

Strange Scholarship in the Wegman Report

A Façade for the Climate Anti-Science PR Campaign

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09/26/10, V1.0

This report offers a detailed study of the “Wegman Report”: Edward J. Wegman, David W. Scott, Yasmin H. Said, “AD HOC COMMITTEE REPORT ON THE ‘HOCKEY STICK’ GLOBAL CLIMATE RECONSTRUCTION” (2006), republicans.energycommerce.house.gov/108/home/07142006_Wegman_Report.pdf .

It has been key prop of climate anti-science ever since. It was promoted to Congress by Representatives Joe Barton and Ed Whitfield as “independent, impartial, expert” work by a team of “eminent statisticians.” **It was none of those.** A Barton staffer provided much of the source material to the Wegman team.

The report itself contains numerous cases of obvious bias, as do process, testimony and follow-on actions.

Of 91 pages, 35 are mostly plagiarized text, but often injected with errors, bias and changes of meaning.

Its Bibliography is mostly padding, 50% of the references uncited in the text. Many references are irrelevant or dubious. The team relied heavily on a long-obsolete sketch and very likely on various uncredited sources.

Much of the work was done by Said (then less than 1 year post-PhD) and by students several years pre-PhD.

The (distinguished) 2nd author Scott wrote only a 3-page standard mathematical Appendix. Some commenters were surprised to be later named as serious “reviewers.” Comments were often ignored anyway. People were misused.

The Wegman Report claimed two missions: #1 evaluate statistical issues of the “hockey stick” temperature graph, and #2 assess potential peer review issues in climate science. For #1, the team might have been able to do a peer-review-grade statistical analysis, but in 91 pages managed not to do so. For #2, a credible assessment needed a senior, multidisciplinary panel, not a statistics professor and his students, demonstrably unfamiliar with the science and as a team, unqualified for that task. Instead, they made an odd excursion into “social network analysis,” a discipline in which they lacked experience, but used poorly to make baseless claims of potential wrongdoing.

In retrospect, the real missions were: #1 claim the “hockey stick” broken and #2 discredit climate science as a whole. All this was a façade for a PR campaign well-honed by Washington, DC “thinktanks” and allies, under way for years.

Most people can just read the 25-page main discussion, but 200+ pages of backup text are included to provide the necessary documentation, as some issues are potentially quite serious.

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(often-contentious) computer performance evaluation, widely used to design computers since. For the last few years he has been studying climate science, anti-science and energy issues. There are bound to be errors, please report them. There likely will be updates, as this story is not over. Contact: JohnMashey (at) yahoo.com.

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Executive Summary

Climate science yields increasingly-stronger scientific results, but obscured by an ever-louder anti-science PR campaign, of which a key part remains the 2006 “Wegman Report,” led by **Edward Wegman**. It was heavily promoted to the US Congress by Representatives **Joe Barton (R-TX)** and **Ed Whitfield (R-KY)** as “independent, impartial, expert” work by a team of “eminent statisticians” to analyze the climate “hockey stick.”

Although problems were clear upon its release, to this day some still reference it positively or even authoritatively:, such as:

- Recent books, a quick sample: US (6), UK(2), Canada(1), Australia (1)
- Submissions (6) to UK Parliament, February 2010, on “Climategate”
- Websites and blogs, including some with large, worldwide readership
- Steady streams of articles, one recently in a real statistics journal

In 2009/2010 Canadian blogger “Deep Climate” (DC) discovered some serious problems, starting with plagiarism. That inspired my longer investigation, which kept growing as interconnected problems multiplied, starting with basic scholarly practice, requiring little specific knowledge.

Quality of basic scholarship?

- Of 91 pages, 35 are mostly plagiarized, but injected with biases, errors or changed meanings that often weaken or invert original results. Some might thus also be called fabrication. DC found 10 pages that plagiarize uncredited sources. Then 25 pages summarize papers, but with extensive plagiarism. Text of “striking similarity” to the originals totals 81% of the words, but 50% is word-for-word identical, cut-and-paste.
- Obvious plagiarism needs so little explanation that fabrications are not generally enumerated, especially as some errors might be attributed to incompetence. Either issue is taken seriously in academe.
- One major fabrication does stand out. It is a distortion of an sketch already obsolete by 1992, but supported strongly and used repeatedly.
- Of 80 references, 40 are not even mentioned (cited) in the text, but just pad the Bibliography. Many are irrelevant or dubious, such as a tabloid writer’s 1987 ozone article in a fringe technology magazine.
- Much of this is a science-seeming façade for a few key PR messages. Many of the science papers, even ones summarized, are mostly ignored. The team really only paid attention to a few papers.

Wegman team – independent?

- Barton and Whitfield rejected an offer of a normal National Research Council (NRC) report, then recruited Wegman via an obscure route likely to find a team to produce the desired results.
- Barton staffer **Peter Spencer** selected the team’s papers or passed them from those behind the PR campaign, local “thinktanks” or close allies.
- At least one of those allies worked directly with the Wegman team.

Wegman team – impartial?

- They ignored standard good practices, but repeated many common anti-science PR messages, most from a well-evolved PR campaign by thinktanks, their allies and a few members of Congress.
- They spent many pages on science-seeming camouflage, but the key messages can all be found in a May 2005 thinktank talk.
- They denigrated the work of relevant climate scientists, never talked to any and often avoided their credible (but inconvenient) results.
- Pervasive bias is especially obvious in highlighted side-by-side comparisons with plagiarized sources. Changes leap off the page.

Wegman team – expert?

- Wegman and 2nd author **David W. Scott**, are clearly distinguished, but Scott wrote only a 3-page Appendix of standard mathematics.
- Much of the writing, perhaps even most, was done by the 3rd author, Wegman student **Yasmin H. Said**, PhD Spring 2005.
- The report acknowledged 2 more Wegman students, not yet PhDs.
- Although they discussed statistics, the team offered no useful new statistical analysis. They avoided doing the obvious “right one.” Casting doubt via statistics discussion was the key mission #1.

Many issues are described in the attached report on the Wegman Report, its associated testimony and related actions. A 25-page discussion should suffice for most readers to understand the clear, if harsh result:

From start to finish, this entire effort was created to mislead the US Congress, the USA and the rest of the world. It still is used that way. This is backed by a mass of interconnected evidence in 200+ pages of Appendices. The team and its report simply do not match the claims made to Congress. The discussion is US-centric, but affects everyone, as the world’s climate anti-science effort really is centered in Washington, DC.

The Wegman Report's own review process.

- Whitfield claimed it was peer-reviewed. **It was not.**
- Wegman and others claimed this to be like a NRC report. **It was not.** The NRC uses a rigorous process run by independent monitors. Report writers and anonymous-at-the-time reviewers are chosen to cover all relevant disciplines. All commit to serious effort on a clear schedule. Barton and Whitfield rejected this standard process.
- Wegman sent the report to a few statisticians, of varying degrees of closeness, but all known to him. **Some were given only a few days to comment on a long report covering unfamiliar topics. Some gave strong advice that was simply ignored.**
- Some were later surprised to find themselves claimed as reviewers. Fine statisticians' names were mis-used to lend unwarranted credibility.

Mis-use of social network analysis against climate peer review.

- With little expertise in such analysis, the Wegman team plagiarized textbooks, then used incomplete analysis to make flawed claims.
- They claimed that coauthorship implied poor peer review, even wrongdoing, but with no evidence whatsoever.
- The team was poorly qualified to evaluate peer review in climate research, but key mission #2 was to cast doubt, which they did.
- In 2007, Said, Wegman and 2 students re-used the plagiarized text to attack climate peer review, in a statistics journal that generally does not cover social network analysis. Their badly-flawed paper was accepted in 6 days, compared to an average of 200. Wegman was a 20-year advisor. Said was an Associate Editor. That may be coincidence.
- That paper acknowledged financial support from 3 US Federal research contracts, none of which had obvious relevance.

Plagiarism and awards among Wegman PhD students

- The Wegman Report's social networks text was re-plagiarized twice more, by Wegman students receiving PhDs in 2008 and 2009.
- Said's 2005 dissertation has 5 other pages of plagiarism, with a cut-and-paste "style" quite like the 35 known in the Wegman Report.
- All 3 dissertations received departmental "Best of year" awards.

Wegman and Said after the Wegman Report.

- Promises were made in 2006 of forthcoming peer-reviewed statistics papers in various journals, but these never appeared.

- For at least 2 years, Wegman reiterated doubt-raising claims, often speaking to audiences likely to lack relevant topical expertise.
- An exception was a 2007 workshop for top statisticians and climate scientists. It was not well-received. His talk showed ignorance of basics and parts might have been thought offensive. He also (mis-)used without acknowledgement 3 slides of the scientist most often attacked.
- Wegman and Said co-chaired a June 2010 statistics conference. At the last minute, they added 2 new sessions, inviting 3 non-statisticians known for climate anti-science. Said gave a "Climategate" talk decrying climatologists' bad peer review, destruction of data, etc.
- Said's 2005 dissertation has long been online, as has her 2007 talk, which unwittingly revealed important facts. In August 2010, both files disappeared and mention of the 2nd edited out of the seminar history.

McShane, Wyner - August 2010 "remake" of the Wegman Report

- A new statistics paper has just appeared, to wide acclaim by those fond of the Wegman Report, on which it relies heavily, but from which it plagiarizes earlier errors plus text Wikipedia text. It fabricates a citation to one of the Wegman Report-plagiarized books. It fabricates several other citations. It uses obsolete sources. Errors are pervasive. Unlike the Wegman Report, it at least offers some actual statistical analyses, although serious problems have been documented with them, too.
- Some newspapers touted the Wegman Report and now the remake. Within weeks, The Daily Telegraph (UK), The Wall Street Journal, and The Australian all ran pieces in its praise, clearly competent PR.

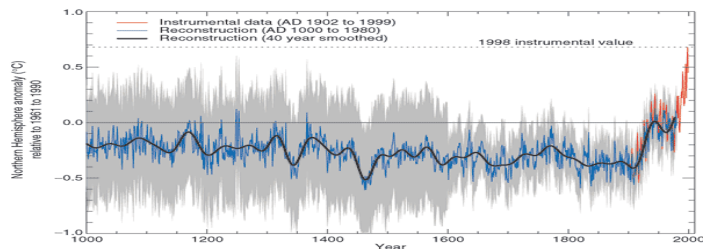
Recommendations.

George Mason University ought to investigate many problems, as should several other universities and journals, the US Office of Research Integrity and perhaps the American Statistical Association (ethics issues). At least 4 agencies may have possible fund mis-uses to consider. Some authors or publishers might pursue copyright issues. **Congress and the DoJ should investigate the manufacture of the Wegman Report.** Possible felonies are covered by the US Code, 18.U.S.C §1001 (misleading Congress), §371 (conspiracy), §4 (misprision), which might involve many more people. The report lists about 30 issues, not all for Wegman Report itself, but including derivations and related activities.

All this is strange. I do not think **most** statisticians try to lie with statistics.

Brief background

The Intergovernmental Panel on Climate Change Third Assessment Report (2001) displayed the following chart, soon known to many as the “hockey stick,” derived from 1998/1999 papers by researchers **Michael Mann**, **Raymond Bradley**, and **Malcom Hughes (MBH)**.



Of the huge number of climate science papers, it offered a simple, graphic understandable by the general public. As a compelling expression of Anthropogenic Global Warming (AGW), it was immediately attacked by people wishing to avoid CO₂ restrictions.

Following a 1998 strategy created with the American Petroleum Institute, the Washington, DC-area “thinktanks”¹ **Competitive Enterprise Institute (CEI)**, **George C. Marshall Institute (GMI)** and others (collectively, **TT**) had been recruiting “new faces” to speak against climate science. In 2001 they connected with Canadian economist **Ross McKittrick**, sponsoring him to speak in Washington. The 2002 actions included a key political strategy memo, several papers and a book coauthored by McKittrick.

Retired mining consultant **Steven McIntyre** began to collaborate with McKittrick (together, **MM**). They attacked the hockey stick in talks, papers and by website. MM have often acted as visible faces, but information is quickly shared among key people. To some extent, MM seem to have taken over public roles earlier played by astrophysicists **Sallie Baliunas** and **Willie Soon**, long involved with GMI.

¹ Some thinktanks are effectively tax-free lobbying/PR organizations, of which many relevant ones are shown on an interactive map. maps.google.com/maps/ms?hl=en&ie=UTF8&msa=0&msid=107940825189517771981.0004815492d08b0c445f9&ll=38.882481,-76.978455&spn=0.771829,1.253815&z=10

By late 2003, TT had brought them to Washington and introduced them to climate anti-science advocates, including Sen. James Inhofe (R-OK). MM became GMI “experts.”

In February 2005, McIntyre started the Climate Audit website. **Senator James Inhofe** claimed at a GMI meeting that their work had discredited the hockey stick, one of the 4 key pillars of AGW. Much publicity followed, including an unusual front-page Wall Street Journal article.

In May 2005, MM visited Washington, gave a talk that outlined many of the ideas used later in the Wegman Report. Soon thereafter, Reps. Barton and Whitfield wrote to Mann, Bradley and Hughes with many demands.

Rep. **Sherwood Boehlert (R-NY)** pushed back against this odd, intimidating procedure, as did the science community. NAS offered a standard (expert, unbiased, independent) NRC panel to look at the problem.

Barton and Whitfield rejected that, but were then left with the problem of having their strategy rebuffed. Via an odd indirect route, they recruited statistician Edward Wegman, who recruited others, mostly his students. This was later presented as being like an NRC effort, but simply was not.

The **Wegman Report (WR)** was finally issued in July 2006, with Congressional hearings and much PR, but some problems were clear even at the time. Many more have been found since. In December 2009, blogger Deep Climate showed that WR §2 was mostly plagiarized, but with changes to weaken or even invert conclusions.

This report started to further explore WR scholarship, already shown as shoddy at best, but a different conclusion eventually emerged. *The WR was created to ratify and amplify MM+TT’s latest PR to mislead Congress and the public. It had two clear missions: #1 discredit MBH99 via statistical arguments, and #2 discredit climate science by mis-applying social network analysis.*

Advice on reading this report

Contradictions are found among WR, related testimony and later efforts, not so obvious when just reading one part. The reader will find some redundancy of description as a result, as when repeating quotations for local reading flow. The complexity of the WR and surrounding events often defies easy simplification, as comprehensive backup evidence must be included. Common properties are given terse codings, and numerous cross-references included. I'd suggest ignoring all this on first read.

This report largely expands on parts of an earlier one:
 #[MAS2010] John R. Mashey, "Crescendo to Climategate Cacophony"
www.desmogblog.com/crescendo-climategate-cacophony
 V1.0 03/15/10.

Many of the people, organizations and activities mentioned briefly here are described in detail there.

Similar typographic conventions are used in the main body here - *Italics* for opinion and emboldening or underlining inside quotes mine. Layout tries to balance convenience between paper-only and on-line readers. The latter might print the main navigational aids (pp.2, 7, 8), then open a 2nd on-line copy of the PDF for jumps among Appendices. People who want to dig deep might also print p.12 as a reference sheet for the many codes.

Citations and references²

Citations found in the WR use its style, in which key MBH and MM papers have short codes, and all others use Author (year). All 80 WR references are listed in **W.8.2**, although some are vague or do not actually exist. **W.8.8** and **W.8.9** comment on ~50 of them, listed in the Index.

This report's own citations mostly use in-line URLs for on-line convenience. Some references are listed in this report's own Bibliography and cited in the form [MAS2010]. Wikipedia is helpful for quick topic introductions, but is never considered authoritative. For brevity, titles and given names are usually omitted, no discourtesy intended to any.

² As per Wikipedia, en.wikipedia.org/wiki/Citation, "More precisely, a citation is an abbreviated alphanumeric expression (e.g. [Newell84]) embedded in the body of an intellectual work that denotes an entry in the bibliographic references section of the work" In-line URLs combine citation+reference.

This report has 4 major parts:

Front matter – 12 pages

This includes the usual Table of Contents, Glossary and a brief Index placed near other navigational aids. It also includes some unfamiliar elements used to classify patterns and problems.

Memes are common climate anti-science messages, repeated so often that many are well-cataloged and numbered elsewhere. Here, a **Theme** is an important, generally accepted scientific idea or practice often ignored by the Wegman Report. The reader will often see text tagged with these, like Meme-18¹, or Theme-A⁴. Other codes include <eE> for Errors, <mm> for Meaning Changes, and <bb> for Biases, capitals rated more important.

Any of these are problems, **but on first read, I would suggest ignoring all this, except to notice how pervasive they are.** Some tables summarize these. The few readers who want to dig deeply can follow the codes..

Finally, the **Color codes** evolved late as a way to simplify categories of Memes&Themes, References, People and Organizations.

Likewise, I would suggest ignoring the colors, except to know

- **Red** usually means active climate anti-science, almost always a problem.
- **Orange** and **green** have various intermediate meanings.
- **Blue** usually means reasonable science or people (OK), often attacked, mis-used, or used as façade material (not OK). Theme⁴.is always bad.

Main discussion - §1 - §5 - about 25 pages.

People familiar with the hockey stick wars can skip §1. The rest summarizes the W.* sections, then puts all the pieces together.

A.* Appendices - about 70 pages.

This collects various topics as backup for the main discussion. Few people would read more than a few, but choices will differ. The Appendices effectively form a 200+-page reference manual, also covering topics related to the Wegman Report.

W.* Annotated Wegman, derivatives - about 135 pages.

Each W.n Appendix corresponds to WR§n, in some cases summarizing, in other cases annotating whole sections, as in the plagiarism studies.

Glossary and some key people

*'d acronyms are just used here. Others are more widely used.

AGW	Anthropogenic Global Warming
AR4 ⁴	IPCC 4 th Assessment Report
ASA ³	American Statistical Association
CEI ¹	Competitive Enterprise Institute (think tank, one of TT)
CHC ¹	Cooler Heads Coalition, front, run by CEI's Ebell and Horner
*CO ¹	Congressional allies of TT, including some unknown
CSDA	<i>Computational Statistics and Data Analysis</i>
*DC	Deep Climate, Canadian blogger (and not this author!)
E&E ¹	Energy and Environment, social sciences journal, low repute
FAR ⁴	IPCC First Assessment Report
GHG	Greenhouse Gases, i.e., CO ₂ , CH ₄ , H ₂ O vapor, etc
GMI ¹	George C. Marshall Institute (think tank)
GMU	George Mason University
*ID	IDentical text, spelled exactly, in order (cyan regular)
IFNA	Interface Foundation of North America, A.6.2.
IPCC ⁴	Intergovernmental Panel on Climate Change
LIA	Little Ice Age
MBH ⁴	Michael Mann, Raymond Bradley, Malcom Hughes
MBH98, MBH99 ⁴	WR codes for key MBH papers
MM ¹ or M&M	Steven McIntyre and Ross McKittrick, allies of TT, CO
MM03, MM05a, MM05b ¹	WR codes for key MM papers
* McI05, McK05, MM05x, MM06 ¹	codes added here for disambiguation
MWP	Medieval Warm Period
NAS ⁴ ³	National Academy of Sciences, one of Academies over NRC.
<i>Nature</i> ⁴	One of two most prestigious general science journals
NH (SH)	Northern (Southern) Hemisphere
NRC ⁴	National Research Council, does research for government
NSWC	Naval Surface Weapons Center, Dahlgren, VA
PCA	Principal Component Analysis (mathematical technique)
PNAS ⁴	Proceedings of the NAS, credible source
SAR	IPCC Second Assessment Report [IPC1995]
<i>Science</i> ⁴	One of two most prestigious general science journals
SNA	Social Network Analysis, study of human networks
*SS	Striking Similarity of text, i.e., usually called plagiarism
TAR ⁴	IPCC Third Assessment Report [IPC2001]
*WP ¹	Wegman Panel, mostly Wegman+Said, helpers, very little Scott
*WR ¹	Wegman Report (2006), also labeled [WEG2006]
WSJ ¹	Wall Street Journal, (Editorial, rarely news)
*TT ¹	Thinktanks (especially Washington, DC), close with MM, CO

Some key people, by group (Bold: visible for WR, regular: not)

CO¹ { James Inhofe¹ (R-OK) is a US Senator.
Joseph Barton¹ (R-TX) and **Edward Whitfield¹** (R-TN) are US Representatives, as is **Cliff Stearns²** (R-FL).
 Peter Spencer¹ (a Barton Congressional staffer) met with the WP, briefed them, sent them “daunting amount of material” to review [SAI2007, p.5]. Many other staffers, such as Mark Paoletta², might be involved, **A.11.2.**

Jerry Coffey¹ recruited Wegman[SAI2007]. He has expressed strong disdain for AGW (“Gore global warming boondoggle”) and praised books by Fred Singer and Pat Michaels [COF2009].

Edward J. Wegman¹, GMU [WEG2005, WEG2010]

WP¹ { **David W. Scott³**, Rice University [SCO2010], minimal role, **W.9**
Yasmin H. Said¹, Johns Hopkins University (2005-2006,) then at GMU
 An unknown 4th person, who later dropped out [SAI2007, p.5]
 Contributions were acknowledged from other Wegman students
John T. Rigsby III², Naval Surface Weapons Center
Denise M. Reeves³ or ², MITRE
Walid Sharabati² finished his PhD in 2008. Unmentioned in the WR, he contributed much of response to Rep. Stupak [WEG2006c, SHA2006].

MM¹ { **Steven McIntyre¹**, retired mining consultant, Ontario, Canada
Ross McKittrick¹, economics, U of Guelph, Ontario, Canada

TT¹ { Myron Ebell¹, CEI¹ and Cooler Heads Coalition¹
 Christopher Horner¹, CEI¹ and Cooler Heads Coalition¹
 William O’Keefe¹, GMI¹ CEO, ex-American Petroleum Institute¹
 Jeffrey Kueter¹, GMI¹ President since 2001, following Jeffrey Salmon¹.
 Mark Herlong¹, GMI¹ Program Director
 Fred Singer¹, SEPP¹ (a one-person thinktank), 20-year ally of GMI
 Pat Michaels², was U VA, now CATO¹, taught at GMU Summer 2010
 Sallie Baliunas², Harvard-Smithsonian Center for Astrophysics, GMI¹
 Willie Soon², Harvard-Smithsonian Center for Astrophysics, GMI¹
 Many others are possible.

I apologize for the dense abbreviations, but spelling out MM and WR alone adds 50 pages. I tried to minimize abbreviations, but it was not easy.

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Color codes

This report is an attempt to make sense of multiple sources, including the WR itself, [BAR2006, BAR2006a, WEG2006c, SAI2007]. It has been a long classification exercise to create attributes to summarize the mass of detail. Various codes were created for the WR Bibliography. As I annotated text of the WR and testimony, I noticed common features. Some were well-known climate anti-science Memes (bad arguments), whose

prevalence originally surprised me. Repeatedly-ignored standard practices got tagged as Themes. Only just before completion did it occur that 4 unifying color codes could be used to organize this, reducing the need to look at the more complex code combination used for the underlying analysis. **Most readers can ignore the detailed codes and just recognize that red 1 is usually a problem.** If they want to go deeper, they can see the specific code and check its origination and usage.

Memes and Themes

- Meme-nn¹ Cataloged climate anti-science meme, found in MM05x, **W.8.9** . Many fit mission #1.
- Meme-?¹ Meme found in MM05x, but not in the catalog, so given a letter code here. Many of these support mission #2.
- Meme-nn² Meme from a standard catalog, but not obvious in MM05x. Red ones are clear, some orange might belong in red. Some might be marginal. For example, Meme-08² is listed because it seems the only reason for 3 (uncited) popular press global cooling articles.
- Theme-?⁴ Theme, a good practice from science ignored often enough in the WR to be named.

The Index shows ~130 page- instances of 24 different Memes and another ~130 of 14 different Themes, but any page might have multiple instances, shown once. Some may be subjective, more easily may have been missed. Some are found elsewhere, not in the WR.

References and Page tally, §2.7

- ¹ Strong carrier of the red memes, work by MM or direct support for them. See also Link- M¹, Link- m² on next page.
- ² Miscellaneous climate anti-science, sometimes irrelevant, often not cited, occasionally problematic science paper.
- ³ Generally reasonable science, but irrelevant, uncited or weakly cited, seemingly as bibliography-padding or credibility-enhancement.
- ⁴ Mainstream relevant science, either being attacked, or perhaps cherry-picked, or being cited, then ignored. This category might have been split further, but this was already complex enough.

