Lately I've gotten into the bad habit of not commenting until a thorough examination is complete. But that results in frequently never making any comment at all (because I get distracted by other things). With that background, here are some first thoughts, which hopefully will not turn out to be final thoughts, time permitting. But who knows.

There are a couple of interesting facets to this review. One is that the review includes discussions from web sites, which is an entirely new direction for sure. Doing so cannot be easy, since internet discussions typically vary wildly in terms of quality and coherence, and ad hominem attacks are quite high in web-based paleoclimate discussions, making it hard to know how much personal acrimony tints the arguments. This factor is huge, making for example, great difficulty in trusting the full objectivity of statements by Michael Mann and Steven McIntyre for example, who very clearly and obviously detest each other with a serious passion. Not only that, but each also has a set of followers, includig other blog writers, that more or less detests the opposing "camp". It's a very sorry state of affairs and the two sides have collectively done nothing so much as create an enormous public confusion and distrust. So to wade into these discussions in an attempt to illuminate the strictly technical issues, is a definite service to the public, many of whom must have had their understanding of the science informed almost entirely by these various web sites. This is very commendable.

Also, there can be informative discussions at such sites that may never have appeared in the scientific literature for one reason or another. I've never been one to argue for the inherent superiority of peer-reviewed literature over discussions elsewhere; the only thing that matters in the end is the validity of the arguments made, and truly bad work appears in the literature on a daily basis. The peer reviwed literature system is seriously flawed in a number of ways, and the defense of a thesis because it's "passed peer review" is an often meaningless and invalid argument.

As for the content, the review focuses almost entirely on the issues involving how to aggregate spatially dispersed (i.e. site specific) paleo-temperature estimates into a hemispheric or global mean. I've only glanced cursorily at this (see below), but it appears to be quite thorough, referencing many sources. It also looks to be written in as non-technical of a language as possible, again very helpful. I hope to be able to comment on some of the actual issues raised at some point.

Given that the review's goal is a comprehensive assessment of the issues involved, my main comment at this point is that it concentrates on issues which are definitely important (potentially, if not actually) but are nevertheless one step removed from more primary issues which must first be addressed and resolved. Simply stated, the various issues involving aggregating site-specific paleo-estimates into larger scale "reconstructions" addressed by C&C only become relevant when it is first known that the site-specific estimates themselves, are in fact, reliable. There is a definite "troubleshooting hierarchy" in the field, stemming directly from the sequence of steps involved in creating any reconstruction.

With respect to tree ring analysis, which is the basis for the vast majority of terrestrial reconstructions, the two most primary issues are (1) whether linear relationships between driver (climate) and response (ring characteristic) can in fact be assumed monotonic (usually, linearly so). This is an enormous, common and highly suspect assumption, and represents the key issue addressed by Loehle (2009), although not only by him.

Secondly, the mathematical methods by which the non-environmental signal is removed from the ring response series, must be valid. Further, this issue must be evaluated with respect to the characteristics of the actual, existing tree ring data sets to which they have been applied, which very strongly involves the age structure of the sampled trees. This issue has been identified If these methods are faulty, one

WILL necessarily mis-estimate the long term trend in the ring response, a result which will be highly critical for all subsequent estimates of relative climatic states over time whenever an actual trend exists, i.e. will defeat the entire goal of the process. This is the essential point in my series of blog posts (and rejected PNAS manuscript two years ago) on analytical problems in dendroclimatology (starting here: http://ecologicallyoriented.wordpress.com/2012/11/10/severe-analytical-problems-in-dendroclimatology-part-1/)

C&C include some discussion of the use of pseudo-proxies in evaluating statistical analysis methods. My final point here is that I am 100% in favor of this approach (not just in this field, but in observational science generally), and am in fact convinced that only via systematically conducteed simulations of realistic tree ring data sets can methodological problems be identified (and potentially corrected). The problem is that this approach has only been applied to the types of issues that C&C address here, and not to the types of issues that Loehle and I address. They need instead to be addressed to ALL of the actual or potential statistical and mathematical analysis steps involved in creating a large scale reconstruction.

Hopefully more to follow, but we shall see.
Jim Bouldin, 10-13-2014