reported calculation (MM05b) using a correlation matrix, where we explicitly drew attention to the PC2. Again, A&W fail to acknowledge our priority on this point, present the claim as a novelty and imply an oversight on our part.

21. A&W (para. 7) state that "most importantly, if all proxy series are used in a framework where they are comparable, i.e. if they are not only centered but also scaled by their respective standard deviation, then the "hockey stick" pattern is prominent in the first two PCs." This is misleading as stated. Using a correlation matrix, as discussed before, the PC2 has a hockey stick shape (but **not** the PC1) – contrary to the impression left here that **both** PCs have a hockey stick shape. A&W fail to credit our prior explicit statement that the bristlecone hockey stick pattern occurs in the PC2 using a correlation matrix (equivalent to the method proposed here). Once again, in addition to failing to credit us, A&W present the result as a novelty, implying that it was something that we overlooked.

22. A&W (para 8) discuss climate reconstructions under various methodological permutations, stating that:

> Climate reconstructions using at least 2PCs from MBH or 2 PCs from our revised MM calculations, or at least 4 PCs based on the MM05a method all result in highly similar Northern Hemisphere temperatures in the 15th century (within five hundreds of a degree on average), the period strongly criticized by MM [see **Wahl and Ammann, submitted -** our bold].

A&W fail to credit our own explicit prior statements on the matter in MM05b, in which we stated the following:

> If a centered PC calculation on the North American network is carried out (as we advocate), then MM-type results occur if the first 2 NOAMER PCs are used in the AD1400 network (the number as used in MBH98), while MBH-type results occur if the NOAMER network is expanded to 5 PCs in the AD1400 segment (as proposed in *Mann et al. 2004b, 2004d)*.

Again, they present identical points as novelties claiming priority in their own unpublished article. In addition to falsely presenting these claims as novelties, they somehow suggest that we overlooked these points.

23. A&W (para. 8) go on to state:

> Any higher number of PCs leads to the same result representing a convergence towards the essential climate. This convergence is also apparent because results using the minimum number of PCs mentioned above also lead to essentially the same climate reconstruction as when all individual series are included by themselves rather than through a representation by PCs.

Once again, they fail to cite our nearly identical comments on the same matter in MM05b, where we stated:

> Specifically, MBH-type results occur as long as the PC4 is retained, while MM-type results occur in any combination which excludes the PC4.

Their "proof" based on "convergence" misleads to the role of the PC4 in the matter.

24. A&W (para. 9) state: "In summary, different standardization procedures prior to principal component analysis can change the order in which the analysis is going to extract information." This is the only statement in the summary paragraph (para. 9) that actually pertains to our GRL article and is the subject of the text of the A&W submission. Again, A&W fail to credit our own statements on the matter in our GRL paper (e.g. para. 4), incorrectly implying that their results are a novelty and that we committed an oversight. Moreover it fails to acknowledge that we showed that the order matters since the MBH98 transformation inflates the first eigenvalue. A&W make no attempt to disprove our point, but phrase their conclusion as if the order is arbitrary. The transformations do much more than simply permute the order of the eigenvectors: they change the identification of the dominant pattern.

UNSUPPORTED SPECULATIONS

25. A&W (para. 4) state that "Division by the standard deviation (scaling) is employed in order to make the series more directly comparable." This is mere speculation regarding motive as the step was neither described nor the rationale provided in MBH98.

26. A&W (para. 7) refer to "the choice by MBH of performing their standardization relative to the calibration period". A&W do not know that it was a "choice" rather than a computer error. If it were a "choice", then MBH misrepresented an important methodology. It seems implausible to us that they would intentionally misrepresent such

an important methodology and our surmise is that it was a programming error, which could easily have arisen using Fortran. The description of this method as a “choice” is potentially inflammatory against Mann et al. but there is no source provided in support of their claim.

ASSERTIONS MADE WITHOUT ARGUMENTS OR DEFINITIONS PROVIDED

27. A&W (para. 2) state that they will “show that this result is an artifact of using only centered, but not standardized, data combined with an unchanged PC rejection criterion that does not properly reflect the modification.” Their promise to discuss “an unchanged PC rejection criterion that does not properly reflect the modification” [whatever that means] is not fulfilled. There is no subsequent discussion of PC rejection criteria. The phrase does not occur again in the article.

28. A&W (para. 4) state that scaling affects “the number of PCs required to capture an adequate amount of the variance in the original data”. They do not define “an adequate amount of variance” and, without such definition, this statement is almost meaningless. We ourselves have been unable to discern any rhyme or reason to MBH98 protocols for PC retention, which were not reported in MBH98 itself and are not clarified by A&W.

29. A&W (para. 6) state that this diagram shows a “reversed allocation of PC1 and PC2 with regard to the **primary** hockey stick shape”. Describing the hockey stick shape as “primary” requires a definition of “primary” and proof that it is primary, but no such argument is made. In MM05a, we argued that the hockey stick shape was not the “dominant component of variance”, if that is what is meant by “primary”, but was merely a local effect of bristlecone pines. A&W seem to accept this point elsewhere yet present their graph as though it says the opposite.