People, Organizations

- ¹ Wegman, Said, Coffey, or others who strongly involved in making the WR happen, not just in writing it.
- ² Involved, but not so clearly, like Rigsby, who clearly did the low-level SNA analysis, but may or may not have understood the larger context.
- ³ Likely not really involved, but inappropriately portrayed as more so, presumably to trade on credible people's names, such as Scott as 2nd author, or some commenters labeled as "reviewers" or even "contributors."
- ⁴ Mainstream climate scientists, often attacked, or who may well argue among themselves, but then get cherry-picked to over-emphasize doubts.

Memes and Themes

The WR repeats many well-cataloged climate anti-science Memes. They may be directly stated in the WR, quote-mined, amplified or indirectly supported via uncited, irrelevant references acting as meme-carriers. Such Memes may not be obvious to the reader unfamiliar with climate anti-science, but experienced watchers see them often. *But it is strange to find them pervading an “expert, objective, independent” report to Congress, especially from statisticians with no obvious experience doing such.*

Skeptical Science offers a convenient numbered list of climate anti-science arguments. Each has a short description linked to a general-audience description and debunking. Peer-reviewed literature is cited as backup. www.skepticalscience.com/fixnum.php

The subset used here is listed on the second following page. The WR uses some others enough to assign Meme letters, most of these appear in some form in MM05x, McK05, GMI2005, or at McIntyre’s website.

Meme-a **IPCC results depend mainly on 1999 hockey stick**
Sen. Inhofe was saying this by 02/10/05 [GMI2005, p.10].

Meme-b **Paleoclimate peer review is poor, due to social network**
McIntyre has history of promoting Memes -b and -c in his blog, *but the ideas may have started with Michaels* at GMI [GMI2003, p.10]:
“*Question: Pat Michaels, University of Virginia. I think what you’re really uncovering here is a larger and pervasive problem in science, which is the peer-review process seems to be missing important and obvious issues, perhaps failing because of the sociology of global warming science.*”
MM were thus “coached” by real experts like Michaels and Singer. “Groupthink” discussion appears May 2005, in McK05 then MM05x for GMI. These sources are referenced (vaguely) in WR, not cited.

Meme-c **No independent verification, since some data shared”**
This one claims data same, MM05x, p.17. **W.8.9.**

Meme-d **It’s a few bad scientists**
Attacks often focus on a few, or mainly one scientist at a time. Targets have been Ben Santer (IPCC SAR), Michael Mann (TAR), and Phil

Jones (“Climategate,” perhaps AR4). Misattribution of multi-person efforts to individuals is effective in personifying results, yielding easier targets compared to larger organizations. All have been repeatedly called criminals, attacked in OpEds and been threatened with violence.

Meme-e **Confounding factors everywhere**
“Confounding factors” are always impediments for which statisticians stay alert, **A.8.** When experts identify such factors and explain methods of dealing with them in numerous papers, amateurs add no value by labeling anything they do not understand as “confounding factors.” This may impress people unfamiliar with the field, but not experts. Bradley (1999) spends hundreds of pages to deal with such issues, but the WR inserts extra “confounding” several times into plagiarized text.

Meme-f **Faux fight between statisticians and climate scientists**
Some parts of the WR and follow-on’s, like [WEG2006c, SHA2006, WEG2007, SAI2008] almost seem attempts to create fights between statistics and climate science establishments. Climate scientists and statisticians have often had fruitful collaborations and interchanges, especially when the former know to ask for help and the latter take time to learn enough science, **A.4.**

Meme-g **General problem applies to all specific cases**
Confounding factors, missing data, data errors, PCA-decentering, suboptimal statistical methods and poor peer-review are all real problems, but may or may not apply in any given case. Labeling unfamiliar specifics as instances of familiar general problems is effective in raising doubts, except with experts who know better, **A.4.** Bad talks or papers seek unwary audiences.

Meme-h **We’re statisticians, only asked to look at statistics, MBH99**
This sometimes appears, seemingly to avoid other discussions, as of basic science or all the later papers, especially in testimony.

Meme-j **Large uncertainty means almost nothing is known**
This is a more general form of Meme-e, specifically added for **A.12,** McShane, Wyner (2010), although its antecedents may lie in McIntyre’s long efforts to denigrate every meaningful proxy.

Here, a Theme is an important, generally accepted scientific idea or practice often ignored by the WP. Many seem to be specific kinds of “Culpable Ignorance,” with Theme-N₄ as a general catchall. Senior researchers should either know these or know to ask experts.

Unlike the numbered Memes, the Themes and lettered Memes emerged from study of the WR. Of related pairs (Meme-56₁, Theme-G₄), the former might be repeated from lack of knowledge, but the latter requires ignoring citations and the WR’s own Summaries.

Science

A₄ – Avoid outdated sources

Scholars prefer well-established, but relatively recent credible sources over substantially older ones, especially those superceded by their own authors or repeatedly refuted in peer-reviewed literature.

Physics

B₄ – Energy is conserved on Earth, as elsewhere

If more energy arrives at the Earth than is radiated, it warms, sooner or later. In the short term, the oceans absorb most of the extra heat content, but it returns sooner or later. This is sophomore physics.

C₄ – The Greenhouse Effect is real, well-understood

GHG’s absorb infrared, and slow down outgoing radiation. That is also sophomore physics, from basic quantum mechanics.

Climate Science

E₄ – Ocean oscillations are not forcings

Ocean oscillations can strongly affect surface temperatures, but they mostly move energy around, rather than directly changing the Earth’s energy balance.

F₄ – Geography matters to surface temperature variability

Northern Hemisphere, NH extratropics and global temperatures are not identical. Land temperature varies more than oceans. The WR often confuses these, casting doubt on resulting conclusions.

G₄ – The Medieval Warm Period varied spatially, temporally

For years, most credible peer-reviewed papers have said this. The WR cites, even Summarizes such papers, but the idea usually gets lost.

H₄ – Late 20th-century warming is unusual, anthropogenic

This follows from Theme-B₄, Theme-C₄. Neither WR nor Wegman testimony ever admitted this, A.2, and it was effectively edited out or weakened in Summaries, W.8. It is not just WR’s “correlation is not causation” comment, W.3.

Statistics

J₄ – Confidence intervals matter in real science

Much science is presented with confidence intervals (bands), not just simple points (lines). Lines can be “close enough” when not identical.

K₄ – Big errors matter, small ones do not. Know which they are

Statistics normally uses various techniques to discover the sensitivity of conclusions to erroneous results or specific pieces of data.

Sociology

M₄ – Social networks are human, coauthorship has long been studied

Computer networks are not generally social networks. Coauthorship studies are not new, but long-established, contrary to several assertions.

Other

N₄ – Culpable ignorance, miscellaneous

This covers anything else where one might plausibly expect the writer or speaker to know better, ranging from reasonably arguable to a synonym for “lie.” In some cases, it is used where a mis-statement of fact is clear, but one cannot prove that someone knew or remembered.

Reference page for Memes, Themes, other codes

Many Memes (arguments) were recognizable from past experience.

www.skepticalscience.com/fixednum.php , plus my a-h.

Meme-01	2	“It’s the sun” [sun]
Meme-02	1	“Climate’s changed before” [change]”
Meme-03	1	“There is no consensus” [consensus]
Meme-05	2	“Models are unreliable” [model]
Meme-08	2	“Ice age predicted in the 70s” [ice70s]
Meme-11	2	“CO2 lags temperature” [co2lag]
Meme-18	1	“Hockey stick is broken” [hockey]
Meme-20	2	“It’s Urban Heat Island Effect” [uhi]
Meme-21	1	“It’s just a natural cycle” [cycle]
Meme-24	2	“Water vapor is the most powerful” [vapor]
Meme-32	1	“We’re coming out of the Little Ice Age” [oldice]
Meme-36	2	“There’s no empirical evidence” [empirical]
Meme-56	1	“Medieval Warm Period was warmer” [MWP]
Meme-64	2	“It’s aerosols” [aerosols]
Meme-107	1	“Tree rings diverge from temperature after 1960” [diverge]
Meme-a	1	IPCC results depend mainly on 1999 hockey stick [ipcc=hs]
Meme-b	1	Paleoclimate peer review is poor, social networks [badpeer]
Meme-c	1	No independent verification, since some data shared [no-indy]
Meme-d	1	It’s one scientist (or a few) [one scientist]
Meme-e	1	Confounding factors everywhere [confound]
Meme-f	1	Faux fight between statisticians and climate scientists [faux]
Meme-g	1	General problem applies to all specific cases [generalspecific]
Meme-h	1	We’re statisticians, only asked to look at statistics, MBH99
Meme-j	1	Large uncertainty means almost nothing is known

Theme-? 4 **WR often ignores these good ideas.**

Science	A 4 – Avoid outdated sources.
Physics	B 4 – Energy is conserved on Earth, as elsewhere
	C 4 – The Greenhouse Effect is real, well-understood.
Climate	E 4 – Ocean oscillations are not forcings
Science	F 4 – Geography matters to surface temperature variability
	G 4 – The Medieval Warm Period varied spatially, temporally
	H 4 – Late 20 th -century warming is unusual, anthropogenic
Statistics	J 4 – Confidence intervals matter in real science
	K 4 – Big errors matter, small ones do not. Know which they are.
Sociology	M 4 – Social networks are human, coauthorship long been studied
Other	N 4 - Culpable ignorance, miscellaneous

Codes

Issues, W11.2, elsewhere

<e or E>	Error (minor/arguable or major)
<c or C>	Change of meaning (minor or major)
	Many might also be Issue-F 6, but are not categorized.
<b or B>.	Bias (minor or major), pro-MM, anti-MBH.
	<bB> is also used in W.8.2 as in selection of source.

Many Issues are combination, such as <ec> or <EB>.

This applies most often to Summaries, where changes are really obvious, but the coding is useful elsewhere. For **A.12**, the following were added, but not widely retrofitted to the WR, given the prevalence of some.

Issue-P 6	Clear plagiarism
Issue-p 6	Marginal, possible plagiarism, not counted in totals
Issue-F 6	Clear fabrication, wrong source or misrepresentation
Issue-f 6	Possible fabrication, often “did they really read this?”

References are coded in **W.8**, used to decide color codes on previous page:

R, r, u, U	Reference cited
	Clearly Referenced → clearly Unreferenced
g, G, G, G	Credibility if not peer-reviewed source
	Grey (popular press) → Beyond grey (fringe)
S, s, n, N	Relevant - relevance or lack thereof
	Should have been Summarized → clearly Not relevant
X	Referenced in [NRC2006], plausible source. Some references may have originated there or from MM+TT or Spencer.

Link- Link to likely sources, **W.8**, sometimes added elsewhere as hint to possible origin, especially for ideas lacking citations.

- M** 1 (21) Likely sourced from MM+TT or indirectly via Spencer. These are MM favorites given unusual emphasis in WR or references unlikely to be used in normal scholarship. Some are very grey, such as (vaguely referenced, but clearly influential) McK05, MM05x.
- m** 2 (31) Referenced by MM, clearly known to them, but might easily have been found through normal research.
- W** 5 Not used for the WR and related efforts, but in analysis of later papers that cite them as credible resource, such as [MCS2010]. Meme-b 1 5 indicates a red Meme sourced through the WR. These do not appear in the main Index, as **A.12** has its own local Index.

1 Background

§1 gives background on the attack on the hockey stick, as context for the WR. Those familiar with the topic can easily skip to §2.

1.1 Attack on the hockey stick

Anti-science strategies for bypassing science and causing confusion are well-known [MAS2010]. The newly published book by Oreskes and Conway [ORE2010] details the 20-year history of climate anti-science, especially from the George C. Marshall Institute (GMI), also a key organization that helped recruit, coach and promote MM well before Wegman was involved. Attacks on the hockey stick were under way in 2002, and the attacks were progressively refined through 2005, clearly articulated in the key MM05x reference.

One might start with the [IPC2007] SPM (Summary for Policy Makers), 18 pages long, from which next few charts are taken:

www.ipcc.ch/pdf/assessment-report/ar4/wg1/ar4-wg1-spm.pdf

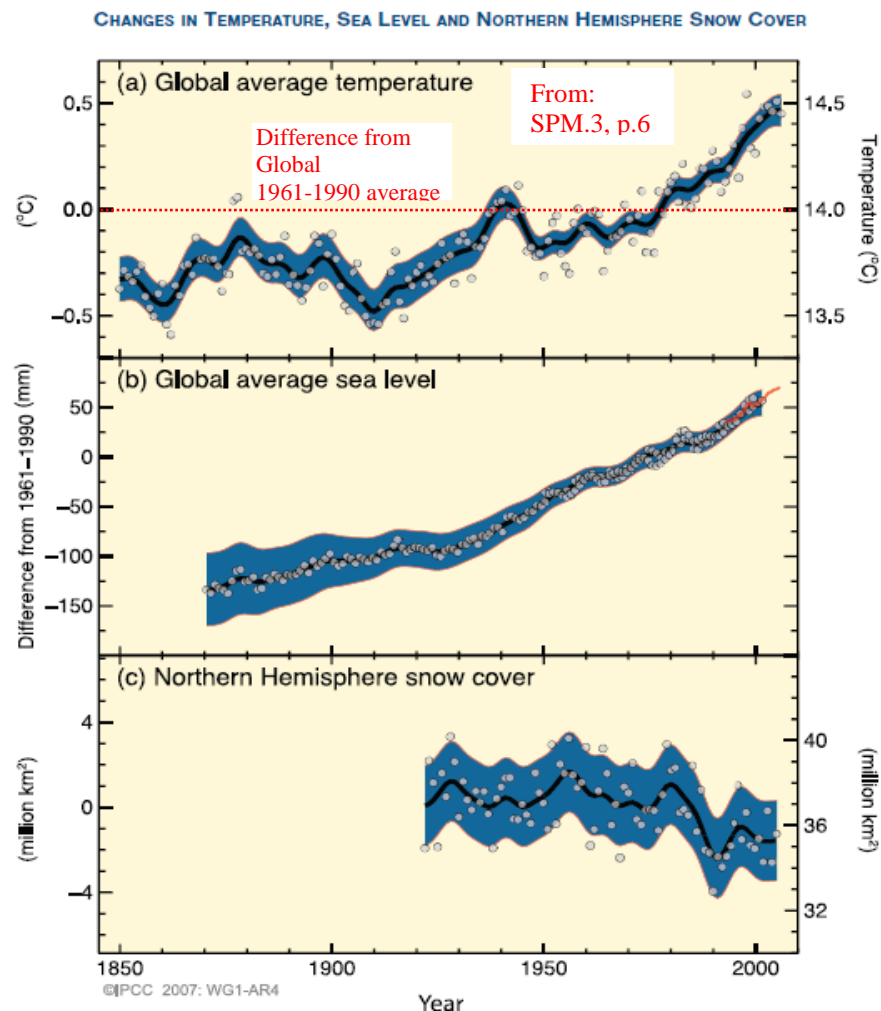
Almost without exception, science societies have a clear position on AGW:

- A. The Earth has been warming (with jiggles) for a century or more.
- B. Recently, most warming is caused by human-generated GHGs.
- C. Warming will continue, severity strongly influenced by human choice, with mostly negative consequences, bad ones on current trends.

Measurements, graphs, trends, jiggles

The chart at right illustrates important ideas (red annotations added).

1. All graphs have yearly data (circles), a smoothed trend line (black), and an *uncertainty interval* shown in blue. A wider blue spread means scientists have less or less reliable data.
2. All graphs have clear trends, up for (a) and (b), down for (c).
3. All have jiggles, because such trends are subject to various sources of *noise*. No scientist expects straight-line trends, especially since yearly noise (El Ninos, huge volcanoes) can exceed yearly trends, enough to need ~15-20 years to be very sure of trends. Properly computed 15-year negative trends have not been seen for decades. El Ninos can jiggle surface temperature strongly for a year or two, but are not in themselves long-term forcings. Some other ocean oscillations operate over longer periods.



Scientists continually argue about

- best estimates for each year
- size and nature of the jiggles,
- size of the uncertainties.

These sorts of arguments happen across science in analogous ways. People unfamiliar with a field may misinterpret some such arguments as fundamental disagreements on basics, but they are not, as is clear from *studying* references found in the WR, not just copying text from them.

1.2 Trends – natural and human factors

From [IPC2007, p.13] at right are NH temperatures relative to average temperature in 1980-1999, not immediately comparable to the previous chart (global relative to 1961-1990), but representative. Black is historical, with gray uncertainty range. The others show projections with their uncertainty ranges for different levels of future GHG emissions chosen by humans. None of these predict exact tracks, due to natural variability (noise). Early drafts [IPC2006] were available to the WP.

Basic physics by paper-and-pencil

Sunlight is absorbed by the Earth. Some is re-emitted as heat radiation. GHGs (CO₂, CH₄, N₂O, water vapor) absorb such radiation and transfer energy to nearby molecules. Some energy is re-emitted Earthward, a good thing as temperatures would be uncomfortably lower otherwise. In 1896, Svante Arrhenius roughly calculated the warming expected from doubling pre-industrial atmospheric CO₂. He was a little high, near the edge of current uncertainty ranges, but not far off, not bad for 1896. See Spencer Weart’s excellent history, Theme-B⁴, Theme-C⁴
www.aip.org/history/climate/co2.htm,

Computer models help reduce uncertainty limits and improve regional projections, but the basics are fairly straightforward.

Energy is neither created nor destroyed. GHGs slow emission in the regions of the spectrum where GHGs absorb thermal radiation. As GHG concentrations rise, the surface warms to maintain balance between incoming solar radiation and thermal radiation to space. Most incoming solar energy is first absorbed in the oceans, measured as Ocean Heat Content, which is increasing. Ocean oscillations cause more or less heat to be returned to the atmosphere, so El Nino years cause warmer atmospheres. Still, energy is always conserved. Unlike some areas of statistics or economics, physics has strong conservation laws, which are not mere correlations. Earth’s energy balance does not change quickly without reasons, called forcings, such as changes in solar irradiance, GHGs, aerosols. Earth’s orbital changes matter, but occur slowly.

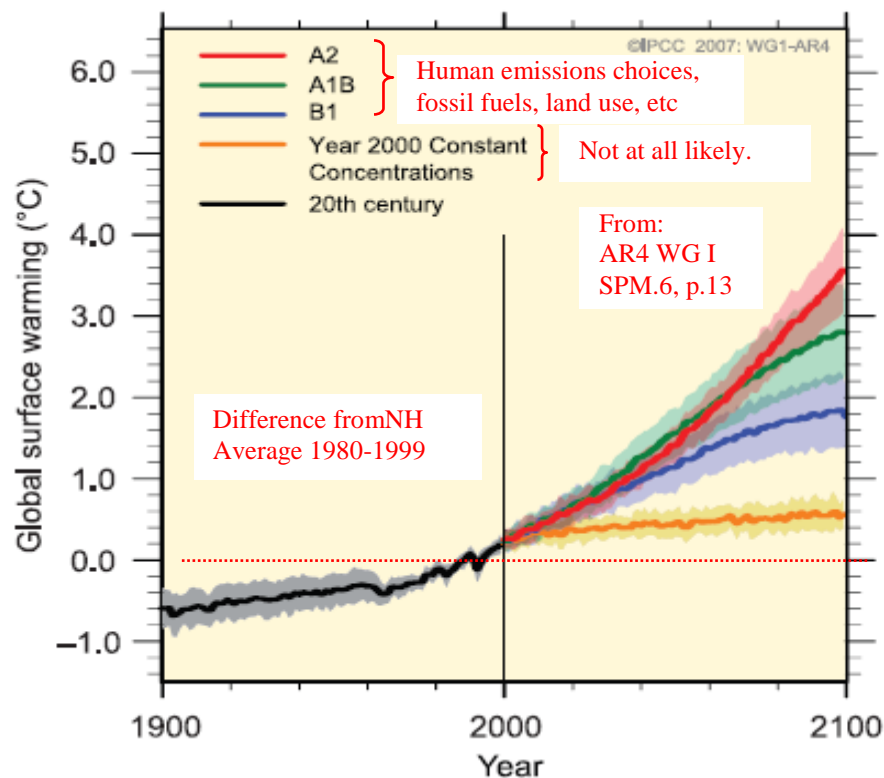
Past and future

The Earth’s future temperature track will be determined by:

- The current state of the Earth, especially total energy content, glacier masses, vegetation coverage and other factors that affect Earth’s

overall albedo, i.e., fraction of energy reflected into space without creating heat. Ice and snow reflect more than oceans.

- Biological, chemical and physical processes.
- Human choices, with “public policy implications, as in [SAI2007, p.5].



Future natural temperatures simply do not depend on the temperature in 1000AD or on our knowing anything about it. Wegman even said this in testimony, A.3. However, better understanding of the past’s natural variability helps researchers calibrate climate models, which is why researchers argue fiercely over the shape and jiggles of the shaft. If people somehow got a full set modern-grade temperature measurements from 1000AD onward, nothing would change for the future except our ability to forecast it better.

The uncertainty limits on each emissions scenario represent huge differences of impacts and costs, so narrowing those limits helps inform human choices.

1.3 Hockey sticks in the IPCC

At right is a sequence of IPCC charts, most available in final form to the WP, except the last, which was available in Draft form, **W.4.4**.

1. [IPC1990, FAR] Figure 7.1.c

This was a sketch, derived from work decades before. The IPCC knew they did not yet know very much. The WR promotes this heavily, **W.4.2**. scienceblogs.com/stoat/2010/06/ipcc_1990_fig_71c_again.php pubs.giss.nasa.gov/docs/2009/2009_Jones_etal_2.pdf Appendix A, p.36.

2. [IPC1995, SAR, p.175] Figure 3.20.

This used a few early studies, lacking uncertainty limits. They knew they knew little pre-1400, but the curve fits the TAR's grey zone. The hockey stick was already starting to appear, and the chart above was gone in 1992. andy russell.wordpress.com/2010/06/15/the-hockey-stick-evolution

3A. [IPC2001, TAR , p.134] Figure 2.21. The main report is 800 pages. MBH99 is the black line, especially known for first computing (grey) uncertainty limits, correctly larger pre-1400AD, given less good data. Red and green lines represent other studies ("spaghetti"). They mostly agree with MBH99 as they mostly lie within the grey. They sometimes disagree, mostly in the depth of the LIA, for which plausible reasons have been given, often in WR-cited papers, Theme-F, Theme-J.

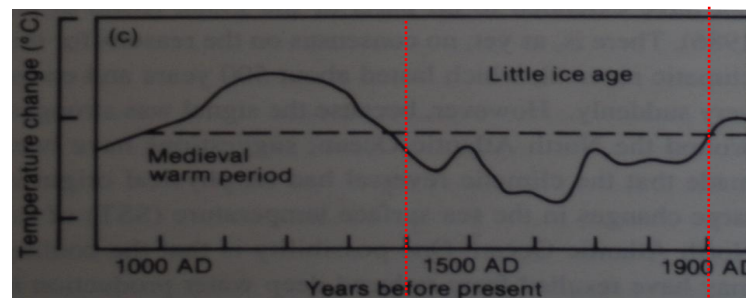
3B. [IPC2001, TAR]TS Figure 5 p.29 or SPM Figure 1 p.3.

This is the famous hockey stick, simplified from 3A, especially for use in the 18-page Summary for Policy-Makers (SPM).

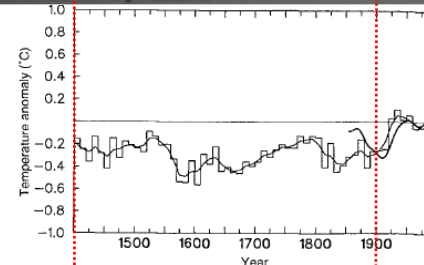
4. [IPC2007, AR4, p.467] Figure 6.10

Most of this chart's papers were published no later than 2006, 7 were cited in the WR, and a Draft was already available [IPC2006]. I have overlaid it with the grey uncertainty limits from 3A. Studies tended to be near or below MBH99, especially during the LIA. If one prefers the others, they would lessen the MWP and make 20th-century warming look even stronger, **W.4.4**. Some of these lines cover different geographies, and curves *should* differ. This chart sequence shows normal progression in science. All this work is concerned with reconstructing the "shaft" or "handle" of the hockey stick, not the "blade" derived from modern measurements.

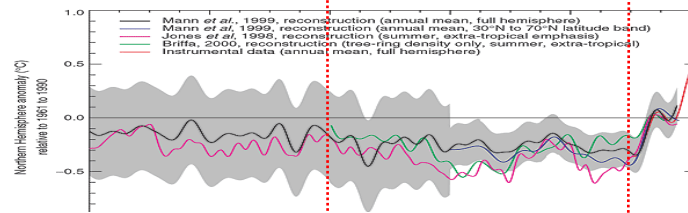
1. 1990



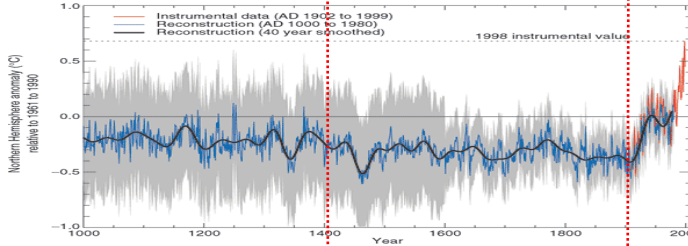
2. 1995



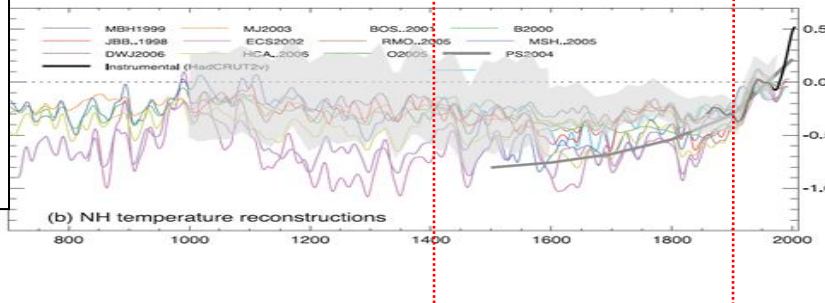
3A. 2001



3B. 2001



4. 2007



1.4 Why do people care about the shaft?

Understanding gained from studies is shown first, split into 3 main eras, showing important elements of science for each. Biology, chemistry and physics inform understanding for all eras. Models and data are continually used to cross-check each other, so scientists care about the past. A straighter hockey shaft implies higher climate stability, whereas stronger jiggles indicate higher sensitivity to CO₂ changes, less stability and higher chance of more extreme (bad) results. Oddly, those who attack the MBH hockey stick for its straightness, ought to like it, as it argues for lower CO₂ sensitivity than inferred by curves with stronger variations.

But of course, the attackers do not like it. A possible rationale for the attack is shown in sketches at right, roughly combining earlier charts. Suppose “avoid CO₂ regulation at all costs” (E) is one’s goal, from:

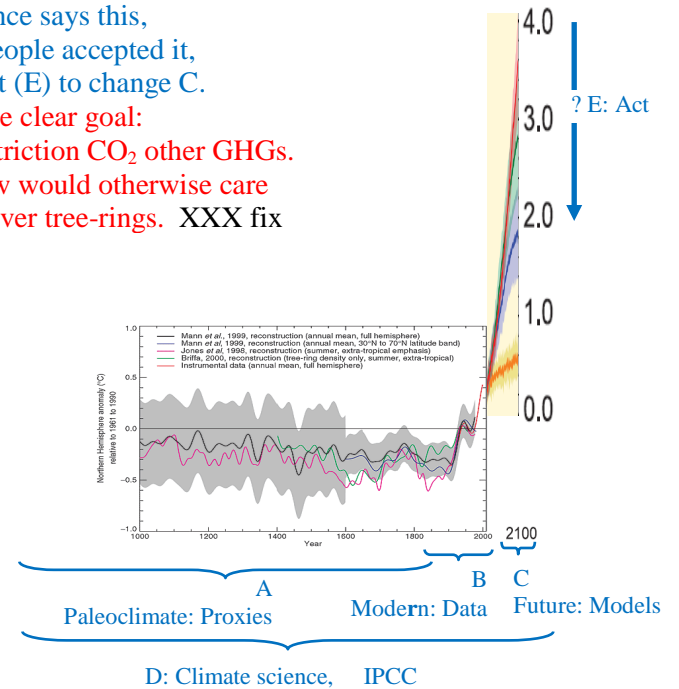
- Strong interest in using or especially selling fossil fuels
- Ideological opposition to government regulation of *anything*
- Any of the many other reasons in [MAS2010, Figure 2.6].

It is not easy to attack the modern temperature record (B, the “blade”), although some try. It is really difficult to attack basic physics, good enough for approximate answers, although some try that as well. The shaft (A) of the hockey stick really does not matter to policy and the blade (B) of the stick is solid. Models (C) that predict temperatures as function of emissions choices are already good enough, getting better and are mostly needed for regional understanding and more accurate forecasts.

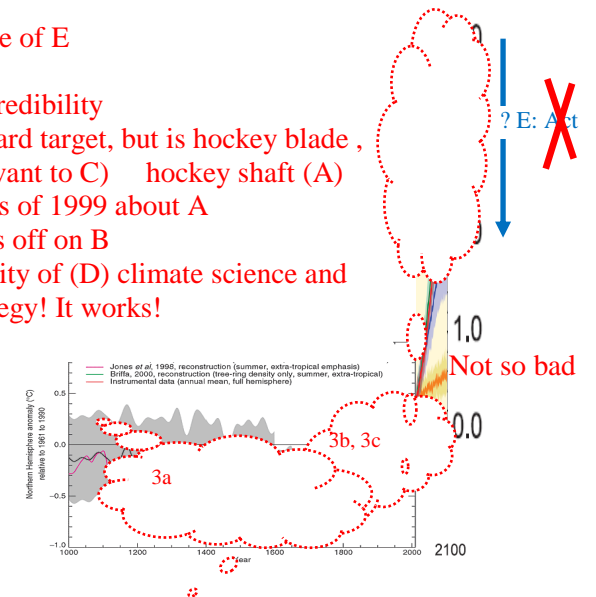
“Science bypass” is based not on doing real science, but on confusing the public, a well-honed tactic developed especially for the tobacco industry, [MAS2010, §1]. Arguments over statistical minutiae raise doubts and confusion in the general public. It works well to invoke arguments accessible only to experts.³ Ideas are often packaged as anti-science Memes, repeated endlessly although long ago debunked. Many seem to attack (A or B) or (D) on credibility of climate science or the IPCC, but the real goal always seems the avoidance or at least delay of inconvenient action (E) (“mitigation) in favor of “adaptation” alone, *usually meaning that someone else adapts, later or elsewhere.*

³ Of people who are sure that MBH99 is a fraud due to decentered Principal Component Analysis (PCA), what fraction had even heard of PCA before?

Our best science says this,
but if more people accepted it,
they might act (E) to change C.
But some have clear goal:
avoid any restriction CO₂ other GHGs.
Relatively few would otherwise care
about fights over tree-rings. XXX fix



- 1) Avoid acceptance of E
 - 1a) Attack models
 - 1b) Attack IPCC credibility
- 2) B increasingly hard target, but is hockey blade,
- 3) Attack (~~irrelevant to C) hockey shaft (A)
 - 3a) Attack statistics of 1999 about A
 - 3b) Confusion rubs off on B
 - 3c) and on credibility of (D) climate science and IPCC ... Good strategy! It works!



1.5 Evidence versus presentation

The development of science can be compared to the construction of a Great Wall [MAS2008a]. Scientists add new bricks to the wall via research publications. Some bricks get kicked away quickly, problems are found later with others, but over time, the Wall of Science builds on masses of interconnected evidence. As much as some would prefer, the Wall does not suddenly collapse because old bricks are found imperfect, especially amidst accumulated cement and steel rebars.

The Economist's March 18th 2010 “The clouds of unknowing” used a similar comparison: “jigsaw puzzle” versus “house of cards”:

www.economist.com/node/15719298 (paywall)

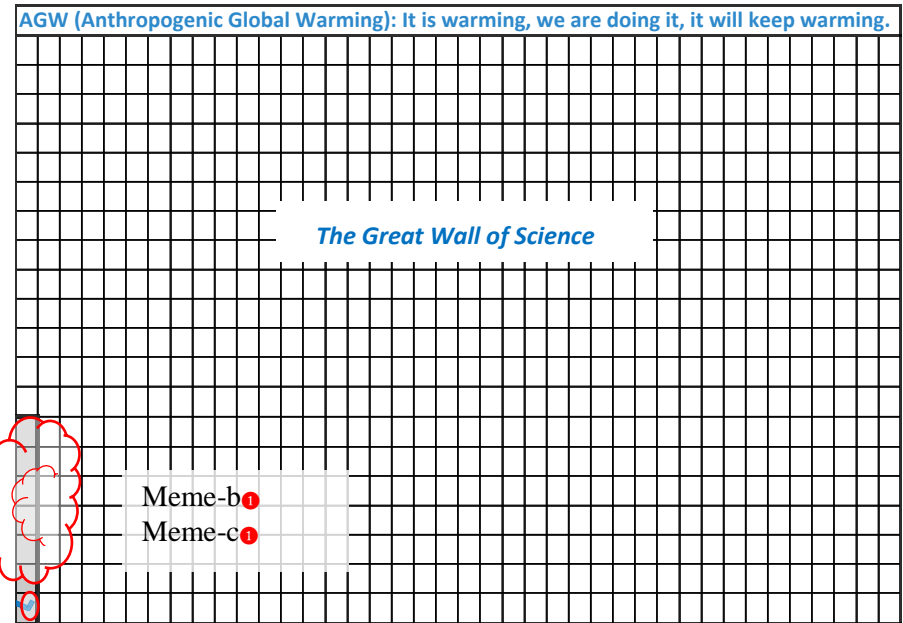
At right are two whimsical sketches. The first illustrates the Wall analogy. The main report of [IPC2001, WG I] is 800 pages long. The various (not just MBH) reconstructions of the last 1000 years are covered in about 7 pages of the 800 of the full report, pp.130-136, shown as the 7 grayed bricks in the 40x20 wall. Suppose two bricks at bottom represent MBH work, and the bottom brick the complaints of MM against the MBH98/99 hockey stick, although actually it should be a tiny fraction of a brick.

The anti-science view (Meme-b₁) treats the hockey stick as a key pillar of AGW, here represented as the elephant precariously balanced atop perfect correctness of the hockey stick in one old paper (MBH99, as per Inhofe [MAS2010]). Focusing primarily on Man is an example of Meme-d₁.

Other researchers have found essentially similar results to MBH99, within the uncertainty range. A reasonable person might have said in 2005:

“MBH99 is 7 years old, many others have obtained results from different methods and combinations of data that fit within uncertainty. If there are errors, do they make a difference or not? Is everybody wrong?”

Meme-b₁, Meme-c₁ are cartooned as the red cloud covering the lower left corner of the Wall. Although the WP claimed narrow focus on MBH-vs-MM, it spent many pages on a strange trip into *human* social networking, a topic in which the WP seemed to have little prior experience **W.2.3, W.5**. Meme-b₁ appears in **W.8** (WR summary of MM05a) and seems especially derived from (uncited) MM05x, **W.8.9**. Meme-c₁ is a related, but subtly different attack on data independence, *very likely* derived from MM05x or McIntyre’s website, **W.5.8, W.5.9**. **All this combines missions #1 and #2.**



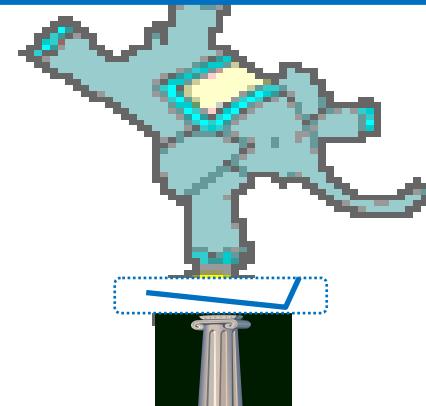
Science

Two views of the hockey stick

Anti-Science

Meme-a₁

AGW (Anthropogenic Global Warming):
It is warming,
Humans are causing most of the recent warming
It will keep warming, with mostly negative effects



Meme-d₁

Mann

2 Wegman report, from bottom to top

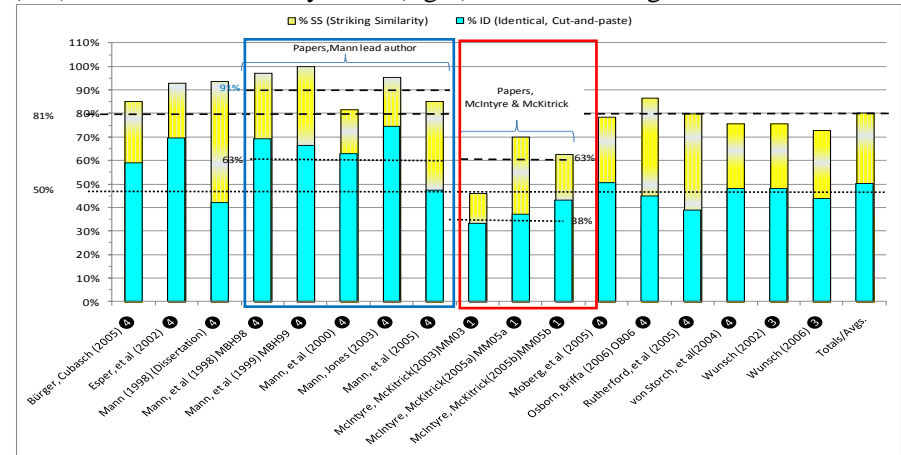
2.1 Important papers - bad summaries

DC's work started this by finding about 10 pages of plagiarism and other WR problems, **W.2**. **W.11.8** adds another 25 pages, dissected in the format below. Skeptical readers are welcome to check all 35 pages, *but I suspect most will read no more than few before the repetitive style gets tiring*. I had to do this to gather and summarize the data. Most people need not.

Serious scholarship might start with key peer-reviewed papers, then follow related citations, ideally with help by field-knowledgeable experts, but the WP did not consult climate scientists, Theme-N. The WR devotes 26 pages to Summaries, few relevant to MBH statistics, Meme-h. *These Summaries seem to exist mostly as camouflage for the papers that matter, those written by MM, whether cited or not*. Most papers' conclusions are ignored, even when Summarized.

The scholarship of these Summaries shows, not expertise, but its absence. About 50% of the total text is identical (cyan below). Add trivial changes (yellow) plus other word moves or minor rephrasing (white regular font), for a total of 81% Striking Similarity. **Even allowing for differing summarization practices, this is (not very competent) plagiarism.** **W.11** gives side-by-side comparisons of WR Appendix C, of which the examples below are truly representative. Widespread Errors, Changes of meaning and Biases jump off the page, as cyan text quickly gets ignored. Wegman's own testimony raises doubts about his careful (or any) reading of some Mann-led papers or even the Summaries, **A.1.3**.

This small version of **W.11.4** chart illustrates the overall pattern of cyan, (yellow+white), and the big difference between Mann and MM. Mann (left) was treated cursorily, MM (right) far less so. Higher bars are worse.



This mixes plagiarism with Changes of meaning and Bias, akin to **W.2.1**. Biases strengthen MM, weaken most others. Other likely WR plagiarism cases seem more straightforward, with merely accidental errors, **W.2.2**, **W.2.3**, **W.5.7**. This plagiarism style strongly resembles that in Said's dissertation, cut-and-paste with trivial changes that introduce silly errors, **A.9**. The consistent style hints at single-person authorship. It would truly odd for 26 pages to be written by anyone not credited as an author. It is not Scott, seems quite unlikely to be Wegman, so the evidence fits Said. *The Summaries do not even seem very well proofread. Why bother?*

<p>WR, p.69, Paragraph 2 Overall the network includes 112 proxies, and each series has been formatted into annual mean anomalies relative to the reference period used for this data, 1902-1980. Certain tree-ring datasets have been represented by a small number of leading principal components.</p>	<p>MBH98, p.779 The long instrumental records have been formed into annual mean anomalies relative to the 1902-80 reference period, ... Certain densely sampled regional dendroclimatic data sets have been represented in the network by a smaller number of leading principal components...</p>
<p>WR, p.80, Paragraph 4 They also note the limited due diligence of paleoclimate journal peer review and that it would have been prudent to have checked the MBH98 data and methods against original data before accepting the findings as the main endorsement of the Intergovernmental Panel on Climate Change. 1. <CB>. MBH98 as the main endorsement of the IPCC? This is a major Change of Meaning, plus Bias, hence <CB>.</p>	<p>MM05a, p.90 recognizing the limited due diligence of paleoclimate journal peer review, it would have been prudent for someone to have actually checked MBH98 data and methods against original data before adopting MBH98 results in the main IPCC promotional graphics. The "results in the main IPCC promotional graphics" part seems fair. The WR made an explicit change amidst ID text.</p>

2.2 Bibliography – padded and strange

The 80 WR references, pp.53-59, are analyzed in **W.8**, whose 3-page **W.8.2** gives a dense listing.⁴ More detailed comments are given on some that were interesting and easily available, ~50 of 80, **W.8.8**.

At most 40 of the total 80 references are even mentioned (cited)⁵ in the text. Some of the other 40 are cited in weak ways, incurring doubt about careful or any reading. Normal scholarship frowns on large numbers of uncited references as “bibliography padding”:

Google: academic ethics padding bibliography

A few uncited references may be innocuous, especially in a long paper, as someone may delete a citation but forget to delete the reference. It is quite reasonable to include “Further Reading” or the equivalent in annotated bibliographies. But it is very strange for 50% of references to be uncited.

This is often a plagiarism tip-off in academe, as citations are removed to make text look original or people may include references not studied or even skimmed, in order to create a façade of expertise. A few seem present only to cover usage in WR Figures 5.8,-5.9, **W.5.8**, **W.5.9**.

In any case the WR has an odd mixture of references. Credible papers are referenced, even cited, but conclusions that contradict the WR viewpoint are ignored or weakened. Two credible, but irrelevant references are mangled together into something nonexistent. Some are wrongly categorized, others may be considered dubious sources. Many seem irrelevant. Some are OpEds by well-known climate anti-science advocates. Some seem to be included only to carry common climate anti-science Memes. One is an economics working paper with 17 lines of MM views. One is a 1987 fringe-technology-journal article by a long-time writer of pseudoscience and conspiracy theories.⁶ It cannot possibly be relevant.

⁴ On-line readers may find it useful to print those 3 pages, to follow the WP references mentioned often. Not everyone knows these by heart.

⁵ en.wikipedia.org/wiki/Citation, “More precisely, a citation is an abbreviated alphanumeric expression (e.g. [Newell84]) embedded in the body of an intellectual work that denotes an entry in the bibliographic references section of the work”

⁶ Tom Valentine in MAGNETS, #52 in **W.8.2**, **W.8.8**. *I was unable to find that!*

This Bibliography fits a clear mission to ratify MM and discredit climate science, although not executed very competently. Recalling that many papers came via Spencer, *presumably originally selected by MM+TT*, [SAI2007, p.9] says:

“Reviewed some 127 technical papers related to paleoclimate reconstruction.”

A plausible scenario, consistent with the evidence is:

- First read the very grey MM05x¹, a presentation and meeting for GMI, May 11, 2005, and perhaps McK05 from a week earlier. These provide many Memes and strategies that appear in the WR. Some content could never survive credible peer review. These are referenced (vaguely), never cited. Some effort is needed to find the original date and sponsor of MM05x (GMI), if not previously known, and no connection with GMI appears anywhere in the WR. Wegman was contacted 09/01/05, and the WR oddly dates MM05x as 09/07/05, not the original 05/11/05. *That hints that it was one of the early papers from Spencer*¹.
- Actually study the 3 MM references (MM03, MM05, MM05b)², whose Summaries show the least cut-and-paste plagiarism. Wegman and the WR show much more familiarity with these and their comments on MBH, then with MBH papers themselves.
- Follow McIntyre’s Climate Audit blog, perhaps¹.
- Read some (credible) papers, but with minimal comprehension. Summarize some, but often ignore their clear conclusions⁴.
- Add more papers for bulk and an illusion of scholarship³, although perhaps without even studying them. One irrelevant pair is mangled.
- Pad the Bibliography with irrelevant climate anti-science Meme-carriers, like Anderson, et al (2005) and the bizarre Valentine (1987) ozone reference².

Like the Summaries, most references seem camouflage for the few that count, those by MM, including the influential, but uncited MM05x. Many are completely irrelevant to the supposed mission to study MM vs MBH. This just presents a scholarship facade, until one looks very closely.

If this seems harsh, see the 20-page analysis in W.8.

One would expect the Summaries and Bibliography to be done by the same person, most likely Said. Like the Summaries, the Bibliography does even seem cursorily proofread.

2.3 Literature review

The entire text of WR pp.23-27 is shown in **W.3**, with interspersed commentary. Many Memes, Themes, Errors and Biases appear, unsurprising, since it summarizes the error-laden WR Appendix C and Bibliography. These include multiple misunderstandings of basic physics and the strong innuendo:

“The variables affecting earth’s climate and atmosphere are most likely to be numerous and confounding. Making conclusive statements without specific findings with regard to atmospheric forcings suggests a lack of scientific rigor and possibly an agenda.”

This combines culpable ignorance with an insinuation of wrongdoing, Theme-N[Ⓢ], Meme-e[Ⓢ].

In any case, Conclusion, Recommendations and Executive Summary often ignore the Literature review and the Summaries on which it is based. So does some of Wegman’s testimony. Again, at a glance, this looks like normal scholarship, but is not. It is just another part of the façade.

2.4 Reconstruction – strange graphs

WR §4 implements #1 of the 2 main missions for the WR: ratify the statistical attack by MM, especially as seen in MM05x.

WR, p.7:

“To this end, Committee staff asked for advice as to the validity of the complaints of McIntyre and McKittrick [MM] and related implications. ...”

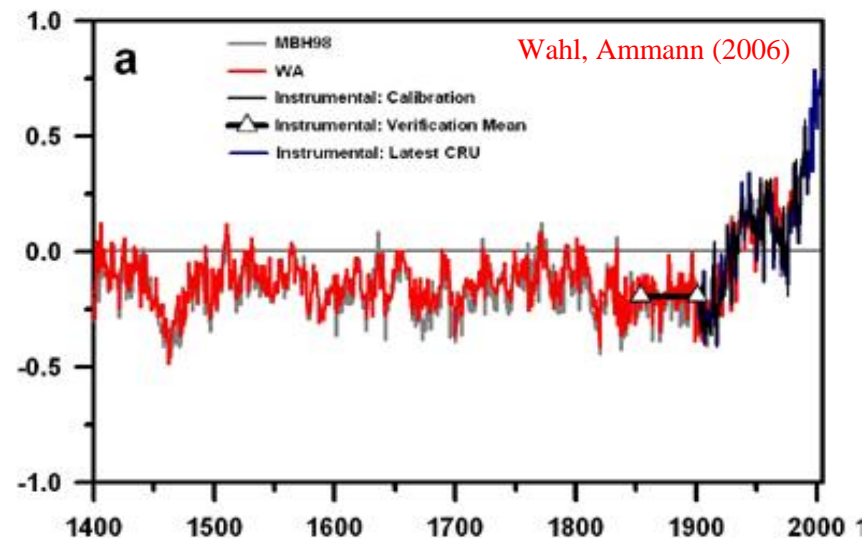
The WR is written by statisticians supposedly to evaluate MM-vs-MBH, statistically. One would expect this WR §4 to provide that, but instead, it reiterates why MM must be right and MBH wrong, reproduces various MM graphs and strongly promotes a distorted version of a 1990 IPCC graph discarded by 1992, **W.4.2**. **W.4.2-W4.4** collect various related strange graphs and graphical contradictions in one place. The reader might want to examine these. Statistical arguments are difficult to follow, but graphs make strong impressions, and can easily be misleading.