30. A&W (para. 7) state: “ No matter what standardization procedure is applied, a “hockey stick” shape appears in the important PCs – as PC1 in MBH, as PC4 in MM05a, and as PC2 in our revision of MM05a.” The term “important PCs” is not defined and without a definition the statement is meaningless. Since PC retention is at the heart of the methodological debate, use of such imprecise terminology makes the point difficult to properly deal with. Moreover, the PC1 in calculations using either a covariance or correlation matrix does not have a hockey stick shape – these are surely “important PCs”. We do agree that the hockey stick shape of the bristlecones appears in the cited PCs under the corresponding method, but to say that these are the “important PCs” requires some definition and terminological consistency.

31. A&W (para. 9) state: “if properly performed, all approaches that capture an acceptable amount of the variance in the underlying proxy data lead to essentially the same reconstruction results”. The only support for this statement is sentence 2 of paragraph 8, which makes a much more specific and restricted claim about

reconstructions using specified numbers of PCs. The specific claim in para. 8 has been levered into a more general conclusion regarding "properly performed" PC analysis using approaches that "capture an adequate amount of variance". The terms "proper" and "adequate" are nowhere defined and the more general conclusion in the summary is merely asserted, not proven. The more general conclusion hardly follows from sentence 2 of para. 8. Further, as explained below, para. 8 is merely a paraphrase of material from an unpublished paper. And in any case the point refers to claims in MM05b (EE) rather than in GRL.

32. A&W (para. 9) state, as part of their summary, that: "The "hockey stick" appears in all the summaries because it is an important part of the ITRDB network." The text does not define what is an "important" part of the ITRDB network or establish that the bristlecones are an "important" part of the network.

33. In the Abstract, A&W state: "Climate reconstructions based on proxy records require steps of standardization of the different series prior to their calibration to instrumental data." This may or may not be true, but it is nowhere argued in the text. In regression calculations, standardization is not required, so the validity of this statement depends on the methodology anyway.

INTERACTING PROBLEMS

We have categorized specific statements to facilitate identifying specific editorial interventions that may be pursued in order to keep the discussion focused. However some of the most important statements in W&A reflect overlapping problems.

A&W (para. 9) state that the "claim by MM that a spurious "hockey stick" climate reconstruction is introduced by data transformation is unfounded."

- This claim pertains to MM05b (EE) which discusses climate reconstructions, not our GRL paper.
- Further, this point simply levers on the para. 8 points, all of which are based on unpublished and unaccepted material (to which we can hardly respond).
- The point as stated is also a misquotation of MM05b (EE). We do not argue that the reconstruction is "introduced by data transformation". We spend a considerable amount of time demonstrating that the data transformation promotes the hockey stick shaped bristlecones. However, we do not limit our analysis to the transformation. Having identified the bristlecones as the "active ingredient" in the hockey stick by following the method, we pointed out their lack of validity as a temperature proxy. For example, since A&W bring MM05b claims into question, we quote the following, which explicitly takes the position that data validity as well as methodological

transformations affect the reconstruction (though not the findings of PC bias argued in GRL).

> Although considerable publicity has attached to our demonstration that the PC methods used in MBH98 nearly always produce hockey sticks, we are equally concerned about the validity of series so selected for over-weighting as temperature proxies. While our attention was drawn to bristlecone pines (and to Gaspé cedars) by methodological artifices in MBH98, ultimately, the more important issue is the validity of the proxies themselves. This applies particularly for the 1000-1399 extension of MBH98 contained in *Mann et al. [1999]*. In this case, because of the reduction in the number of sites, the majority of sites in the AD1000 network end up being bristlecone pine sites, which dominate the PC1 in *Mann et al. [1999]* simply because of their longevity, not through a mathematical artifice (as in MBH98).
>
> Given the pivotal dependence of MBH98 results on bristlecone pines and Gaspé cedars, one would have thought that there would be copious literature proving the validity of these indicators as temperature proxies. Instead the specialist literature only raises questions about each indicator which need to be resolved prior to using them as temperature proxies at all, let alone considering them as uniquely accurate stenographs of the world's temperature history.

As another example: A&W (Abstract) states: "McIntyre and McKitrick [2005a] suggest that the procedure applied to North American tree ring records led to a systematic bias in the famous "hockey stick" series of Northern Hemisphere temperature [Mann et al. 1998, 1999]..and that this claim is unfounded." Again, this claim refers to MM05b (EE) not MM05a (GRL), as the text goes on to make clear (see above.) And as expressed by W&A, it is a misquotation of our actual explicit statements. Finally, to the extent that the claim is justified, it is simply the result of a pyramid scheme – claims in para. 8 based on an unpublished and unaccepted paper by A&W are levered into the summary (para. 9) and then levered into the Abstract, despite the absence of any validation in the article itself.