Recomputing the MBH-decentered PCA with proper centering is the obvious task, as suggested by Cressie, and independently done by Wahl, Ammann (2006), whose following chart is discussed more in **W.8.4**. The PCA decentering only affects a small fraction of the data for the early time period. They computed the red curve with that fixed, some data problems

fixed, but also with correct selection procedures for the number of proxies. *If all that is jargon, do not worry.*

Just look at the following chart and compare the (red) line, done in 2006, with the original (grey) version done in 1998.

This whole fuss is about the difference between grey and red. Of course they differ slightly, for good reason. Does this matter?



The WR does not do this, Meme-f[Ⓢ], Theme-N[Ⓢ], but the topic is discussed in some detail in [WEG2006c, pp.10-15]. They cite Wahl, Ammann (2006) only in a footnote, to discredit it. Later, in response to Rep. Stupak, we find:

[WEG2006c, p.11]:

“**Ans:** The Wahl and Ammann paper came to our attention relatively late in our deliberations, but was considered by us. Some immediate thoughts we had on Wahl and Ammann was that Dr. Mann lists himself as a Ph.D. co advisor to Dr. Ammann on his resume. As I testified in the second hearing, the work of Dr. Ammann can hardly be thought to be an unbiased independent report.”

That simply does not address the statistics issues claimed to be the purpose of the WR. As shown the **Page tally §2.7**, the WR actually provides zero useful new statistical analysis of the hockey stick, for which peer-reviewed articles were promised in 2006, **A.1.3**. They have yet to appear.

2.5 Social Network Analysis

One of the strangest parts of the WR and follow-on activities was the excursion into Social Network Analysis (SNA), a well-established discipline in which the WP had done little previous research.⁷

All this seems aimed to support Meme-b, not merely to attack the hockey stick, but to discredit paleoclimate in general, §1.6. WR §5 implements the mission #2 to complement WR §4. Wegman insisted on its inclusion, despite urgings to the contrary, A.11.2, slide 19.

WR, p.7:

“We will also comment on whether issues raised by those criticisms discussed in McIntyre and McKittrick (2005a, 2005b) raise broader questions concerning the assessment of Mann et al. (1998, 1999) in **peer review** and the IPCC ...”

WR§2.3 (**W.2.3**) plagiarizes several textbooks, generally without Bias, although not without occasional Errors. Some of the same text is re-used in [SAI2008] and again in several PhD dissertations, [SHA2008, REZ2009]. **W.3.2** shows the changes from one version to the next, including some tip-off errors: “statues”→”statues” (3 times)→”states.” The “statues” example is simply ludicrous. As a side-effect of reexamining documents, Said’s dissertation [SAI2005]⁸ was found to have 5 pages of plagiarized ethanol discussion, done in a similar style, **A.9**.

WR§5 (**W.5**) uses relatively unsophisticated, incomplete SNA to claim, with no evidence, that the paleoclimate coauthorship shows likely peer-review problems there, in essence, guilt-by-association as Mann is labeled as playing a central role.⁹

Having been criticized for this, in 2007 they published an analysis contrasting the paleoclimate net with Wegman’s, using the odd idea that a

⁷ [RIG2004] had used SNA terminology to analyze computer networks, but that is not SNA, just re-use of similar underlying graph theory. [SAI2005] had one reference to an SNA paper of no relevance to coauthorship analysis. Hence, they knew about SNA, but had not really done research there.

⁸ [SAI2005, SHA2008, REZ2009] all won departmental Outstanding Dissertation Awards.

⁹ In a study of one person’s group of coauthors, that person will indeed have “high centrality.” This is no surprise. Good SNA research looks at larger networks, not just those focused on one person’s coauthors. The WR actually has a bit of that, Figures 5.6-5.7, but the results do not support the claims.

person’s network is limited to coauthorship. The network of Mann (1 year post-PhD at time of MBH99) was claimed more likely to be subject to abuse than that of Wegman, a well-networked senior researcher. They made strong claims unsupported by the data, with poor references. It was sent to a non-SNA journal, *Computational Statistics and Data Analysis*, and accepted in 6 days, unrevised, compared to a 200-day average. Possibly, this was helped by Wegman’s 20-year tenure as an advisor and Said’s role as an Associate Editor at the time, 2 years post-PhD.

Elsevier’s sister journal *Social Networks* would have been far appropriate, but awkward. Its editorial group included several people who were authors of the plagiarized texts, or colleagues of such authors. *Even without the plagiarism, this paper would have been very unlikely to have gotten far.*

Mere association is never evidence of guilt, but since the WR raises this issue, **A.6** studies a few of Wegman’s subnets, not just the coauthorship net. It simply shows the absurdity of someone with Wegman’s large, strong, multi-decadal network attacking that of recent PhD Mann.

2.6 The rest of the WR

W.1 describes the straightforward introduction.

W.2 describes mostly plagiarized text identified by DC. **W.2.1** introduces serious Biases in plagiarizing Bradley (1999) on tree rings. **W.2.2** strangely plagiarizes various sources on PCA and statistics, with some Errors. Was PCA unfamiliar to the writer of this? **W.9** is a straightforward, mathematical description, unlike anything else in the WR, apparently as it was the only part written by Scott.

W.10 discusses some of the answers to questions from Rep. Boehlert.

Then, **W.6** and **W.7** analyze Findings and Recommendations, much of which is then re-used as shown in **W.0**, the Executive Summary, usually written last. When faced with a 91-page science-seeming report, many people read little more than the Executive Summary, Findings and Conclusions. Those must emphasize key messages and they do. The next page consolidates all this into one table.

2.7 Page tally

This section tallies the use of pages in the WR. About 60 WR pages are spent on paleoclimate review / discussion and SNA topics in which the WP demonstrates little expertise. Another 11 pages are just acknowledged copies from elsewhere. Appendix A is fine. Other pages list uncited or irrelevant references, show graphs or reiterate MM's critiques of MBH. What's *missing* is serious peer-review-quality statistical analysis of MBH,

W.8.4. Plagiarism and <eEcCbB> issues are detailed elsewhere, as are overall flows from antecedent sources, in **A.0**. CAVEAT: page count is not proportional to visual appearance here. Almost half the pages are white, i.e., blue/green sources, but with red insertions. This is a science-seeming façade for anti-science messages (red), relying on statistics many people cannot follow, a common technique for doubt creation
WR §4 and §5 implement missions #1 and #2.

Page Tally by Topic and Attributes				General	Paleo	Math Descr	MM	Statistics	SNA	Find, Rec	Plagiarism	Copy, Ack'd	<eE> Errors	<cC> Changes	<bB> Biases	Key takeaway messages: --Errors, Changes, Biases pervade the WR --Many pages are red, and white boxes include red
Wegman Report Sections	WR Pages	WR Page Count	Likeliest Writer(s)													
(0.) Exec background	2	0.5		0.5											2	Legend General text Paleoclimate Math Description (not doing math) MM replication or related charts Statistics Relevant statistical analysis of MBH that might pass peer review SNA - Social Network Analysis Findings, Recommendations Plagiarism Pages with substantial SS Copy, Acknowledged Pages mostly just copied, Ack'd, so not plagiarism. Not new work, does add bulk. <eE>, <cC>, <bB> issues Page counts done to .5 page, as full, for simplicity. *See [DEE2010j], the "1"s just note presence, not counts. @This isn't Statistics, but might not be exactly MM, either. Graph likely came from MM. # Counts only unSummarized references + White = mix of blue/green with red/orange insertions & Grey: neutral, straightforward
(0.) Exec Global Climate	2-3	1.0			1.0								1	1		
(0) PCA, CFR, CPS	3-4	1.0				1.0									2	
(0) Findings	4-5	1.5								1.5			1	9		
(0) Recommendations	6	1.0								1.0					1	
1. Introduction & Copy of Barton/Whitfield	7-9	2.0		1.0							2.0				1	
2.1 Paleo background	10	1.0	Said		1.0							2	1	1		
Tables from Bradley & Text from Bradley (tree rings) *	11-12	2.0	Said		2.0					1.5	2.0	3	1			
Text from Bradley (ice, coral) &	13-14	1.5	Said		1.5					1.0		1	1	1		
2.2 Background on PCA*&	14-15	1.0	Said		1.0					2.5		1	1			
2.3 Background on SNA*&	15-17	2.0	Said			2.0				5.5		1	1			
3. Literature review, from App. C. +	17-22	5.5	Said						5.5			1	1			
4. Reconstruction, PC Methods	23-27	5.0			5.0							7	7	16		
" " "	28	0.5			0.5					0.5		1		1		
Replicate MM graphs	28-29	1.5				1.5						3				
Copy/modify IPC1990	30-33	4.0					4.0			3.0						
Make noisy IPC1990s @	34	1.0					1.0			1.0		1				
5. SNA	35-37	3.0					3.0									
5.8 Figure	38-45	8.0	Said						8.0						1	
5.9 Figure			Rigsby													
6. Findings	46	1.0	Mcl?				1.0					1		1		
7. Recommendations	47	1.0	Mcl?		1.0					1.0		3	3	3		
(8) Bibliography +	48-50	3.0										5		11		
Appendix title page &	51-52	2.0										1		4		
(9) App. A, PCA math &	53-59	7.0	Said		7.0							8		30		
(10) App. B. Boehner	60	1.0		1.0												
(11) App C. Summaries +	61-63	3.0	Scott			3.0										
Total pages/pages per topic	64-66	3.0		3.0						1.5		3	7			
% pages per topic	67-92	26.0	Said		26.0					25.0		33	24	37		
		91		7.5	46.0	7.5	9.0	0.0	13.5	7.5	35.5	11.0	76	39	129	
				8%	51%	8%	10%	0%	15%	8%	39%	12%				

At this point, discussion shifts to related topics not directly corresponding to WR sections, but based on the previous analysis.

2.8 Comments, but hardly reviews

Its review did not resemble NRC procedure in the slightest, covered in detail, **A.1**. The WP sent it to a few people, all well-known to Wegman, some then quite close, some not. Some got a few days' notice, at least some dissented, but were ignored. **Some commenters were clearly mis-used and surprised to be labeled "reviewers."** Had I time to dig deeper, I would not be surprised to find more, but enough evidence already exists to show a serious problem, including repeated efforts to present the WR to Congress as being like an NRC effort.

Wegman's testimony is sometimes confused or contradictory about this. Promises were made of forthcoming peer-reviewed work, **A.1.3**, none of which seem to have happened, except the problematical [SAI2008], **W.5.6**.

As an example of the long-term persistence of WR credibility and misinformation, climate scientist Judith Curry writes, 04/25/10 in: www.collide-a-scape.com/2010/04/23/an-inconvenient-provocateur/#comment-3198

"Specifically with regards to the Wegman Report, this was a paper that had been commissioned by a Committee of the US House of Representatives, peer-reviewed in exactly the same way as NRC 2006, and was read into the House record on 17 July 2006."

It was indirectly commissioned by Barton/Whitfield through Coffey.

2.9 Moving on, or not

[**BAR2006a**, p.134] Wegman says:

"In a real sense the paleoclimate results of MBH98/99 are essentially irrelevant to the consensus on climate change. ... We think it is time to put the 'hockey stick' controversy behind us and move on."

If MBH98/99 was irrelevant, the whole WR makes little sense, but in any case Wegman did not move on, **A.3**. He and Said gave climate-science related talks over the next few years, in a few cases seemingly paid by government contracts. Related talks were given at several conferences, organized through a Wegman-led small organization, **A.6**. Others were given to audiences unlikely to include many climate-knowledgeable people, Meme-f¹, Meme-g¹.

The one talk given to an expert audience in 2007, was not well-received, **A.4**. It was inappropriate, perhaps sometimes gratuitously offensive to experts, whom Wegman late seemed pleased to have irritated. It even "borrowed" 3 slides from Mann, used without attribution and out of context, in fact, essentially reversing the original sense of his talk.

Interface 2010 (a long-running small statistics conference) was organized by Said and Wegman, **A.6.4**. Seemingly at the last minute, they created two new sessions filled mostly with professional climate anti-science advocates like Fred Singer, Jeff Kueter (GMI), and Don ("imminent global cooling") Easterbrook. It seems unlikely this was the first contact between them, **§3.3**. Said gave a talk on "Climategate."

"The lack of transparency by some climate researchers, the willingness to bend the peer review process, and the willingness to destroy data rather than share it with researchers of a different perspective all raise fundamental issues of climate change policy."

It is difficult to understand how all this is "moving on."

Regarding data destruction, Said's informative public seminar at GMU 09/07/07 has been online for years unchanged, and listed in several seminar schedules. In August 2010, the file disappeared and the reference was edited out of one seminar history, and her PhD dissertation also disappeared, shortly after mention of that appeared at Deep Climate, **A.11**.

All this may seem a harsh assessment. That is the reason for the inclusion of 200+ pages of backup evidence in more detail than most people will ever need.

2.10 The PR campaign

The WR was never about science, but was PR designed to mislead Congress, confuse the public, offer good quotes and be referenced again and again, to this day. The immediately-preceding events are shown here, with more detail in **A.5**, followed by a brief discussion of longer-term PR.

07/13/06 (no later) sent to WSJ, because:
 07/14/06 WSJ Editorial “Hockey Stick Hokum” [WSJ2006]
 07/14/06 10AM Barton, Whitfield announce WR [BAR2006]
 It is well worth reading this 2-pager.
 07/14/06 10AM Whitfield announces 07/19/06 10AM hearing

Just as Myron Ebell had copies of the 2005 Barton/Whitfield letters, before some recipient(s), good PR tactics make sure helpers are ready, and the other side knows nothing, learn of it from a WSJ Editorial.

07/19/06 , Rep. Joseph Barton (R-TX) introduces House hearings:
[BAR2006a, pp.7-9]

“... I have never met Dr. Wegman. We asked to find some experts to try to replicate Dr. Mann's work. ... He picked some **eminent statisticians** in his field and they studied this thing. ... and Dr. Wegman and his colleagues who as far as I know have got **no axe to grind**, have said the Mann study is flat wrong. ... So I want to thank Dr. Wegman and his colleagues for giving us an unvarnished, flat out **non-political report**. ...

We are going to put it up there, **let everybody who wants to, take a shot at it**. Now, my guess is that since Dr. Wegman came into this with **no political axe to grind, that it is going to stand up pretty well**. ...

PREPARED STATEMENT ...

I would especially like to thank Dr. Edward Wegman who, on his own time and his own expense, assembled a pro bono committee of statisticians to provide us with **independent and expert guidance** concerning the hockey stick studies and the process for vetting this work. Dr. Wegman and his committee have done a great public service. Their report, with **clear writing and measured tone**, has identified significant issues concerning the reliability of some of the climate change work that is transmitted to policymakers and characterized as well scrutinized.”

The WR has been referenced frequently since then in blogs and other non-peer-reviewed items , rather rarely in actual science journals.
 All this fuss sits atop arguments that make little difference, §2.3.

As an experiment, the reader might try:

Google: wegman report

Some are negative, but many support it strongly. Besides all the websites and blogs, one finds a steady stream of books that rely on the WR.

A quick sample of recent books includes entries from US(6), UK(2), Canada(1) and Australia(1). I own most, but I am sure many more exist.

- *[ALE2009] Ralph B. Alexander, *Global Warming False Alarm – The bad science behind the United Nations’ false assertion that man-made CO2 causes global warming.*
- *[GOR2010] Steve Goreham, *Climatism.*
- *[HAY2008] Howard C. Hayden, *A Primer on CO2 and Climate*, 2nd Ed.
- *[HOR2008] Christopher Horner, *Red Hot Lies: How Global Warming Alarmists Use Threats, Fraud, and Deception to Keep You Misinformed.*
- *[LAW2009] Nigel Lawson, *An Appeal to Reason: A Cool Look at Global Warming*, (UK).
- *[MIC2009] Patrick J. Michaels, Robert Balling, Jr , *Climate of Extremes - Global warming science they don’t want you to know*, CATO Institute (“in cooperation with the George C. Marshall Institute”) .
- *[MON2010] A. W. Montford, *The Hockey Stick Illusion*, (UK).
- *[PLI2009] Ian Plimer, *Heaven and Earth: Global Warming – The Missing Science*, Australia. This has 6 pages mostly quoting WR.
- *[RAP2008] Donald Rapp, *Assessing Climate Change: temperatures, solar radiation, and heat balance*. The 2nd edition, 2010 has same material.
- *[SOL2008] Lawrence Solomon, *The Deniers*, (Canada).

Meanwhile, the British parliament was sent (at least) 6 submissions to that cited the WR regarding “Climategate” [BRA2010, EWE2010, HOL2010, MCI2010, MEN2010, PEA2010].

See **A.12** for an August 2010 remake of the WR in a real statistics journal, loudly trumpeted across the Web, mostly by people with no obvious understanding of the science. The WSJ quoted it.

The WR lives on, a fine PR façade, but now it is time to look behind it.

3 History and helpers, behind the façade

At this point, the discussion shifts to the detailed history of the façade's creation and the helpers behind it.

3.1 Plausible strategies

Following are a few examples of strategies one might follow in objectively evaluating MBH-vs-MM, using references available in mid-2006.

Minimal science

One might simply look at the paleoclimate sections in [IPC2001, IPC2006] or study the charts in §1.4. If one understands Theme-J¹⁰, one might conclude, especially from the “spaghetti graph”:

- MBH99 has substantial error bars.
- Most points of most reconstructions generally fit.
- Reconstructions sometimes differ for good reason, such as choice of geographical coverage, Theme-F¹⁰.
- We may never know the real answer, but it is very likely in there somewhere. These people are trying to extract signal from relatively small numbers of noisy datasets.
- The Earth is (likely, very likely, who knows) warmer than it was during the MWP, but if not, it will be very soon.
- And in any case, the MWP temperature is irrelevant to the future.
- Science is progressing as usual and IPCC is reasonable.
- *There is no big problem with MBH, time to move on.*

More science

In addition, one might read:

- MBH98, MBH99, [IPC2001- paleoclimate, TS,SPM in that order]
- MM03, MM05, MM05b
- And maybe Mann, et al (2005)

One might notice the careful caveats in the papers and how graphics got simplified, from MBH98/99 to IPC main report, to TS, to SPM, some of which is a necessity. One might worry a bit about making nontechnical audiences understand the meaning of the grey uncertainty zone, but have sympathy for the general problem of science communication, conclude:¹⁰

- *There is no big problem with MBH, time to move on.*

Serious Science Review

This might be done with an expert panel [NOR2006, NRC2006], with substantial review of many documents, including those marked X or labeled Relevant (R). They in effect concluded:

- *There is no big problem with MBH, time to move on.*

Minimal statistics

One might read:

- MBH98, MBH99, [IPC2001 - paleoclimate, TS,SPM in that order]
- MM03, MM05, MM05b
- Mann, et al (2005)
- Wahl, Amman (2006), **W.8.4**

That might be enough or perhaps one would get the code from Wahl, Amman and study that, do one's own experiments, concluding that PCA-decentering was incorrect, but doing it “right” made little difference, especially given the size of the grey uncertainty zone, conclude:

- *There is no big problem with MBH, time to move on.*

3.2 WR, as presented, contradictions

The WP could have taken the Minimal Statistics approach with less effort than was actually spent to reach the two key conclusions:

- *There are big statistics problems with MBH98/99, mission #1.*
- *There are big problems with paleoclimate peer review, mission #2.*

To which one might add

- *Never move on, MBH98/99 must be discussed forever.*

As seen in the **Page tally, §2.7**, most WR pages are irrelevant.

WR, p.2

“This committee, composed of Edward J. Wegman (George Mason University), David W. Scott (Rice University), and Yasmin H. Said (The Johns Hopkins University), has reviewed the work of both articles, as well as a network of journal articles that are related either by authors or subject matter, and has come to several conclusions and recommendations.”

This statement might be misleading. Who actually selected the articles? It is plausible that Said reviewed some of the articles, and Wegman a few, although his testimony showed clear unfamiliarity with later Mann papers:

¹⁰ This is more-or-less what I did a few years ago.

[BAR2006a, p.38]:

“MR. STUPAK. Okay. Let me ask you this question. **Have you reviewed any of Mr. Mann's later refinements of his 1999 report?**

DR. WEGMAN. **I have reviewed some level of detail, not in intense level of detail, the continuing papers, the continuing papers, most of which are referenced--in fact, the ones that are referenced--**

MR. STUPAK. **Did he refine his data and his methodology?**

DR. WEGMAN. **My take on the situation is that rather than accept the criticism that was leveled, he rallied the wagons around and tried to defend this incorrect methodology.”**

[BAR2006a, p.41]:

“MS. SCHAKOWSKY. Well, let me ask you this. Dr. Mann has published dozens of study since the original hockey stick study and as I said earlier, beginning in 2003 he reformulated the statistical methods. Do you take into account these later studies in your report?

DR. WEGMAN. **I have read his later studies. I was not asked about his later studies.”**

None of this is consistent, but avoids answering an inconvenient question, Meme-h¹. If Wegman had not been asked about later studies, why are they referenced, Summarized and discussed in the Literature Review? The WP was not asked to become expert in Bristlecone pines, nitrogen fertilization or SNA, but Wegman opined on them, too.

Even ignoring the plagiarism, the poor quality of work is clear in **W.11.8**. Mann, et al (2005) was the latest-available Mann-led paper, making it important. One need only scan that quickly to know Wegman is wrong. One can read the WR Summary of that paper, see that Mann, et al had moved from PCA to RegEM and had evaluated various methods. The same is in the Literature Review, p.24. *He was clearly wrong about a simple fact of an Important Paper, how much time did he spend elsewhere?*

Who actually wrote or edited the 17 Summaries? I cannot know for sure, but Said certainly seems the likely choice, although help from Reeves, Rigsby or even MM+TT cannot be ruled out. In any case, the task of actually understanding the relevant literature seems to have been left to junior person (s) with no relevant experience, but obvious incompetence. The key PR messages of the WR mostly ignore its own Summaries, Bibliography and Literature Review. *They just look like scholarship.*

Once again, the two key missions #1 and #2 were:
WR, p.7:

“To this end, Committee staff asked for advice as to the validity of the complaints of McIntyre and McKittrick [MM] and related implications. ... **We have sought to reproduce the results of MM in order to determine whether their criticisms are valid and have merit.** We will also comment on whether issues raised by those criticisms discussed in McIntyre and McKittrick (2005a, 2005b) **raise broader questions concerning the assessment of Mann et al. (1998, 1999) in peer review and the IPCC** and whether such science assessments involving work of a statistical nature require some type of strengthening to provide reliable guidance for policy makers.”

Is it plausible that the Committee staff “asked for advice” or did they really want someone to ratify and amplify the MM+TT ideas implied above? Did they want someone to find MBH and paleoclimate both guilty? Was the attack on peer review expected from the start, Meme-b¹? People urged Wegman not to include that, A.11.2, slide 19.

Suppose they actually wanted expert, unbiased answers. An NRC panel was the right way. It might have been barely plausible to formally ask the ASA for statisticians to evaluate the MBH statistics, #1, assuming availability of some with at least minimal climate knowledge. But Wegman obviously did not seem to understand (or accept) the Greenhouse Effect and Said showed no obvious expertise. At least Wegman or Scott likely might have done the right math, but Scott was barely involved.

But consider claimed mission #2, to evaluate paleoclimate peer review and the IPCC. That requires a serious multidisciplinary group of senior people, like the NRC panel, but plausibly adding social scientists who actually study such issues. At the least, one would want a distinguished panel, as done in “Climategate” investigations run by Ron Oxburgh or Muir Russell. ***Would one pick a statistician, senior, but quite unfamiliar with the entire field, with help from a new PhD and some students? I doubt it.***

Is it plausible that Barton and Whitfield would have gone forward with this effort unless they were absolutely sure the WR would produce the “right” answers? They certainly were happy with the final report.

But behind the façade is much more.

3.3 The façade and its construction

It is nontrivial to show complex sequences of activities, some of which were intended to be hidden, involving many people and organizations unfamiliar to many readers. The following is a 2-page attempt to summarize a sequence of 20 actions, each clearly involving some people and possibly involving others. These pages show a sequence of “netflow” charts, showing organizations, people, cross-group relationships of information flows. See [MAS2010] for background.

The approach is inspired by Edward Tufte’s “Small Multiples,”¹¹ in which small diagrams are arrayed together, sharing structure in a way that helps people focus on the changes and differences.

Each of the 20 “netflows” contains one or more of the same elements, perhaps with notes or other organizations.

TT (Think Tanks) includes, but is not limited to the Competitive Enterprise Institute (CEI) and the George C. Marshall Institute (GMI), and lists a few of the most active people, who have worked together for years, sometimes 15-20. One can assume everyone in the leftmost box knows everyone else and emails are likely to be quite frequent.

MM were repeatedly brought to Washington, DC, introduced to people, presented talks, got feedback, exchanged email often, especially with Fred Singer.¹² In some cases, information was provided by MM or TT, or MM information sent through TT, but from outside that is unknown. Also, the WP did work directly with McIntyre on some issues.

Red names are known to be involved, or at least very likely. Grey names are people who might have been involved, or might have at least known what others in the same box knew. For example, Activity 06 is simple, as it shows a presentation and meeting for which some people are introduced as being present. One cannot be sure either way of some others. This example is ~2 years before formation of the WP.

¹¹ Edward R. Tufte, *Envisioning Information*, Graphics Press, 1990, pp.67-80.

¹² In social networking jargon, Singer exhibits high centrality, i.e., knows everybody, [MAS2009, MAS2010].

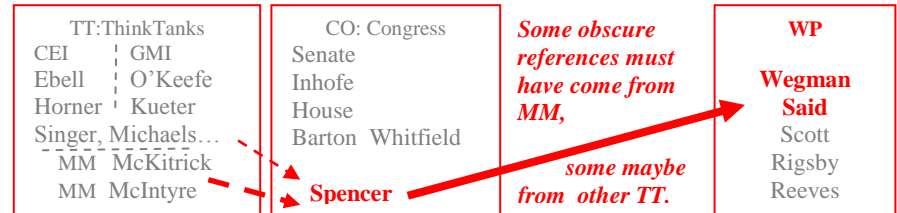
06 2003.11.18 MM talk to CEI + GMI; are GMI “experts” by 1Q04



Aloysius Hogan, counsel for Inhofe, asks questions about tree-rings statistics. Other GMI: Baliunas, Soon, Jastrow Singer thanked McIntyre for earlier sending data

In Activity 14, Spencer provided many of the references to Wegman and Said. Some seem likely to have originated from MM, especially the more obscure proxies studied by McIntyre. Some other technical ones may have come from Singer or Michaels. I cannot imagine where Valentine(1987) originated. Material went to Wegman and Said, but the rest are unclear.

14 2005-2006 Flow of references



A conventional narrative follows the 20-step list, keyed back to it. I hope the key ideas are visible in the 20-step list. Try reading the narrative for context and returning to look at the overall structure:

- TT recruited and fostered MM for years, promoting them to CO, and getting them introduced to Inhofe, who was using their ideas by 2005.
- In mid-2005, the May 11 MM05x meeting laid out a strategy, and it was shortly handed off to Barton and Whitfield. Ebell was ecstatic.
- In Wegman, they later found someone to execute this strategy.
- At that point, TT minimized their visible involvement. Having worked towards this for years, *it is unlikely they stopped helping, just that their involvement became less obvious.*
- Ironically, this is a good application of thinking about social networks, in this case, time-varying ones with changes in team composition.

Part 1 – Events before the Wegman Panel formed

01 1998 GCSCCT organized by American Petroleum Institute

TT:ThinkTanks
 CEI | GMI
 Ebell | O’Keefe*
 Salmon
 Singer**
 *O’Keefe then API
 **via his wife

“Identify, recruit and train a team of five independent scientists to participate in media outreach. These will be individuals who do not have a long history of visibility and/or participation in the climate change debate. Rather, this team will consist of new faces ...”

02 2001.10.11 McKitrick talk for Congress

TT:ThinkTanks
 CEI | GMI
 Ebell | O’Keefe
 Horner | Kueter
 Singer, Michaels...
 McKitrick

CO: Congress
 Senate
 Inhofe
 House
 Barton Whitfield
 Spencer

Ebell connects with McKitrick, brings to Washington, DC.

03 2002.07.25 House hearings on climate

TT:ThinkTanks
 CEI | GMI
 Ebell | O’Keefe
 Horner | Kueter
 O’Brien, Michaels

CO: Congress
 Senate
 Inhofe
 House Stearns
 Barton Whitfield
 Spencer

04 2003.02.27 Essex, McKitrick talk for Congress, Senate offices

TT:ThinkTanks
 CEI | GMI
 Ebell | O’Keefe
 Horner | Kueter
 Singer, Michaels...
 McKitrick
 Essex

CO: Congress
 Senate
 Inhofe
 House
 Barton Whitfield
 Spencer

Ebell promotes Essex and McKitrick, speaking about their book.

05 2003.11.xx MM meet Inhofe

TT:ThinkTanks
 CEI | GMI
 Ebell | O’Keefe
 Horner | Kueter
 Singer, Michaels...
 MM McKitrick
 MM McIntyre

CO: Congress
 Senate
 Inhofe
 House
 Barton Whitfield
 Spencer

McIntyre has connected with McKitrick, now working together, Ebell promotes them, and introduces them to cast of anti-science all-stars at GMI. (next netflow).

In these activities, red shows people known to be involved.

06 2003.11.18 MM talk to CEI + GMI; are GMI “experts” by 1Q04

TT:ThinkTanks
 CEI | GMI
 Ebell | O’Keefe
 Horner | Kueter
 Singer, Michaels...
 MM McKitrick
 MM McIntyre

CO: Congress
 Senate
 Inhofe, Hogan
 House
 Barton Whitfield
 Spencer

Aloysius Hogan, counsel for Inhofe, asks questions about tree-rings statistics. Other GMI: Baliunas, Soon, Jastrow
 Singer thanked McIntyre for earlier sending data

07 2005.02.10 Inhofe talks for GMI; WSJ Article on MM, OpEd

TT:ThinkTanks
 CEI | GMI
 Ebell | O’Keefe
 Horner | Kueter
 Singer, Michaels...
 MM McKitrick
 MM McIntyre

CO: Congress
 Senate
 Inhofe
 House
 Barton Whitfield
 Spencer

TT+CO, but who? Barton later claimed WSJ article noticed, led to letters. Really? He and Inhofe are long allies, oil \$\$\$.

WSJ
 2005.02.14
 Regalado
 Front page
 2005.02.18
 OpEd

08 2005.05.11 MM talk for GMI, MM05x KEY SOURCE

TT:ThinkTanks
 CEI | GMI
 Ebell | O’Keefe
 Horner | Kueter
 Singer, Michaels...
 MM McKitrick
 MM McIntyre

CO: Congress
 Senate
 Inhofe
 House
 Barton Whitfield
 Spencer

Unlike earlier meetings, this only names a few people, does not name question askers. Some of the questions are technical (likely Singer), but these meetings also usually have CO attendee(s).

09 2005.06.23 Barton, Whitfield letters to MBH, others

TT:ThinkTanks
 CEI | GMI
 Ebell | O’Keefe
 Horner | Kueter
 Singer, Michaels...
 MM McKitrick
 MM McIntyre

CO: Congress
 Senate
 Inhofe
 House
 Barton Whitfield
 Spencer

Mann, Bradley, Hughes Pachauri, Bement
 Ebell had copies of letters immediately, even before at least one recipient.
 TT promotion of MM goes underground, CO is carrying the ball, Ebell is elated.

10 2005.07.15 Letter from NAS offering NRC report, rejected

TT:ThinkTanks
 CEI | GMI
 Ebell | O’Keefe
 Horner | Kueter
 Singer, Michaels...
 MM McKitrick
 MM McIntyre

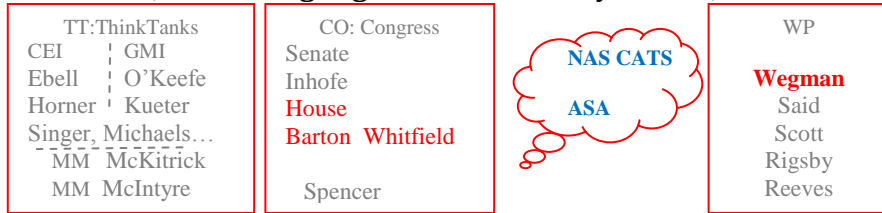
CO: Congress
 Senate
 Inhofe
 House
 Barton Whitfield
 NO
 Spencer

Rep. Boehlert chastises Barton & Whitfield, says ask NAS for advice.

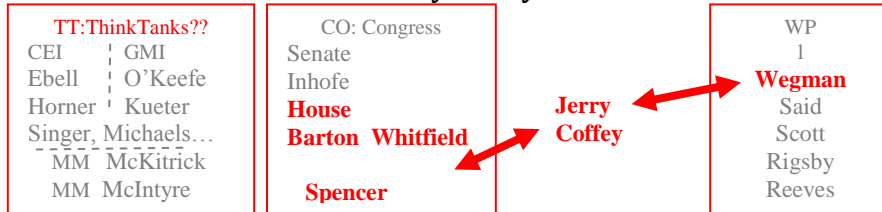
2005.07.15
 Ralph Cicerone
 President, NAS
 Rejection awkward.

Part 2 - Wegman Panel and after

11 Contact, misleading vague claims later by Barton, Whitfield



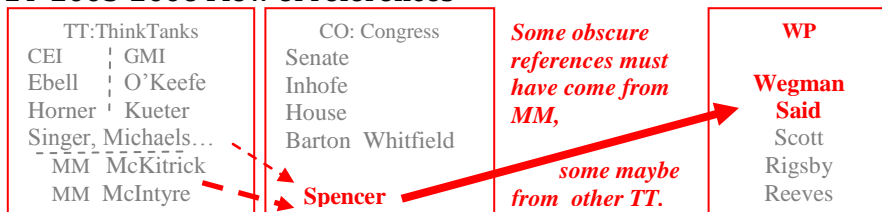
12 2005.09.01 Actual contact by Coffey



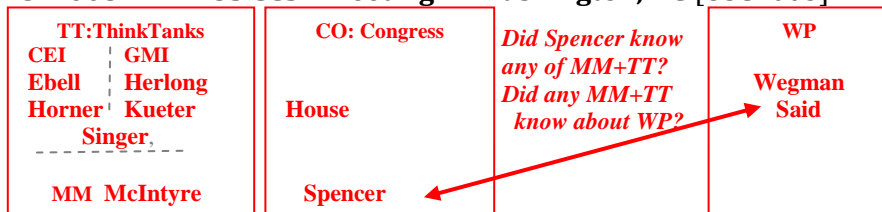
13 2005.09.xx Contact by Spencer



14 2005-2006 Flow of references

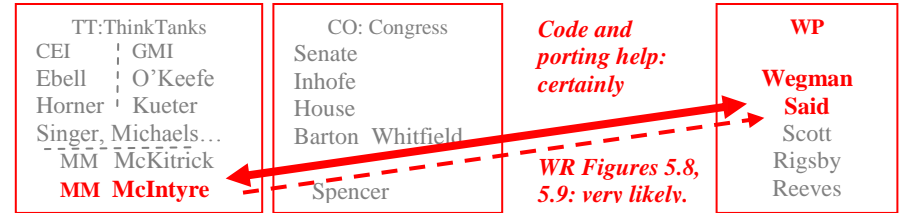


15 2005.11.14 US CCSP meeting in Washington, DC [USC2005]



In these activities, red shows people known to be involved

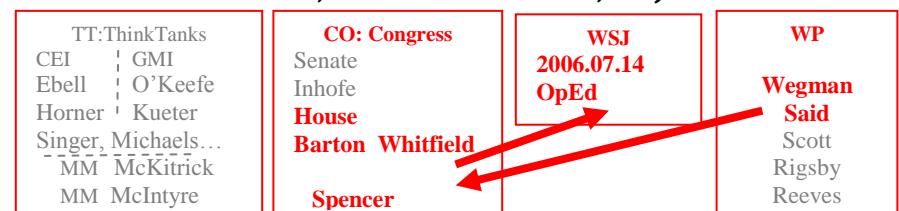
16 2005-2006 Other interactions



17 2006.03.01 NAS Panel, Washington, DC



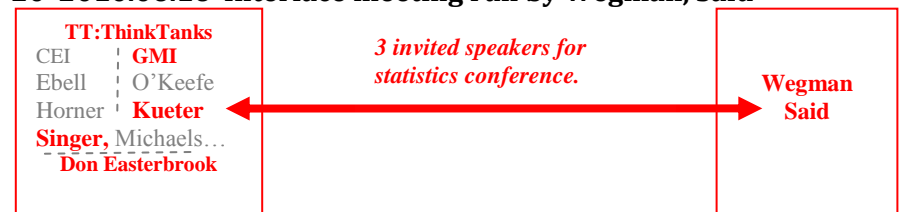
18 2006.07.14 Barton, Whitfield announce; WSJ Editorial



19 2006.07.19, 27 House hearings



20 2010.06.16 Interface meeting run by Wegman, Said



Activities 01-06

Following a 1998 strategy created with the American Petroleum Institute MM were opportunistically recruited, first by Ebell (CEI), then with CEI and GMI working together. They were brought to Washington, DC various times, coached by experts, including Singer, with whom McIntyre was already corresponding in 2003, at which time they were introduced to Inhofe.

Activity 07

By early 2005, Inhofe was already using MM+TT anti-hockey stick material, and the WSJ provided front-page publicity.

All this follows a standard pattern of using spokespeople who can seem independent (like Baliunas and Soon, or MM), but work very closely behind the scenes with TT and Congressional allies (CO), certainly including Inhofe and Barton, at least. Such spokespeople get invited to speak, although it seems to be getting more difficult, i.e., having to ask the Viscount Christopher Monckton. Barton (TX) and Inhofe (OK) ran parallel committees in House and Senate, were long-time allies and both heavily funded by fossil energy companies:

Inhofe: (Koch Industries is #1, relevant due to the Koch's others)
www.opensecrets.org/politicians/summary.php?cid=N00005582&cycle=Career
www.opensecrets.org/politicians/contrib.php?cycle=Career&cid=N00005582&type=I

Barton:
www.opensecrets.org/politicians/summary.php?cid=N00005656&cycle=Career
www.opensecrets.org/politicians/contrib.php?cycle=Career&cid=N00005656&type=I

It would be astonishing if they had not communicated regularly on climate issues, and if their staffs had not cooperated closely for years.

From years of cooperative efforts and emails shown in [MAS02010, A.9], *it is hard to imagine MM ideas and plans not spreading quickly through*

TT. If something useful is learned by any of the key group in the Washington, DC area, they would likely all hear about it quickly:

- (CEI) Ebell, Horner
- (GMI) Kueter, O'Keefe, perhaps Herlong
- (SEPP) Singer

From some emails, even trivial matters seemed to propagate quickly.
 Email is wonderful, unless someone comes looking with subpoena power.

Activity 08

MM presented MM05x, as key talk for GMI, 05/11/05, followed by discussion, with chronology starting in **A.5** (Step 08). Unlike some earlier meetings, questioners are not named, but these meetings seem to be used:

- to evaluate the newest material,
- to make suggestions, and
- to sometimes include a few outsiders to learn to anticipate questions likely to be asked in less friendly environments.

Of course, this is the publicly recorded material, not the side conversations and meetings. Given records of past GMI meetings, I would be surprised if there had been no CO staffers in attendance.

The WR vaguely references this as dated 09/06/05, but never cites it or identifies it as a GMI-hosted document. *That is probably not accidental.* The date hints that it is an early document provided by Spencer. See **W.8.9** for a longer discussion of its ideas, including the red-marked Memes **1**.

Much of this talk would never survive credible peer review, but it represents a good set of talking points, refined over years by MM+TT, and it sounds like science.

Activity 09 – Barton, Whitfield letters

It seems *plausible* that these letters were triggered by the Phil Cooney scandal and impending energy bill. TT+CO *likely* needed some good PR, and MM05x was fresh in people's minds, with Ebell especially helpful. The letters were written.

[MAS02010, A.9.6] showed that Ebell had copies of the PDFs of the Barton/Whitfield letters less than 2 hours after they were created late Friday afternoon 06/24/05, before recipients could possibly have responded

or in at least one case, even gotten it. Yet, Ebell was already sending copies to the White House (Perhach). It is not surprising he was so excited, MM+TT had been working for this kind of effort since 2002.

Activity 10

Rep. Boehlert (R-NY)¹³ pushed back against this odd, intimidating procedure, as did the science community. NAS offered a standard (expert, unbiased, independent) NRC panel to look at the problem, 07/15/05.

The NRC panel offer was rejected, but this left TT+CO the awkward problem of leaving its strategy rebuffed. What might they want?

- Legitimize and amplify the statistics-based MM+TT narrative, the direct attack on the hockey stick, §1.5.
- Ratify MM papers and MM+TT views in every way possible.
- Find some way to discredit the IPCC and climate science as whole, §1.6, starting with complaints against peer review and study independence.
- When possible, promote other common climate anti-science Memes.
- And under no circumstances admit to reality of AGW.

How might they get all that?

- Find a senior statistician who might be willing to do this and able to recruit at least a semblance of a team. Statisticians actually involved with climate science would be unlikely to help, especially since one must ignore most of the Themes listed here.
- But avoid any with a history of outspoken climate anti-science views, as they would lack credibility, i.e., the “fresh voices” approach.
- Do not ask anyone who might say “no” and talk about it.
- Find someone sympathetic or at least persuadable and sound them out personally, not through a normal NRC-like selection process.
- Announce nothing until it is clear that the “right” answers will emerge.
- Use MM+TT to provide as much help as possible through Spencer or sometimes even directly.

Having rejected a NRC panel, would Barton and Whitfield take the slightest chance on a panel that might produce the “wrong” answers?

¹³ Often mentioned by scientists as a strong supporter of science, [NOR2006].

That seems very unlikely. In any case, they were clearly ecstatic with the report eventually produced, even to the happy pictures at [SAI2007, p.27].

Activities 11, 12

Now, I can only speculate, since the next visible event was the 09/01/05 Coffey/Wegman connection, which took 6 weeks. I would guess that:

- *TT+CO tried very hard to come up with names of candidates or at least people who could suggest some.*
- *Although Wegman had some old history with Star Wars (hence possible GMI connection), one might have expected a faster recruitment if he was still well-known to GMI or CEI.*
- *Someone in TT+CO knew Coffey, who suggested his friend Wegman. Alternatively, someone thought of Wegman and knew Coffey a good indirect route. Coffey’s climate anti-science views are demonstrably intense. It is hard to believe he would recruit anyone who would even admit AGW might be possible. In any case, Wegman was asked.*
- *But later, vague words about ASA, NAS and NRC were used to try to add credibility. Coffey was unmentioned except via [SA2007].*

Activities 13, 14

Wegman agreed to do it, recruited Said quickly. They met with Spencer, who started sending documents. [SAI2007, p.9, shown in A.11.2] says: “Reviewed some 127 technical papers related to paleoclimate reconstruction.”

Whether the WP was connected with TT at that point is unclear, but given the way CO has long worked closely with TT, *I would guess* that MM+TT at least knew about Wegman within a week. The selection of papers is covered with MM+TT fingerprints. Singer, Ebell Horner have broad knowledge of anti-science sources, possibly accounting for many of the grey references. However, some almost certainly came from MM.

Activity 15

The US CCSP November 14-16 included an interesting combination of people. It is unknown who met, but McIntyre and the 5 from TT all knew each other. Spencer, Wegman and Said knew each other. Wegman and Said knew of McIntyre and by previous reasoning MM+TT very likely knew of Wegman. *I would be surprised if there were not substantial contact during the 3-day meeting. Did McIntyre pay for his trip?*

Activities 16

The WP worked directly with McIntyre on code, and it seems very likely that he provided the work for WR Figures 5.8 and 5.9, and maybe even the figures themselves, **W.5.8, W.5.9**.

Activity 17

The NAS panel offered another chance for some of the people to meet. Attendee list is not generally known.

Activities 18,19

Barton and Whitfield make their announcement, working with WSJ. The hearings are held.

Step 20 (a 2010 event)

Wegman and Said invite Singer, Kueter, and Don Easterbrook to speak.

At some point, the WP must have gotten the idea of doing SNA to pursue mission #2, Meme-b. Rigsby may have been recruited then, as he at least had familiarity with some of the tools. His analysis is straightforward, the problem is in the interpretation and words around it.

It is very likely that Said read and summarized the paleoclimate papers. It is unlikely Wegman spent much time on that, except for the MM papers.

When I first started, I was puzzled by the poor quality of Summaries, and how often they were ignored. I was puzzled by the mass of irrelevant references. Most people do research and then reach conclusions. I started by looking at the Summaries and Bibliography, then following to WR original work and conclusions. But with every additional page considered, the WR departed further and further from a credible assessment effort.

This originally was going to be a quick 30-page examination of the Summaries and Bibliography. But each new issue unearthed more threads to follow elsewhere, including testimony contradictions and the various related activities.

3.4 Strategy behind the façade, evidence

From all this analysis, the evidence is consistent for a strategy by Wegman, implemented by him and Said, with some help from others who may have not really known the real missions #1 and #2, not the claimed ones.¹⁴

- Do everything possible to promulgate MM+TT+CO views.
 - Is there anything at all in the WR that Barton or Inhofe would dislike?*
- Start with the uncited McK05 and especially MM05x as guides. Read MM03, MM05a, MM05b carefully and agree with everything.
- Work closely with MM, especially with McIntyre, not just for code. *In particular, McIntyre seems very likely the direct source for several pages, WR §5.8. WR §5.9, for the reasons described, W.5.8, W.5.9.*
- Write Summaries and Bibliography to provide an illusion of scholarship. That is done by junior person(s), perhaps edited by Wegman, perhaps not, given the pervasive issues.
- Attack MBH98/99 on narrow statistical grounds, essentially ignoring later MBH studies and others, mission #1.
- Avoid Wahl, Amman (2006), ignore other critiques of MM05b, **W.8.4**.
- Try to discredit not just MBH, but the rest of paleoclimatology, mission #2, following ideas of Michaels in 2003→MM (McK05, MM05x). Use SNA, known slightly to the team, but apply the tools and terminology to draw impressive graphs and make baseless claims of poor peer review in paleoclimate, followed up with [SAI2008]. Make that rub off on IPCC.
- Never admit that recent GW is AGW.
- Do not answer basic science questions, Meme-h, but still speak confidently about nitrates, bristlecone pine, obscure proxy issues and other “confounding factors.”
- Do everything possible to create doubt and confusion for a general audience, and provide quotes for TT+CO.
- Follow up for several years, usually with talks to non-expert audiences.
- In 2010, Singer and Kueter get to speak at Interface 2010. Perhaps Wegman and Said knew them well by then.

¹⁴ For instance, [SAI2007, p.5] mentioned a 4th person who dropped out, for which many reasons are plausible. An intriguing possibility is that it was described to them as an objective, unbiased assessment, and after a while, he or she realized that it was not. I cannot know, but the question should be asked.

4 Issues, legal and otherwise

Following is a list of potential issues, showing which entities might want to look at them, with my best guesses as to relevant people. Issues vary in severity and some are just questions. In some cases (like plagiarism), the result is clear but exact responsibility is not always clear, in which case multiple people are listed to show who might at least know.

I think these questions need asking, although it is not my role to judge the results, and some questions would likely only ever get answered by Congressional or DoJ investigations.

A.10 describes possible (serious) legal issues. Misleading Congress can be a felony, as is conspiracy to do so, as is misprision of felony (knowing about it, not telling). Should investigations ever get that far, see [MAS2010] for the much larger network in which some people mentioned here participate. Put another way, from looking at just a few of the emails mentioned there, one might guess that quite a few more thinktank members, their allies, Congressional staffers, key media allies and lobbyists were quite aware of the whole Wegman project. This is likely true just on the visible tendency of some people to send or forward email, especially when excited. See [MAS2010, A.9] for examples. I suspect some of this email would make far more interesting reading than climate scientists arguing about tree rings.

Attempted destruction of evidence is not a good idea either.

On the second following page are listed various unresolved questions of lesser import. TT+CO includes other Congressional staffers (like **Mark Paoletta**, who appears in [SAI2007, p.26], Representatives (beyond Cliff Stearns) or Senators, especially Inhofe, i.e., people who potentially knew what was going on and were perhaps helping. Again, I cannot know, but a serious investigation by Congress or DoJ might find more,

The first page shows some serious problems up to and including possibilities of multiple felonies for some people. Item 22 might be appropriate, but I would be surprised were it to occur, given that Virginia Attorney General **Ken Cuccinelli** and his assistant **Wesley Russell** are both GMU graduates. Like Barton, Inhofe, key thinktanks, and GMU itself, Cuccinelli has received substantial funding from fossil fuel interests, often including Koch Industries or the Koch foundations. This possibly has some connection with the recent attacks on Mann and the University of Virginia.

The second enumerates miscellaneous lingering questions. Many of these may be irrelevant or loose ends, but seem worth recording. I have been surprised before.

4(a) Problems Entity to Ask	Barton	Whitfield	Stearns	Spencer	MM	TT+CO	Coffey	Wegman	Said	Scott	Rigsby	Reeves	Sharabati	Rezazad	Rapp	McShane	Wyner	Potential Problems, *d are marginal, shown as reminders.	Refer	Who
1 U Southern California?															X			Rapp(2008) <-- WR 2.1 <-- Bradley (1999), plagiarism	W.2.1	
2 Springer															X			Rapp(2008) <-- WR 2.1 <-- Bradley (1999), copyright	W.2.1	Elsevier
3 U Penn/Wharton															X			[MCS2010] <-- WR (plagiarism), various (fabrication)	A.12	
4 Northwestern U															X	X		[MCS2010] <-- WR (plagiarism), various (fabrication)	A.12	
5 Johns Hopkins?									X*									X* work was done while Said at JHU. Any Issues?	WR	
6 GMU								X	X*									WR 2.1 <-- Bradley (1999) plagiarism	W.2.1	
7								X	X									WR 2.1 <-- Bradley (1999) copyright	W.2.1	Elsevier
8 GMU								X	X*									*WR 2.2 <-- Jolliffe (2002), Rangajaran, Ding (2003)	W.2.2	Springer
9 GMU								X	X*									*WR 2.2, WR2.3 <-- Wikipedia (marginal plagiarism)	W2.2, W2.3	
10 GMU								X	X*	?	?							WR 2.3 <-- Wasserman, Faust (1994), plagiarism	W.2.3	
11								X	X	?	?							WR 2.3 <-- Wasserman, Faust (1994), copyright	W.2.3	Cambridge
12 GMU								X	X	?	?							WR 2.3 <-- de Nooy, Mrvar, Bateglj (2005), plagiarism	W.2.3	
13										?	?							WR 2.3 <-- de Nooy, Mrvar, Bateglj(2005),copyright	W.2.3	Cambridge
14 GMU													X					[SHA2008] <-- WR.2.3 <-- sources above, plagiarism	W.5.6-7	
15 NIH, ORI								X										[SAI2008] plagiarism, gov't contract cited	W.5.6, W.2.3	
16 Purdue?													X					Is [SHA2008] an issue for Purdue?	W.5.7	
17 GMU														X				[REZ2009] <-- WR.2.3 <-- sources above, plagiarism	W.5.6.10	
18 GMU								X	X	X	X	X	X	X				(general) Review PhD Supervision practices?	A.9,W.5.7,W.5.10	
19 GMU								X										[SAI2005] <-- Shakashiri web page, plagiarism	A.9	
20 GMU																		Get outside SNA experts to review "SNA" research?	A.5.1-2	
21 ASA								X	X	?								Ethical Guidelines? Any comments?	A.8	Statisticians
22 State of VA AG								X	X		X	X						GMU is in VA. Were any VA state funds used?	WR	VA Public
23 NSWC											X							[SAI2008] Were Federal funds used?	W.5.6, W.2.3	US Public
24 MITRE												X						Were Federal funds used?	WR, A.7	US Public
25 NIH, NOIAA, ORI									X									[SAI 2008] 1 F32 AA015876-01A1, funding use?	A.7	US Public
26 ARO (Army Res. Org.)								X										[SAI2008] W911NF-04-1-0447, any issues?	A.7	US Public
27 ARL (Army Res. Lab.)								X	X									[SAI2008] W911NF-07-1-0059, funding use?	A.7	US Public
28 Congress	X	X		X														WP pro bono, but this fuss not free. How much \$\$?	A.7	US Public
29 Congress	X	X	?	X	X	?	X	X	X	?	?	?						18.U.S.C §1001, 18.U.S.C §4 OR 18.U.S.C §371	A.12	Congress,DoJ
30 GMU								?	?									18.U.S.C §1512, etc. Which person(s) deleted files?	A.11, A.12	Congress, DoJ

4(b) Questions	Barton	Whitfield	Spencer	CO other	MM	TT+CO	Coffey	Wegman	Said	Scott	Rigsby	Reeves	Sharabati	4th Unk	Various Questions Chronology in A.5 may be useful reference	Sections
1	X	X	X	?		?	?								How was Wegman recommended?	
2	X	X	X	?		?									How was Coffey selected to contact Wegman?	
3							X	X							What was the real charter from Coffey 09/01/05?	
4								X	X		X	X			When did others get recruited? What were they told?	
5														?	Who was the 4th person, when did he or she drop out? Why?	[SAI2007]
6			X					X	X	?	?	?	?	?	Who attended kickoff meeting with Spencer?	
7								X		?					When did Scott actually get asked for Appendix A?	
8			X		X	?	?	X	X	X	?	?			For each reference, who really suggested it?	
9			X		X	X		X	X						US Climate Change Workshop, who did WP talk to?	A.5
10					X			X	X						WP worked directly with McIntyre. How about McKitrick?	
11			X												When did Spencer know about Wahl, Amman(2006)?	
12								X	X						When did WP know about it?	
13								X	X		?	?			Who actually did the plagiarism on each section?	W.2, W.11
14					?	?		X	X						Who edited the Biases in?	W.2.3, W.11
15					?	?	?	X	X		X	X			Who wrote each part (besides Scott's App. A)? Any from outside?	
16					?	?	?	X	X						Did anyone other than the "reviewers" review&comment?	A.1
17								X	X				X		Did Sharabati contribute to WR itself, not just [SHA2006]?	
18									X						Wiley Computational Statistics shows SAID @ Oklahoma St U?	A.6.5
19															St. Louis Fed Reserve: why does Anderson do anti-science?	W.8.8
20															J. Economic Methodology: climate peer review? (Anderson)	W.8.8
21															Drexel U: why does McCullough write climate anti-science?	W.8.8
22								X	X						GMU: Why did [SAI2007, p.23] call GMU meeting a "Bad One"?	[SAI2007]

5 Conclusions

Abysmal Scholarship by any standard

Without even considering the statistics or science issues, the Summaries of Important Papers seem like abysmal scholarship. Half the text is simple cut-and-paste and another quarter is trivial rewording, offering little evidence of understanding. Papers with Mann as lead author are treated especially poorly, with the highest rates of cut-and-paste. The WR introduces common climate anti-science Memes into summaries of peer-reviewed papers. The summaries include many outright errors, obvious when examining side-by-side comparisons. DC found 10 pages of plagiarism. This adds 25 more. ***It is not even clever plagiarism.***

The Bibliography is the strangest I have ever seen in something claimed to be serious. Irrelevant papers are treated as Important, while some key papers are ignored completely. Some publications are totally miscategorized. Half the references are never cited, and many others are cited only weakly. It seems very unlikely that most of these references were read, much less studied seriously.

More than a quarter of the references are “grey” to some extent or other, some are beyond grey in using sources well-known to use opinion pieces to attack climate science. One reference goes so far as to list a fringe technology publication by a writer of pseudoscience.

More than a quarter of the references seemed to show bias in their selection. It seemed that the WR took many opportunities to promote MM and denigrate MBH, the IPCC, and climate science. Much of the WR seems to arise from McK05, MM05x, Climate Audit and possibly direct interactions with MM. All this supports the real missions #1 and #2.

Some references are nothing but Meme-carriers. The pervasiveness of standard Memes hints at the WP receiving help from experienced climate anti-science people, MM+TT.

Theme-G¹ was ignored numerous times, as the WP keeps trying to support a warm, synchronous MWP, no matter how many papers they cite or even Summarize saying otherwise. The WR features a distorted version of a graph whose source was not what they claimed.

The evidence of scholarly incompetence and Bias is pervasive. The WR sourced many of its references through Barton staffer Peter Spencer, and some of those seem almost certain to have been provided by MM+TT. Some references are so strange that no one could have read them. This was repeatedly presented to Congress as expert, objective, and independent. *The evidence presented here shows that it was none of those things.* The relentless pervasiveness of problems shows that this was not accidental and almost certainly began very early. Its “review” process was a façade as well.

It was in no way objective, and testimony was often contradictory, evasive or even misleading. It was not independent of MM+TT+CO.

It certainly mis-used some people, and may have mis-used even more. People were retroactively credited with much more involvement than was appropriate or were surprised to be named at all.

The WR does not even provide serious, peer-reviewable statistical analysis of MBH, despite multiple discussions of PCA mathematics.

It is a science-seeming façade for well-honed climate anti-science efforts of MM+TT+CO, simply another step in a long PR campaign. But it is still popular among some, even to this day.

I think this was a well-organized effort, involving many people, to mislead the American public and Congress. The former happens often, but the latter can be a felony, as is conspiracy to do it, and not telling about it.

Many know Darrell Huff’s famous book “How to Lie with Statistics.” We are properly wary of manipulated statistics. We generally trust good statisticians to use their methods and ethical guidelines to help us find better approximations of truth, not increase confusion.

The WR misleads by avoidance of good scholarship, good science and even good statistics.

Fortunately, I think most statisticians do not lie like this.

Acknowledgements

Deep Climate inspired all this by finding plagiarism and earlier issues in the WR, long after I'd stopped paying any attention to it. DC offered many useful bits of information and tips, including suggesting the cyan/yellow color scheme and many useful conversations over months. DC reviewed earlier versions and offered to host this and related files.

While I am entirely responsible for the words herein and no one mentioned here endorses any of this, the report simply would not have happened without DC and input from other people, whether named or not.

William Connolley provided some useful comments and the [IPC1990, Figure 7.1c] used in §1.4. John Cook kindly created the fixnum version of his Skeptical Arguments list for me. "Eli X. Rabett" did several very thorough early reviews, with many good suggestions, and found the useful www.dtic.mil/dtic. Another did an exhaustive later review that helped me clean up many low-level details. Among those making a late round of comments were Andy Skuce, Arthur P. Smith. I also thank others for comments at various stages, particularly those who argued strongly for an Executive Summary more accessible to non-experts.

I thank my wife for putting up with this for 6 months. I thank my large-screen, 8GB laptop with 3 extra displays, without which a tedious task would have taken even longer.

In light of the WR's mis-use of Social Network Analysis, it is perhaps ironic that much of this report depends on (informal) SNA, as have my 3 previous reports.

Bibliography

Most citations/references are given in-line, usually via URLs, for on-line convenience, especially if only mentioned once. The following are cited more often, offer relevant background or were used for consistency with [MAS2010]. Some are *examples of anti-science* (*), a few are about science or its history (+), some describe anti-science activities (#), unmarked ones are mixtures or other. Without categorization, some readers might be misled into assigning credibility to those marked (*). Those familiar with the topic might cringe to encounter these references, as they are usually tip-offs to anti-science writing. Since this report must reference some very "grey" sources, readers should at least be warned of my opinions, whether they agree or not.

*[ALE2009] Ralph B. Alexander, *Global Warming False Alarm – The bad science behind the United Nations' false assertion that man-made CO2 causes global warming*, 2009, Canterbury Publishing. Much of Chapter 3 (of 8) (pp.19-45) is devoted to hockey stick, claims of corruption at IPCC, etc.

*[BAR2006] Joe Barton, "Report Raises New Questions About Climate Change Assessments," 07/14/06, House Energy and Commerce Committee. This was the announcement. republicans.energycommerce.house.gov/108/home/07142006_Wegman_fact_sheet.pdf

An announcement went out on Newswire, about 10AM:
www.encyclopedia.com/doc/1P3-1077535171.html

[BAR2006a] Joe Barton et al, "Complete Transcript, of Wegman Report," 07/19/06, 07/27/06.

deepclimate.files.wordpress.com/2010/04/hockey-stick-hearings-2006-ec-committee.pdf DC provides a PDF version of the original:
frwebgate.access.gpo.gov/cgi-bin/getdoc.cgi?dbname=109_house_hearings&docid=f:31362.wais

Any page numbers here reference the PDF page numbers, since the original lacks them. In prepared testimony or reports, people can take more care than with Q&A, but can also write around awkward issues. Q&A sometimes does evoke contradictions or evasions, Theme-N₄.

#[**BRA2007**] Allan M. Brandt, *The Cigarette Century- The Rise, Fall, and Deadly Persistence of the Product that Defined America*, 2007.

It is very difficult to understand the history of anti-science in the USA without understanding cigarette wars that trained people and think tanks in the methods. See Index for “more doctors smoke Camels” and “Controversy constructed by tobacco industry.”

*[**BRA2010**] Philip Bratby, Memorandum to Parliament (CRU 17), www.publications.parliament.uk/pa/cm200910/cmselect/cmsctech/memo/limitedata/contents.htm

*[**COF2009**] Jerry Coffey, comments on blog 10/24/09

Coffey was the link to Wegman, as per[SAI2007, p.3]::

“Dr. Edward Wegman was approached by Dr. Jerry Coffey on 1 September 2005 concerning possible testimony in Congress about a statistical issue associated with paleoclimate reconstruction.

– This approach was based on independent recommendations from Dr. Fritz Scheuren, ASA 100th President and from the National Academy of Science where Dr. Wegman chaired CATS.”

This is not how NAS does official NRC panels, [NOR2006] , so the NAS reference above seems strange. Likewise, the reference to Scheuren seems strange. If Congress wanted to work through Scheuren, it could have asked him to talk to Wegman, but instead, the approach was indirect. I do not know whether Congress selected Wegman, and then asked Coffey to approach him, or whether they asked Coffey to suggest someone.

GMU hosts the website of the Washington Statistical Society, in which both Coffey and Wegman have been involved, as well as Fritz Scheuren: scs.gmu.edu/~wss/02book.pdf
science.gmu.edu/~wss/index.html

Coffey has clear views on politics and AGW:

www.rpvnetwork.org/profile/DrJerryLCoffey
www.personalliberty.com/news/study-suggests-gun-possession-may-not-protect-against-assault-19409715/#comment-40161 :

“I guess the best evidence of that is the Gore global warming boondoggle (in the early 1980s I was the reviewer for the US climate change program).”

www.personalliberty.com/news/study-suggests-gun-possession-may-not-protect-against-assault-19409715/#comment-40314 :

“My favorite short read on global warming is Lawrence **Solomon**’s “The Deniers.” I particularly enjoyed the chapter on Ed Wegman since I had a ringside seat when Ed’s analysis got started. Others books you might enjoy are the last couple by Patrick Michaels; Fred Singer and Dennis **Avery** on the 1500 year cycle; and Spencer’s latest. ... But there may still be hope. My money (if I had any) would be on the latest iteration of the Svensmark Galactic Cosmic Ray theory and the CLOUD experiment at CERN.”

Covers of several of these books appear in [WEG2007, p.4], although the slides themselves have an odd history, **A.4**.

Given a long association, perhaps they had discussed the AGW topic.

+ [**BRA1999**] Raymond S. Bradley, *Paleoclimatology – Reconstructing Climates of the Quaternary, Second Edition*, 1999. This is a famous, widely used book, cited in the WR as Bradley (1999).

#[**DEE2009**] Deep Climate, “Contrarian scholarship: Revisiting the Wegman Report,” 12/17/09.

deepclimate.org/2009/12/17/wegman-report-revisited/

#[**DEE2009a**] Deep Climate, “Wegman (and Rapp) on tree rings: A divergence problem, part 1,” 12/22/09.

deepclimate.org/2009/12/22/wegman-and-rapp-on-tree-rings-a-divergence-problem-part-1

#[**DEE2009b**] Deep Climate, “A Comparison of (WR) 2.1 p.13-4 and (Bradley) section 10.2,” 12/22/09. Newer file uses highlighting:

deepclimate.files.wordpress.com/2010/07/wegman-bradley-tree-rings-v2-1.pdf

deepclimate.files.wordpress.com/2009/12/wegman-bradley-tree-rings.pdf

#[**DEE2010**] Deep Climate, “Wegman (and Rapp) on proxies: A divergence problem part 2,” 01/06/10.

deepclimate.org/2010/01/06/wegman-and-rapp-on-proxies-a-divergence-problem-part-2

#[**DEE2010a**] Deep Climate, “A comparison of (WR) section 2.1, p14-5 and (Bradley) 5.1,5.2,6.8” 01/06/10. Files v2 and v31 use highlighting:

deepclimate.files.wordpress.com/2010/09/wegman-bradley-ice-cores-corals-v31.pdf

deepclimate.files.wordpress.com/2010/07/wegman-bradley-ice-cores-corals-v2.pdf

deepclimate.files.wordpress.com/2010/01/wegman-bradley-ice-cores-corals1.pdf

#[DEE2010b] Deep Climate, “Donald Rapp: More divergence problems,” 01/07/10.

deepclimate.org/2010/01/07/donald-rapp-more-divergence-problems

DC analyzes examples of the use of “grey literature, and does make one wonder about Springer-Praxis.

#[DEE2010c] Deep Climate, “Willie Soon and Sallie Baliunas ... (as quoted by Donald Rapp),” 01/07/10.

deepclimate.files.wordpress.com/2010/01/rapp-soon-proxies-quotes.pdf

This has more useful analysis of the use of “grey literature.”

#[DEE2010d] Deep Climate, “Steve McIntyre and Ross McKittrick, part 1: In the beginning,” 02/04/10.

deepclimate.org/2010/02/04/steve-mcintyre-and-ross-mckittrick-part-1-in-the-beginning

#[DEE2010e] Deep Climate, “Steve McIntyre and Ross McKittrick, part 2: The full story behind the Barton-Whitfield investigation and the Wegman Panel,” 02/08/10.

deepclimate.org/2010/02/08/steve-mcintyre-and-ross-mckittrick-part-2-barton-wegman

#[DEE2010f] Deep Climate, “Wegman and Said on social networks: More dubious scholarship,” 04/22/10.

deepclimate.files.wordpress.com/2010/09/said-et-al-social-networks-2.pdf

deepclimate.org/2010/04/22/wegman-and-saids-social-network-sources-more-dubious-scholarship

#[DEE2010g] Deep Climate, “A comparison of Ad Hoc Committee Report and Unattributed Sources,” 04/15/10. File v2 uses highlighting:

deepclimate.files.wordpress.com/2010/07/wegman-social-networks-v-2.pdf

deepclimate.files.wordpress.com/2010/04/wegman-social-networks1.pdf

#[DEE2010h] Deep Climate, “A comparison of Said, Wegman, et al and Unattributed Sources,” 04/15/10, 09/08/10: the newer version has color and includes the 3-way comparison with [SHA2008].

deepclimate.files.wordpress.com/2010/09/said-et-al-social-networks-2.pdf

deepclimate.files.wordpress.com/2010/04/said-et-al-social-networks1.pdf

#[DEE2010j] Deep Climate, “Wegman Report update, part 1: More dubious scholarship in full colour,” 07/29/10.

deepclimate.org/2010/07/29/wegman-report-update-part-1-more-dubious-scholarship-in-full-colour

This updates and summarizes the earlier plagiarism discussions, adds a color scheme similar to the one here, but with slightly different algorithms. Additional plagiarism is found in WR 2.2, pp.15-17, whose side-by-side is the next reference.

#[DEE2010k] Deep Climate, “A comparison of Ad Hoc Committee Report ... and Various unattributed sources on noise models (mostly various Wikipedia articles)” 07/14/10.

deepclimate.files.wordpress.com/2010/07/wegman-principal-components-and-noise-models.pdf

A few paragraphs seem to have come from Joliffe (2002), and Rangajaran & Ding (ed) (2003).

#[DEE2010m] Deep Climate, “What have Wegman and Said done ... lately?” 08/03/10

deepclimate.org/2010/08/03/what-have-wegman-and-said-done-lately

This describes the 2 bizarre sessions at Interface 2010.

#[DEE2010n] Deep Climate, “McShane and Wyner, 2010,” 08/19/10.

deepclimate.org/2010/08/19/mcshane-and-wyner-2010

#[DEE2010p] Deep Climate, “Wegman report update, part 2: GMU dissertation review,” 09/15/10.

deepclimate.org/2010/09/15/wegman-report-update-part-2-gmu-dissertation-review

#[DEE2010q] Deep Climate, “An Analysis and review of sections 1 and 2 of (McShane and Wyner)”

deepclimate.files.wordpress.com/2010/09/mcshane-wyner-1-and-2-analysis.pdf

*[DEF2002] Chris de Freitas, “Are observed changes in the concentration of carbon dioxide in the atmosphere really dangerous?” *Bulletin of Canadian Petroleum Geology* Vol 50, No 2 (June 2002), P.297-327. Received 03/28/02, Accepted 06/23/02.

web.archive.org/web/20030526163750/www.cspg.org/deFreitas_climate.pdf

The hockey stick is attacked pp.11-14.

[DEN2005] Wouter de Nooy, Andrej Mrvar, Vladimir Batagelj, *Exploratory Social Network Analysis with Pajek*, 2005. The WR used text from this, unacknowledged, as did [SAI2008, SHA2008].

www.sna.pl/teksty/ESNAwP.pdf on-line copy, no need for OCR

*[ESS2002] Christopher Essex, Ross McKittrick, *Taken by Storm – The troubled science, policy, and politics of global warming*. November 2002. Neither author is a climate scientist. The book offers plenty of confusion, and many references to science literature later strongly refuted, such as Christy&Spencer on satellite MSU anomalies, D’Aleo, Zbiginew Jaworowski, S.Idso, John Daly, GES, etc.

Chapter 5 of 10 (p.155-174) is “T-Rex plays hockey.” – they were attacking that in 2001-2002, well before McIntyre’s involvement. Donner Canadian (closely connected with the Fraser Institute) apparently gave \$20K to U of Guelph Economics 06/15/02 (presumably McKittrick), and then after the book was published, awarded the pair another \$20K as runner-up Donner Book Prize. The book thanks Donner for their support.

*[EWE2010] Susan Ewens, Memorandum to Parliament (CRU 13) www.publications.parliament.uk/pa/cm200910/cmselect/cmsctech/memo/limitedata/uc1302.htm

*[GMI2003] George Marshall Institute, Washington Roundtable, MM, “The IPCC, the “Hockey Stick” Curve, and the Illusion of Experience.,” 11/18/03. *Key document*. Ebell introduced MM to GMI, interesting people

attend, such as Inhofe counsel Hogan asking about tree-ring statistics, not usually a topic of interest to Congressional lawyers.

www.marshall.org/pdf/materials/188.pdf

*[GMI2005] George Marshall Institute, Marshall News, describing 02/10/05 Roundtable, “U.S. Climate Policy After Kyoto’s Ratification,” p.1,10,11. See Inhofe, and GMI2005 in **W.4**.

www.marshall.org/pdf/materials/300.pdf

*[GMI2005a or MM05x] George Marshall Institute, Washington Roundtable, MM, “The Hockey Stick Debate: Lessons in Disclosure and Due Diligence.,” 05/11/05. Also McIntyre, McKittrick(2005).

Key document: in essence lays out a campaign of which some was done.

www.marshall.org/pdf/materials/316.pdf

web.archive.org/web/20060213060236/www.marshall.org/pdf/materials/316.pdf

[GMU2007] GMU Statistics Colloquium Series, Fall 2007.

www.galaxy.gmu.edu/stats/colloquia/ColloquiaFall2007.html

This is an invaluable reference, that has since been removed, see **A.11**.

*[GOR2010] Steve Goreham, *Climatism!* 2010.

pp.180-181 “Dr. Edward Wegman, an expert in statistics, was chosen by the national Academy of Sciences to lead a team to provide an independent critique of Dr. Mann’s work for the House committee.”

No, he was not. This is a persistent idea.

#[GUT2009] Donald Gutstein, *Not a Conspiracy Theory – How Business Propaganda Hijacks Democracy*, 2009. (Canada)

This gives a useful Canadian viewpoint. See Chapter 7 on Fraser Institute and the National Post.

*[HAY2008] Howard C. Hayden, *A Primer on CO2 and Climate, Second Edition*, 2008, Vales Lakes Publishing. (self-published).

*[HOL2010] David Holland, Memorandum to Parliament (CRU 24)

www.publications.parliament.uk/pa/cm200910/cmselect/cmsctech/memo/limitedata/uc2402.htm

*[**HOR2008**] Christopher Horner, *Red Hot Lies: How Global Warming Alarmists Use Threats, Fraud, and Deception to Keep You Misinformed*, 2008. Regnery Press, November 11, 2008.

Horner is a lawyer at CEI. I do not own this, but Google books shows: pp.327-328 mention Wegman.

p.387, # 387 cites the WR, claiming it to be issued by the NAS.

p.395, #91-98 cite the WR also claiming NAS, Wegman testimony.

[**INT2010**] Interface 2010, *41st Symposium on the Interface: Computing Science and Statistics*, June 16-19, 2010, Seattle, WA. www.interfacesymposia.org/Interface2010/ScheduleforInterface2010.pdf

[**IPC1990** or **FAR**] *Climate change. The IPCC scientific assessment, working group 1 report*, 1990. IPCC, WMO and UNEP, edited by Houghton, J.T., Jenkins, G.J. and Ephraums, J.J. Cambridge University Press, 364 pp. (I do not actually have this report, just Figure 7.1c)

[**IPC1995** or **SAR**] *Climate change 1995; the science of climate change. Contribution of working group 1 to the second assessment report of the Intergovernmental Panel on Climate Change*. Edited by Houghton, J.T., Meira Filho, L.G., Callendar, B.A., Harris, N., Kattenbureg, A. and Maskell, K. Cambridge University Press, 572 pp. go2.wordpress.com/?id=725X1342&site=andyrussell.wordpress.com&url=http%3A%2F%2Fwww.ipcc.ch%2Fipccreports%2Fsar%2Fwg_1%2Fipcc_sar_wg_1_full_report.pdf&sref=http%3A%2F%2Fandyrussell.wordpress.com%2F

[**IPC2001** or **TAR**]: *Climate change 2001: the scientific basis. Contribution of working group 1 to the third IPCC scientific assessment*. Edited by Houghton, J.T., Ding, Y., Griggs, D.J., Noguer, M., van der Linden, P.J., Dai, X., Maskell, K. and Johnson, C.A. Cambridge University Press, 881 pp. www.grida.no/climate/ipcc_tar/wg1/index.htm

[**IPC2006**] 2nd order Draft of AR4, available 03/03/06. The AR4 1st -order draft was available 08/15/05, before the WP was formed. The 2nd -order draft 4 months before the WR. These were

available to anyone who asked, although labeled “Not to be quoted or cited.” These certainly could have been consulted.

hcl.harvard.edu/collections/ipcc index to 1st, 2nd drafts, comments pds.lib.harvard.edu/pds/view/7786989 Chapter 6, comments on 2nd pds.lib.harvard.edu/pds/view/7768990?n=538&imagesize=1200&jp2Res=25 Chapter 6: Paleoclimate, 2nd-order draft pds.lib.harvard.edu/pds/view/7768990?n=564 Ch. 6.6, Last 2000 Years

[**IPC2007** or **AR4**]: *Climate change 2007: the physical science basis*. Edited by Solomon, S., Qin, D., Manning, M., Chen, Z., Marquis, M., Averyt, K.B., Tignor, M. and Miller, H.L. Cambridge University Press, 996 pp. www.ipcc.ch/ipccreports/ar4-wg1.htm

*[**LAW2009**] Nigel Lawson, *An Appeal to Reason: A Cool Look at Global Warming*, 2009. (UK). Lawson mentions WR, p18, in related discussion pp.17-19.

[**LI2007**] Bo Li, Douglas W. Nychka, Caspar M. Ammann, “The ‘hockey stick’ and the 1990s: a statistical perspective on reconstructing hemispheric temperatures,” *Tellus* 59A (2007), 501-508. onlinelibrary.wiley.com/doi/10.1111/j.1600-0870.2007.00270.x/abstract [paywall]

This offered another approach to estimating uncertainties and looking at decadal maxima. Fig.4 shows an interesting statistical analysis and once again confirms the hockey stick. This reference is included because Wegman mentioned Nychka as a mainstream statistician [WEG2006c, p.6], **A.2**. Related later work by the same authors is: www.image.ucar.edu/~nychka/manuscripts/JASALiPaleo.pdf See also [TEB2005].

#[**MAS2008**] John R. Mashey, “ANOTHER ATTACK ON GLOBAL WARMING’S SCIENTIFIC CONSENSUS,” March 23, 2008 www.desmogblog.com/skeptics-journal-publishes-plagiarized-paper

+ [MAS2008a] John R. Mashey, “How to Learn About Science,” August 17, 2008. scienceblogs.com/deltoid/2008/08/john_mashey_on_how_to_learn_ab.php

#[MAS2009] John R. Mashey, “Science Bypass - Anti-science Petition to APS from folks with SEPP, George C. Marshall Institute, Heartland, CATO,” November 11, 2009.

www.desmogblog.com/another-silly-climate-petition-exposed

#[MAS2010] John R. Mashey, “Crescendo to Climategate Cacophony”

www.desmogblog.com/crescendo-climategate-cacophony

V1.0 03/15/10.

*[MCI2010] Stephen McIntyre, Memorandum to Parliament (CRU 32)

www.publications.parliament.uk/pa/cm200910/cmselect/cmsctech/memo/limatedata/uc3202.htm

*[MCS2010 or MW] Blakeley B. McShane and Abraham J. Wyner, “A Statistical Analysis of Multiple Temperature Proxies: Are Reconstructions of Surface Temperatures Over the Last 1000 Years Reliable?,” in press at the Annals of Applied Statistics:

www.imstat.org/aoas

[www.e-](http://www.e-publications.org/ims/submission/index.php/AOAS/user/submissionFile/6695?confirm=63ebfddf)

[publications.org/ims/submission/index.php/AOAS/user/submissionFile/6695?confirm=63ebfddf](http://www.e-publications.org/ims/submission/index.php/AOAS/user/submissionFile/6695?confirm=63ebfddf)

It is discussed in some detail in **A.12** , in which it is labeled MW.

*[MEN2010] Clive Menzies, Memorandum to Parliament (CRU 19),

www.publications.parliament.uk/pa/cm200910/cmselect/cmsctech/memo/limatedata/uc1902.htm

*[MIC2005] Patrick J. Michaels, Ed *Shattered Consensus - The true state of global warming*, 2005, Rowman & Littlefield Publishers, Copyright, GMI.

It includes chapters by Michaels, McKittrick, Balling, Cerveny, Christy Legates, Oliver Frauenfeld, Davis, Baliunas, Soon&Posmentier. The Foreword is by O’Keefe and Kueter of GMI. Chapter 2 (of 10) by McKittrick, is “The Mann et al Northern Hemisphere “Hockey Stick” Climate Index: A Tale of Due Diligence” occupies pp.20-49.

*[MIC2009] Patrick J. Michaels, Robert Balling, Jr , *Climate of Extremes – Global warming science they don’t want you to know*, 2009, CATO Institute (“published in cooperation with the George C. Marshall Institute”) WR discussion on peer review pp..200-201, hockey stick in p.217-219.

*[MON2010] A. W. Montford, *The Hockey Stick Illusion*, 2010.

This purports to tell the complete story, but the most interesting pieces are sadly missing. See [TAM2010] for a detailed review and commentary. For a fascinating history, see the Wikipedia talk page, in which any positive review, no matter how unqualified, is defended to the end. Search the second Wikipedia page for “dog astrology.” I wrote a short description of errors and especially the strange propagation of the David Deming email. No one was actually willing to answer the questions, but it incurred intense complaints and multiple deletion attempts via various rules inapplicable to Wikipedia talk pages.

bishophill.squarespace.com is his blog.

en.wikipedia.org/wiki/Talk:The_Hockey_Stick_Illusion

en.wikipedia.org/w/index.php?title=Talk:The_Hockey_Stick_Illusion&oldid=380146816

[NOR2006] Gerald North, “Surface Temperature Reconstructions for the Last Millenium,” 08/29/06, seminar at Texas A&M University.

geotest.tamu.edu/userfiles/216/NorthH264.mp4

North describes the history of hockey stick, Barton-Whitfield letters, the NRC panel he chaired [NRC2006], asked by NAS President Cicerone in response to Rep. Boehlert in Fall 2005.

10:30 Barton-Whitfield letters

11:30 Rep. Boehlert rebukes Barton, says get NRC

15:00 Barton gets own committee

16:30 Hockey stick first to try to do error bars, widely seen

18:30 Best guess in 1990 IPCC report, chart shown often lately

19:45 Wegman Report

“We got to see it about 3 days before the Congressional hearing”

20:00 Wegman, Scott, Said

20:30 NRC Report, strong panel

21:40 NRC 12 Anonymous referees, 70 pages, 2 monitors to make sure every criticism answered

22:15 Regarding WR “referees,” North paraphrases email:

“What about their referee job? They claimed it was refereed but in fact they just sent it out to some friends right at the last minute. And in fact, one of them that it was sent to, Grace Wahba, who some of you may know at Wisconsin, she sent me an email and she says: Hey they used my name and they said I was a referee. He sent it to me about 3 days beforehand and I sent him a bunch of criticisms which they didn’t take into account.”

22:45 WR network analysis, comments about coauthorship, statisticians

25:45 Discussion of temperature reconstructions

29:30 MM got PC right, but did it make any difference?

40:30 Ice core records, low-latitudes, like hockey stick

42:00 Glacier lengths, hockey stick

42:50 Boreholes, corals

45:50 Forcings, CO₂, CH₄

47:00 Sunspots

49:00 Volcanoes

50:00 Other reconstructions, new studies

51:40 Spaghetti curve, look at envelope

54:15 Put spaghetti with hockey stick error bars

56:50 30-year averages warmest, 400 years likely, 1000 years plausible

58:15 end of talk

58:50 MWP likely varied globally

01:01:50 LIA seems more global

01:03:00 Does it have anything to do with AGW? No key = physics.

+**[NRC2006]** Committee on Surface Temperature Reconstructions for the Past 2,000 Years. *Surface Temperature Reconstructions for the Past 2,000 Years*. The National Academies Press, 2006. Publicly available 06/22/06. www.nap.edu/catalog/11676.html See **[NOR2006]** for background history.

***[PEA2010]** Peabody Energy, Memorandum to Parliament (CRU 52), www.publications.parliament.uk/pa/cm200910/cmselect/cmsctech/memo/climatedata/uc5202.htm

#**[ORE2010]** Naomi Oreskes, Erik Conway, *Merchants of Doubt – How a Handful of Scientists Obscured the Truth on Issues from Tobacco Smoke to Global Warming*, May 25, 2010.

The long history of GMI and Singer is explored in meticulous detail. www.youtube.com/v/XXyTpY0NCp0 03/05/10 Oreskes talk at Brown U merchantsofdoubt.org

***[PLI2009]** Ian Plimer, *Heaven and Earth: Global Warming – The Missing Science*, 2009. (Australia.)

en.wikipedia.org/wiki/Heaven_and_Earth_%28book%29

Plimer’s pp.91-97 mostly consists of excerpts from the WR.

***[RAP2008]** Donald Rapp, *Assessing Climate Change: temperatures, solar radiation, and heat balance*, 2008. This book reused parts of the WR several years later, **[DEE2009, DEE2009a, DEE2010, MAS2010]**, including some of the text the WR plagiarized from Bradley(1999).

A 2010 2nd edition has the same issues.

[REZ2009] Hadi Rezazad, *Enhancement of Network Robustness and Efficiency through Evolutionary Computing, Statistical Computation and Social Network Analysis (2009)*

gradworks.umi.com/33/64/3364566.html

[DEE2010p] shows the plagiarized text comparison.

[RIG2003] J. T. Rigsby, J. L. Solka, “Social Networks and Computer Networks – Co-membership of Clients on a Network,”

Proc. Interface 2003. This seems to have led to **[RIG2004, RIG2005]**.

www.galaxy.gmu.edu/interface/I03/I2003Proceedings/RigsbyJohn/RigsbyJohn.presentation.pdf

[RIG2004] John Rigsby, Jeff Solka, “Actor Allegiance and Blockmodel Strength,” May 26-29, 2004, Interface 2004.

www.interfacesymposia.org/I04/I2004HTML/contents.html

This is also in Computing Science and Statistics, Volume 36, edited by Said, Marchette, Solka, i.e., Proceedings of Interface 2004, **A.6**.

www.interfacesymposia.org/I04/I2004Proceedings/RigsbyJohn/RigsbyJohn.paper.pdf

Both Program Chairs were Wegman’s PhD Students, Solka, Marchette. (1996). Marchette was affiliated with NSWC and Johns Hopkins, Solka with NSWC, and Rigsby with NSWC. A later (10/08/04) related talk was: “Allegiance, Blockmodels, and Computer Networks”:

www.galaxy.gmu.edu/stats/colloquia/AbstractsFall2004/TalkOct08.pdf

That seems to emphasize “social network methods for computers, “ a somewhat strange idea. The underlying graph theory is similar, but SNA

normally studies *humans*. Graph theory and statistical techniques have been used for decades to study computer and communication networks without calling them social networks.

Note: some text ascribed to this may actually be derived from the next, but the former was available online.

[RIG2005] Rigsby, J.T. *Block Models and Allegiance*, MS Thesis, George Mason University, 2005. I have not found an online copy, but it is occasionally referenced for text that looks similar to [RIG2004]. Computational Statistics Outstanding M.S. Thesis 2005:

www.galaxy.gmu.edu/stats/awards.html

W.5.2 offers the possibility that this “allegiance” idea is simply a reinvention of old ideas from SNA or cluster analysis, not used by anyone outside Wegman’s group, and published in venues unlikely to be noticed by experts. *Nobody expects MS students to know everything.*

[RUS2010] Muir Russell, *The Independent Climate Change E-Mails Review*, July 2010. (UK)

www.cce-review.org/pdf/FINAL%20REPORT.pdf

www.cce-review.org

This 160-page report has much detail on Climategate. Scientists were cleared of wrongdoing, but criticized somewhat for responses to FOI requests. Chapter 7 is particularly relevant.

[SAI2005] Yasmin Hassan. Said. AGENT-BASED SIMULATION OF ECOLOGICAL ALCOHOL SYSTEMS, Spring 2005.

www.galaxy.gmu.edu/stats/syllabi/IT871/MasterCopyDissertation.pdf

This includes the note:

“She enrolled in the Ph.D. Program in Computational Sciences in Fall 2003 and pursued the Ph.D. with a specialization in Computational Statistics with a 4.0 grade point average.”

It indeed is an impressive achievement to do a PhD in 2 school years.

Director: Wegman. Committee included David S. Anderson, Jeffrey L. Solka, William F. Wiczorek, David Wong, Jim X. Chen.

Outstanding Ph.D. Dissertation Award, 2005.

www.galaxy.gmu.edu/stats/awards.html

Unfortunately, 5 pages seem to be plagiarized, A.9. This file disappeared August 2010, A.11, although [DEE2010p] referenced a Google cached copy on 09/15/10, and another copy is at:

web.archive.org/web/20060905150733/www.galaxy.gmu.edu/stats/syllabi/IT871/MasterCopyDissertation.pdf

[SAI2007] Yasmin H. Said, *Experiences with Congressional Testimony: Statistics and The Hockey Stick*, George Mason University, *Data and Statistical Sciences Colloquium Series*, Sept 7, 2007.

www.galaxy.gmu.edu/stats/colloquia/AbstractsFall2007/TalkSept7.pdf

Infinite thanks to DC for this, which revealed much hidden information.

This key file disappeared August 2010, but a copy is shown in A.11.2, and of course DC saved one also:

deepclimate.files.wordpress.com/2010/09/said-talksept7.pdf

See also [GMU2007, SAI2005, WEG2010].

[SAI2008] Yasmin H. Said, Edward J. Wegman, Walid K. Sharabati, John T. Rigsby, “Social networks of author–coauthor relationships,” *Computational Statistics & Data Analysis* 52 (2008) 2177 – 2184.

Received 8 July 2007; accepted 14 July 2007.

web.ics.purdue.edu/~wsharaba/SNA/Author-Coauthor%20Relationships.pdf

[SAI2008a] Yasmin H. Said, Edward J. Wegman, Walid K. Sharabati, “Author-Coauthor Social Networks and Emerging Scientific Subfields,” CLADAG 2008.

web.ics.purdue.edu/~wsharaba/SNA/Preferential%20Attachment.pdf

[SCO2010] David Scott C.V., January 29, 2010

www.stat.rice.edu/~scotttdw/cv.pdf

This shows no obvious work in SNA or paleoclimate, *leading one to think that such topics were mainly addressed by others.*

[SHA2006] Walid Sharabati, “Edward J. Wegman’s Author-Coauthor Social Network,” monograph, pp.16-44 of [WEG2006c], as pp.1-28.

www.uoguelph.ca/~rmckitri/research/StupakResponse.pdf

[**SHA2008**] Walid Sharabati, *Multi-Mode and Evolutionary Networks*, 10/31/08. PhD Dissertation, GMU.
digilib.gmu.edu:8080/handle/1920/3384?mode=full
 Outstanding Ph.D. Dissertation Award, 2008, one of two that year, **W.5.7**.
www.galaxy.gmu.edu/stats/awards.html
 This is discussed [DEE2010p].

*[**SIN1999**] S. Fred Singer, *Hot Talk Cold Science – Global warming’s unfinished debate*, Revised 2nd Ed, 1999. I originally read this in 2001, at which time the disparity between ground stations and (some) satellite results was still a legitimate scientific argument.

*[**SIN2007**] S. Fred Singer, Dennis T. Avery, *Unstoppable Global Warming every 1,500 years*, 2007.
 It is a good exercise to read [SIN1999] first, and see evolution or lack thereof, especially in the light of major revisions to satellite and balloon results made around 2005. First global warming was not happening, then it was happening, but only natural. Policy advice stays constant: no CO2 restrictions. Much of Chapter 5, p.55-74, is attack on hockey stick.

*[**SOL2008**] Lawrence Solomon, *The Deniers*, 2008. (Canada).
 This collected a series of articles in the National Post .
www.desmogblog.com/the-deniers-the-world-renowned-scientists-who-dont-actually-deny-global-warming
 Chapter 2 of 14 (p.9-22), “The Case of the Disappearing Hockey Stick” gives Wegman and this topic the most prominent place in the book.

*[**SOO2003**] Willie Soon, Sallie Baliunas, “Proxy climatic and environmental changes of the past 1000 years,” *Climate Research* Vol. 23:89-110, 2003. Submitted: 04/11/02; Accepted: 08/29/02; Published 01/31/03.
www.cfa.harvard.edu/~wsoon/1000yrclimatehistory-d/Jan30-ClimateResearchpaper.pdf
en.wikipedia.org/wiki/Soon_and_Baliunas_controversy
 This (*poor*) paper by two astrophysicists eventually resulted in mass resignations, because the publisher would not let Editor-in-Chief von Storch repudiate it. See [DEF2002] and the longer de Freitas discussion in

[MAS2010], in which Soon was reviewing [DEF2002] during the time when de Freitas was handling this paper.
 This paper strongly references early Lamb work (from which the [IPC1990] sketch was derived) and devotes much effort to claiming global synchrony and strength of the MWP, so it seems one of the main antecedents for Meme-56. It references 7 of the WR’s uncited or weakly cited papers: Biondi (1991), Bradley, Jones (1993), Briffa (2001), Crowley, Lowery (2000), Huang, et al (1997), Huang (2000), Jones, et al (1998). While these are referenced elsewhere, it seems possible that this paper may have been the original source of some, selected either by MM+TT or Spencer.

#[**TAM2010**] tamino, “The Montford Delusion,” 07/22/10
www.realclimate.org/index.php/archives/2010/07/the-montford-delusion
 tamino is the pseudonym of a statistical expert (especially on time series) with a strong, credible history of articles at his blog:
tamino.wordpress.com
 His real-world identity is well-known and should be obvious to anyone who looks at that site. He has authored peer-reviewed work whose coauthors are well-published climate scientists. [MON2010] created a story in essence from McIntyre’s viewpoint. This is credible debunk in one place, a useful place to start for the reader who does not want to wade through numerous blog posts to find the statistics discussion. The attached commentary thread is also enlightening.

[**TEB2005**] Claudia Tebaldi, Richard L. Smith, Doug Nychka, Linda O. Mearns, “Quantifying Uncertainty in Projections of Regional Climate Change: A Bayesian Approach to the Analysis of Multimodel Ensembles,” *J. Climate* 18, 1524-1540.
journals.ametsoc.org/doi/full/10.1175/JCLI3363.1
 See also [LI2007].

[**USC2005**] US Climate Change Science Program, Participants, 14=16 Nov. 2005.
www.climatechange.gov/workshop2005/participants.htm
 Attendees include: Ebell, Horner (CEI); Herlong, Kueter (GMI); Singer, McIntyre, Spencer, Wegman, Said.

[**WEG2005**] Resume of Edward Wegman, October 2005.
web.archive.org/web/20060220212732/galaxy.gmu.edu/stats/faculty/wegman.resume2.htm

Research interests included:

- “Statistical Graphics and Scientific Visualization
- Computational Statistics
- Time Series Analysis
- Function and Curve Estimation including Splines
- Inference under Order Restrictions
- Parallel Computing
- Massive Data Sets and Data Mining
- Modeling Alcohol Use Behavior”

This shows no obvious work in social networks or paleoclimate, which were added later, seen in [WEG2010].

*[**WEG2006** or **WR**] Edward J. Wegman, David W. Scott, Yasmin H. Said, “*Ad Hoc Committee Report On The ‘Hockey Stick’ Global Climate Reconstruction,*” 07/14/06.
republicans.energycommerce.house.gov/108/home/07142006_Wegman_Report.pdf

This is usually just called the Wegman Report (WR) here.

[**WEG2006b**] Edward J. Wegman, testimony 07/27/06, part of [BAR2006]
republicans.energycommerce.house.gov/108/Hearings/07272006hearing2001/Wegman.pdf

[**WEG2006c**] Edward J. Wegman, “Response of Dr. Edward Wegman to Questions Posed by the Honorable Mr. Bart Stupak in Connection with Testimony to the Subcommittee on Oversight and Investigations,” no later than 08/02/06.
deepclimate.files.wordpress.com/2010/04/stupakresponse-reduced-with-appendix.pdf OR
www.uoguelph.ca/~rmckitri/research/StupakResponse.pdf which has: Appendix A, pp.15-44, is written by Sharabati regarding Wegman’s coauthorship network, and cited here as [SHA2006].

*[**WEG2007**] Edward J. Wegman, “20 Questions Statisticians Should Ask!,” 10/27/07.

www.image.ucar.edu/public/Workshops/ASAclimate/wegmanASA.htm

This asks many questions *whose answers Wegman should have known if he had read the literature or even the WR’s own references,* **A.4.**

[**WEG2010**] Resume of Edward Wegman, February 2010.
www.galaxy.gmu.edu/stats/faculty/wegman.resume2.pdf

Research interests included those in [WEG2005], plus

- “Text Mining
- Social Networks
- Statistical Methods for Computer Intrusion Detection
- Paleoclimate Reconstruction”

See **A.11.**

[**WIK2006**] Wikipedia, Colors of Noise, April 12, 2006 version
en.wikipedia.org/w/index.php?title=Colors_of_noise&oldid=48074837#Technical_definitions See [DEE2010f], search for “color,” for discussion.

[**WIK2006a**] Wikipedia, “Social Networks” (January 2, 2006 version),
 See [DEE2010h, DEE2010
en.wikipedia.org/w/index.php?title=Social_network&oldid=33590649

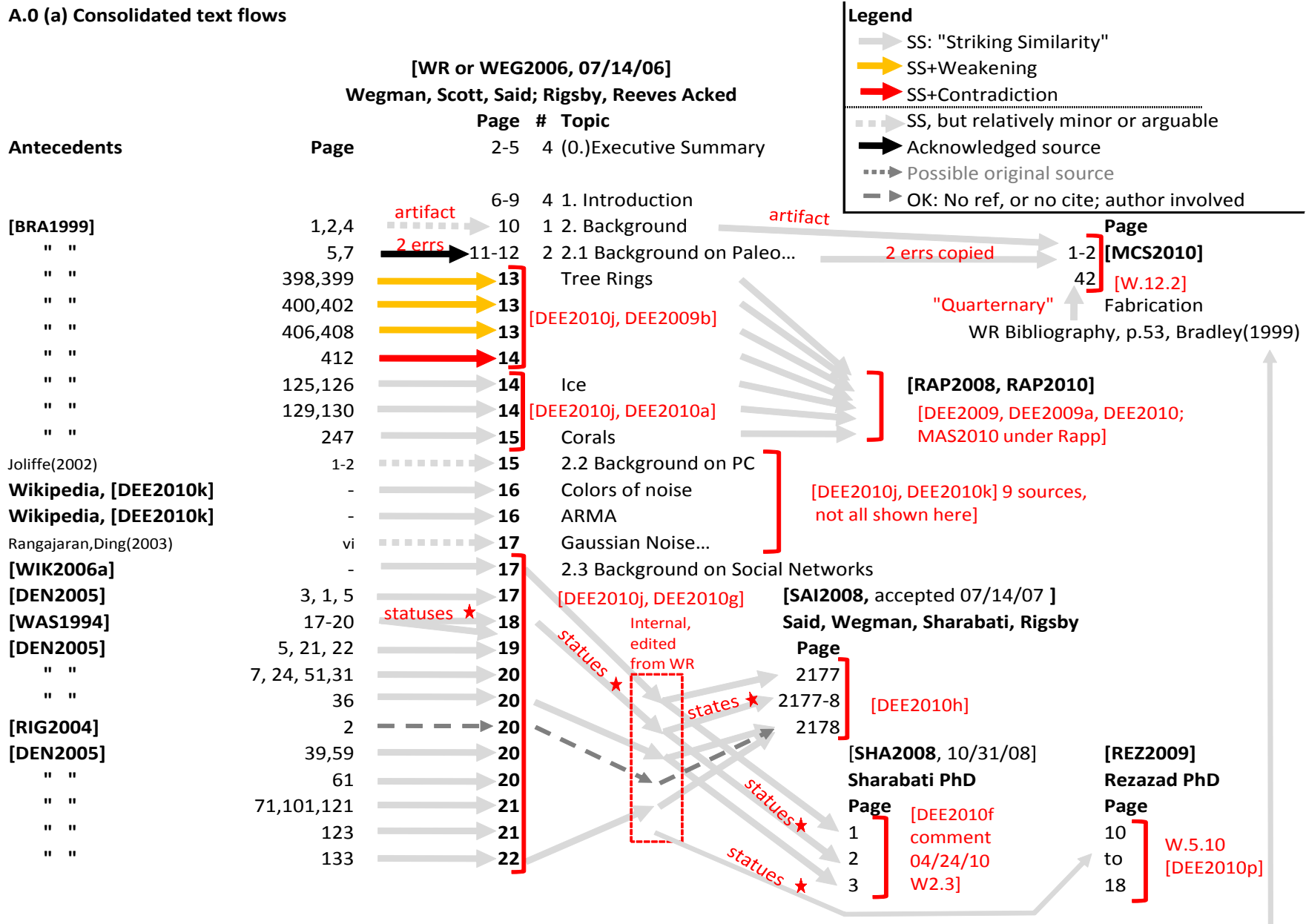
[**WSJ2006**] Wall Street Journal Editorial, “Hockey Stick Hokum,”
 07/14/06.

republicans.energycommerce.house.gov/108/News/07142006_1990.htm
 WSJ knew about this no later than 07/13/06, before it was announced, which of course is the way good PR campaigns work.

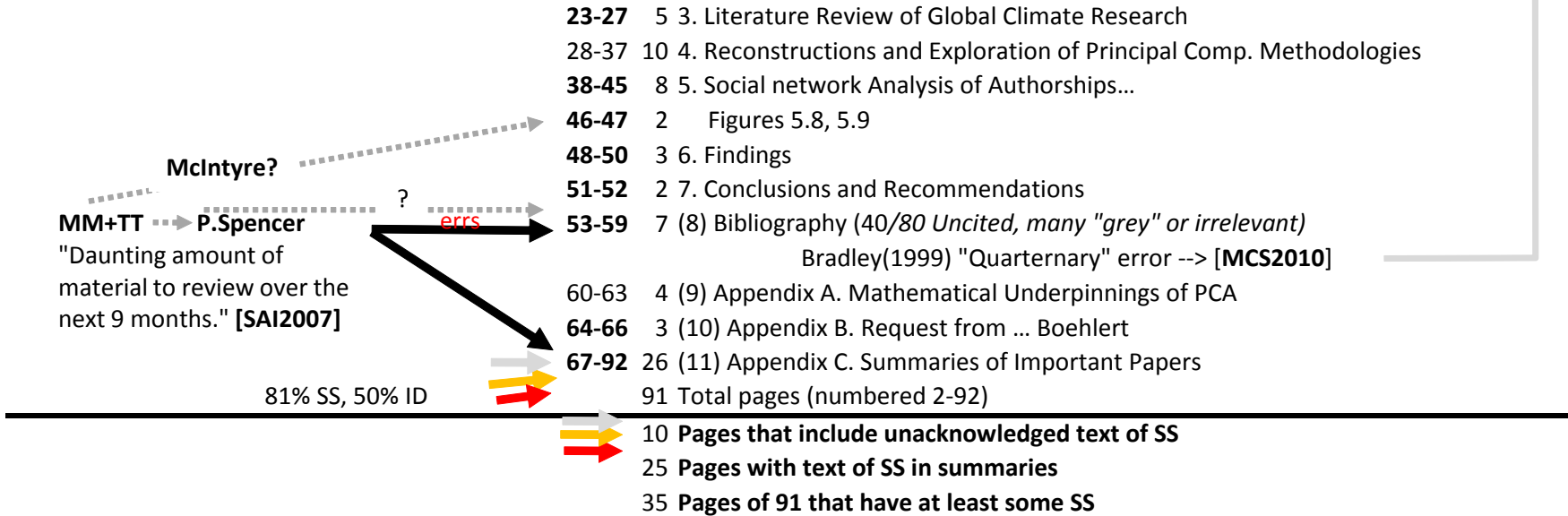
[**ZOR2010**] Eduardo Zorita, “McShane and Wyner on climate reconstruction methods,” 08/19/10.
klimazwiebel.blogspot.com/2010/08/mcshane-and-wyner-on-climate.html
 Zorita is a climate researcher who (with von Storch) has often criticized MBH, but generally within the normal processes of science. Such arguments inside science are worth comparing with PR attacks from outside.

A.0 From source to WR and beyond

A.0 (a) Consolidated text flows



A.0 (b) Consolidated text flows



[WIK2006a] http://en.wikipedia.org/w/index.php?title=Social_network&oldid=33590649 01/02/06 version

Prof. Shakashiri		
www.scifun.org/CHEMWEEK/PDF/Ethanol.pdf	[SAI2005] Said PhD Dissertation	A.9, [DEE2010p]
1-2	→ 6-10	5 1.1 Ethanol, Ethyl, Grain Alcohol, Alcohol

A.1 Comments, but not reviews

The WR and follow-ons often denigrate paleoclimate review quality, but with no real evidence, Meme-b. As to review practices around the WR, such evidence as exists is not positive.

A.1.1 NRC Panel Led by Gerald North

An NRC panel includes a wide range of relevant experts, is peer-reviewed by other experts, anonymous to the panel, but listed later. Others (“monitors”) manage the review process, requiring that every question be answered, by North’s description, in this case 70 single-spaced pages [NOR2006]. The Panel was distinguished [NRC2006, p.6]:

“GERALD R. NORTH (*Chair*), Texas A&M University, College Station
FRANCO BIONDI*, University of Nevada, Reno*
PETER BLOOMFIELD, North Carolina State University, Raleigh
JOHN R. CHRISTY, University of Alabama, Huntsville
KURT M. CUFFEY, University of California, Berkeley
ROBERT E. DICKINSON, Georgia Institute of Technology, Atlanta
ELLEN R.M. DRUFFEL, University of California, Irvine
DOUGLAS NYCHKA, National Ctr. for Atmospheric Research, Boulder, CO
BETTE OTTO-BLIESNER, Nat. Ctr for Atmospheric Research, Boulder, CO
NEIL ROBERTS, University of Plymouth, United Kingdom
KARL K. TUREKIAN, Yale University, New Haven, Connecticut
JOHN M. WALLACE, University of Washington, Seattle”

The referees were also distinguished.

“David Brillinger, University of California, Berkeley
David Chapman, University of Utah
Julia Cole, University of Arizona
Thomas Crowley*, Duke University
Alexander Flax, Independent consultant
Claus Fröhlich, PMOD Technologies
Ricardo Garcia-Herrera, Universidad Complutense de Madrid
Peter Huybers*, Woods Hole Oceanographic Institution
Richard Muller, Lawrence Berkeley Laboratory
Robert Stine, University of Pennsylvania
Lonnie Thompson, The Ohio State University
Connie Woodhouse, National Oceanic and Atmospheric Administration”
Carl Wunsch*, Massachusetts Institute of Technology

The WR cited peer-reviewed articles by *’d authors.

A.1.2 WR Background

As a reminder, the WR project included:

- 2 distinguished statisticians of long association, Wegman and Scott, although Scott seems uninvolved beyond Appendix A. Others were or had been Wegman’s students.
- Statistician Said (PhD, 2005).
- Unknown 4th person who dropped out, [SAI2007, p.5].
- With help acknowledged from a several other statisticians, Rigsby (MS 2005) and Reese (PhD 2009).¹⁵
- Help from another statistics student, Sharabati (PhD 2008) for [WEG2006c, SHA2006], offered later in support.

NRC panels and referees are multidisciplinary and widely spread among institutions. Given the large number of WR errors and issues described in this report, the extent and independence of its internal review is unclear. The external review seems to have been a gathering of quick comments from a few associates, not reviews in the usual sense, certainly not peer-reviews and certainly not anonymous.

[BAR2006]:

“Following receipt of the letter responses, committee staff informally sought advice from independent statisticians to determine how best to assess the statistical information submitted. Dr. Edward Wegman, a prominent statistics professor at George Mason University who is **chair of the National Academy of Sciences’ (NAS) Committee on Applied and Theoretical Statistics**, agreed to independently assess the data on a pro bono basis. Wegman is also a **board member of the American Statistical Association**.

About the Wegman committee: ... **All worked independent of the committee, pro bono**, at the direction of Wegman. In the course of Wegman’s work, **he also discussed and presented to other statisticians on aspects of his analysis, including the Board of the American Statistical Association.**”

¹⁵ Scott was certainly with Rice. Said was labeled as affiliated with Johns Hopkins, true during most of the WR work, but may not have been true at the time of the final report. Given affiliations listed, a casual WR reader may not have noticed that Said (JHU), Rigsby (NSWC), and Reese (MITRE) were all recent or current Wegman students. Reese’s involvement is especially unclear.

They did not work independent of Barton's committee, because much of their reading material was provided to them by Barton staffer Peter Spencer, [SAI2007, p.5], Theme-N⁴.

Pro bono was possibly true at the time, arguable later, as government research grants were later acknowledged for work that was done a part of this effort. Other than not getting directly paid, the meaning of this is quite unclear for professors and part-time graduate students, **A.7**. The ASA has good ethical guidelines, not obviously followed, **A.8**

Here, Wegman's role in NAS CATS is mentioned, correctly, but an unwary reader might be forgiven for thinking there was some NAS involvement. There was none. However, in the testimony, Whitfield goes even further .

A.1.3 WR Testimony

[BAR2006a, p.5]

"PREPARED STATEMENT OF THE HON. ED WHITFIELD, ...
Dr. Wegman is Chairman of the National Academy of Sciences Committee on Applied and Theoretical Statistics. **At the Committee request**, Dr. Wegman assembled an ad-hoc committee of statisticians to examine the hockey stick studies and related articles."

[BAR2006a, p.4]

"MR. WHITFIELD: ...
Dr. Wegman is Chairman of the National Academy of Sciences Committee on Applied and Theoretical Statistics, **and at the committee's request** he assembled this ad hoc committee of statisticians ...
... and I can tell you right now that his document has been peer reviewed also, and we will get into that later."

<EB> Wegman indeed was the Chairman of NAS CATS (2004-2007) but the WR was not requested by CATS and had no connection with it.¹⁶ The peer-review claim is clearly wrong, **A.1**, Theme-N⁴. Whitfield seems to have told 2 clear untruths to Congress, **A.10**.

¹⁶ Whitfield might claim that the "Committee request" meant the House Committee on Energy and Commerce. The reader may decide if that is reasonable, given that the House Committee is not mentioned in Whitfield's prepared statement until the next page. The NAS idea has persisted.

[BAR2006a, p.147]

"MR. STEARNS. Okay. Dr. Cicerone, you are the President of the National Academy of Science. Dr. Wegman is an appointed member of the National Academy of Science Board of Mathematical Sciences and Their Application. He is chair of the NAS Committee on Applied and Theoretical Statistics, highly credentialed in math and statistics, wouldn't you say?"

NAS CATS is a legitimate part of Wegman's background, but the thoughtful reader might wonder about the repetition of this, although Stearns did not go as far as Whitfield.

[BAR2006a, p.7]

"MR STUPAK: ...

But we note that Dr. Wegman's work is not yet published or peer reviewed so it is very difficult for us to evaluate his work. Dr. Wegman's criticism of Dr. Mann should have been interdisciplinary and include a statistician can also be said of Dr. Wegman's work. "

"DR WEGMAN: ...

Because of this apparent isolation, we decided to attempt to understand the paleoclimate community by exploring the social network of authorships in the temperature reconstruction area. ...

Although we have no direct data on the functioning of peer review within the paleoclimate community but, with me having 35 years of experience with peer review in both journals as well as evaluation of research proposals, peer review may not have been as independent as would generally be desirable."

This idea was espoused in MM05x, Meme-b¹. The Wegman team went off into a new area, for whose introductory material they had to plagiarize textbooks, **W.2.3**, **W.5**. As for Wegman's long experience with peer review, he may wish to explain [SAI2008], **W.5.6**, Theme-N⁴.

[BAR2006a, p.36]

MR. STUPAK. Thank you, Mr. Chairman. Dr. Wegman, in your report you criticized Dr. Mann for not obtaining any feedback or review from mainstream statisticians. **In compiling your report, did you obtain any feedback or review from paleoclimatologists?**

DR. WEGMAN. **No, of course not, but we weren't addressing paleoclimate issues.** We were addressing—

Yes, they certainly were addressing paleoclimate. The **Page tally**, **§2.7**, identifies 47 of 91 pages devoted in whole or part to paleoclimate.

Reasonable people might disagree with some of those, but any way one counts, the WR spends much of its text on paleoclimate, Theme-N⁴.

MR. STUPAK. **But you said you had difficulty understanding some of the terms of art that Dr. Mann used and you had to call your social network to figure it out so wouldn't it have been helpful to have paleoclimatologists?**

DR. WEGMAN. **To say that I didn't contact any climate people is not entirely accurate.** We have—

MR. STUPAK. But they weren't used in compiling your report--that was the question--correct?

DR. WEGMAN. **Well, I am not sure how to answer that.** I certainly—

MR. STUPAK. Well, **yes or no** is probably the best way.

Did you have any paleoclimatologists when you compiled your report?

DR. WEGMAN. **Not on our team, but that doesn't mean I didn't talk to any.**

[WEG2006c, p.7] says:

“Ans: I spoke with no one in paleoclimate studies. To the best of my knowledge neither have my colleagues.”

This is either a different answer, a week later, or Wegman mis-answered the question about “paleoclimatologists” as “climate people,” Theme-N⁴. *Many people might be called “climate people” of whom only some are paleoclimatologists.*

[BAR2006a, p.36]

MR. STUPAK. **Did anyone outside your social network peer review your report?**

DR. WEGMAN. Yes.

MR. STUPAK. Who was that?

DR. WEGMAN. **Well, Enders Robinson.**

MR. STUPAK. Is that the e-mail we were talking about earlier?

DR. WEGMAN. Pardon?

MR. STUPAK. Is that the e-mail that was--

DR. WEGMAN. Yes. So--

MR. STUPAK. When you do peer review--

DR. WEGMAN. Let me answer the question. **Enders Robinson,**

Grace Waba, who is a member of the National Academy, **Noel Cressy,** who is at the Ohio State University, **Bill Wasorik,** who is at Buffalo State SUNY,

David Banks, who is at Duke University, **Rich Schareen** is the immediate past president of the American Statistical—
Wegman has a strange idea of his network, **A.1.5, A.6, W.5,** Theme-K⁴.

[BAR2006a, pp.36-37]

MR. STUPAK. Let me ask you this question. If you had a peer review, when are peer reviews usually done? Before a report is finalized or after?

DR. WEGMAN. We had submitted this and had feedback from--

MR. STUPAK. No, no, I am talking about general peer review. If you are going to have a peer review, don't you usually do it before you finalize your report?

DR. WEGMAN. Yes.

MR. STUPAK. Well, your peer review was after you finalized it?

DR. WEGMAN. **No, it was before. We submitted this long before.**

It was not peer review in any sense, but Wegman does not correct that. The meaning of “long before” is unclear, **A.1.5.** Theme-N⁴.

MR. STUPAK. Well, when was your report finalized?

DR. WEGMAN. I think we dated the final copy about 4 days ago.

“Finalized” and “dated” are not the same. The final report was dated 07/14/07, but the PDF was last modified 07/12/06, **A.5,** Theme-N⁴.

MR. STUPAK. Four days ago, so that would be about July 15. This e-mail sort of indicates it is July 17 that you asked for this peer review.

DR. WEGMAN. I had feedback from Enders much earlier than that. We had asked him to send material to us for purposes of coming here.

MR. STUPAK. Well, the e-mail read into the record is

Tuesday, July 18, so that would be 3 days after you finalized your report.

DR. WEGMAN. I am sorry. We--

MR. STUPAK. Have you seen this e-mail, the one that--

DR. WEGMAN. Yes, of course I have. Dr. Robinson saw our material before the 18th, before the 17th, before the 16th. He gave us feedback. We incorporated that. He gave us feedback verbally. We incorporated that because there was some interest in getting this report to the committee....

MR. STUPAK. In doing peer reviews, do scientists who do the report, do they usually submit to people they want to do the peer review? Isn't that sort of an independent review?

DR. WEGMAN. **This is basically the same mechanism that was used at the National Academy.** The national--you know, this is not a—

No, it is not, **A.1.1,** Theme-N⁴, Robinson's email can scarcely be called a review in any normal sense.

MR. STUPAK. Did you ask these people to do your peer review?

DR. WEGMAN. Yes.

MR. STUPAK. So would they be part of your social network?

DR. WEGMAN. No. When I talk about social network, I am talking about people with whom I have actively collaborated in writing research papers.

This is a strange idea of social network. Neither SNA researchers nor the general public restrict “social network” to collaborators on research papers. In fact, WR, p.18’s copy of [WAS1994] lists “extensive range and type of social ties,” **W.2.3, A.6**, Theme-M⁴.

MR. STUPAK. It sounds--

DR. WEGMAN. None of these people have actively collaborated with me in writing research papers.

This is factually incorrect regarding Wegman, given coauthorship with Amy Braverman, but more generally misleading. No one asked the more inclusive question of reviewer connections with the WP, **A.1.5, A.6**, Theme-N⁴.

MR. STUPAK. Isn't the same kind of social network you criticized Dr. Mann on because the people that reviewed his were paleoclimatologists?

DR. WEGMAN. Were the people that had actually worked with and published papers with.

MR. STUPAK. And you have published papers with some of these people that peer reviewed your report?

DR. WEGMAN. **No. I just told you no, I haven't.**

Wegman still forgets Braverman, *very strange since she was then President of IFNA, long run by Wegman*, **A.1.5, A.6**, Theme-N⁴.

[**BAR2006a**, p.38]

“MR. STUPAK. Okay. Let me ask you this question. **Have you reviewed any of Mr. Mann's later refinements of his 1999 report?**

DR. WEGMAN. **I have reviewed some level of detail, not in intense level of detail, the continuing papers, most of which are referenced--in fact, the ones that are referenced—**

The WR spends 7 pages of Summaries on Mann, et al (2000), Mann, Jones (2003), Mann, et al (2005), and Rutherford, et al (2005). It cites Jones, Mann (2004). They are labeled as Important, but essentially ignored. Still, the WR includes 10 mentions of the PCA replacement RegEM, not just in Summaries, but in the Literature Review, p.24.

MR. STUPAK. **Did he refine his data and his methodology?**

DR. WEGMAN. **My take on the situation is that rather than accept the criticism that was leveled, he rallied the wagons around and tried to defend this incorrect methodology.**

MR. STUPAK. **But did he refine his methods in later studies that he conducted, not whether he rallied the troops? Did he refine his methods? Was his job more accurate as he went on with later reports?**

DR. WEGMAN. **I believe that he does not acknowledge his fundamental mistake and that he has developed additional papers with himself and his colleagues that try and defend the original hockey stick shape.**

MR. STUPAK. **Do you know that or are you just guessing?**

DR. WEGMAN. **I am guessing that.**

One might ask if Wegman had personally studied (or even read) the key papers or even the WR itself, Theme-N⁴, Meme-d¹. *Did Wegman’s student (s) write most of the WR?*

MR. STUPAK. Okay. Statisticians, should they guess or should they have facts to--

DR. WEGMAN. That is called statistical estimation, yes.”

It is not “statistical estimation” to know the simple fact that Mann and others had upgraded techniques, as to RegEM for example, Theme-N⁴.

[**BAR2006a**, p.63-65]

“PREPARED STATEMENT OF DR. THOMAS J. CROWLEY...

Finally, I would like to comment that the Wegman Report now before the committee has not undergone any extensive peer review from anyone in the climate community prior to its submission to the committee for inclusion into the record and, most problematically, possible use as a guide to further recommendations by the committee.”

[**BAR2006a**, p.69]

“PREPARED STATEMENT OF DR. HANS VON STORCH...

In a long discussion, von Storch often expresses concerns about issues of peer review in general, regression in MBH, the IPCC, etc. von Storch has long been critical of MBH results in various ways, but has carried out arguments in credible peer-reviewed journals, i.e, normal arguments within the scientific process, not disputing basic physics.

[**BAR2006a**, p.102]

“JULY 27, 2006...

MR. WHITFIELD:

Dr. Wegman **was not seeking to impugn the integrity** of any of the scientists who work in the area¹⁷, but it is clear that peer review somehow failed to pick up the flaws in the hockey stick studies. ...

DR. WEGMAN: ...

The fact is Dr. Mann continues to appeal to peer review **but the fact is the peer review process failed in the 1998 paper.**

MRS. BLACKBURN. And you would say that was primarily **because it was not an independent and separate review outside of that social network?**

DR. WEGMAN. I believe that is the case....

Given the evidence in this report, is there any reason to ascribe the slightest credibility to Wegman's unsupported opinions about paleoclimate peer review, stated as "It is a fact ..." If so, consider W.5.6.

[BAR2006a, p.159] Mann:

“The **participation of statisticians in climate science has become so routine that there is an entire field of climate research known as "statistical climatology," which involves the collaboration of large numbers of statisticians and climate scientists.**

[BAR2006a, p.160] Mann:

“I participated as a graduate student in GSP's inaugural workshop in 1994. Many leading statisticians (e.g., Dr. Grace Wahba, Dr. Arthur Dempster, and Dr. Noal Cressie) were participants.”

[BAR2006a, p.161-162] Mann:

“So let us consider just my climate-related papers (i.e., post 1993), as Wegman purports to do. In climate research, I had 14 co-authors through the year 1999. I had 101 co-authors through the end of 2005. ...

Apart from the fact that even my closest collaborators are perfectly willing to criticize my work when they think it is flawed, Wegman's math just does not

¹⁷ This is a strange statement given:

WR, p.26: “Making conclusive statements without specific findings with regard to atmospheric forcings suggests a lack of scientific rigor and **possibly an agenda.**”, WR pp.35-45 on SNA, and [WEG2006c, p.11] “Some immediate thoughts we had on Wahl and Ammann was that Dr. Mann lists himself as a Ph.D. coadvisor to Dr. Ammann on his resume. As I testified in the second hearing, the work of Dr. Ammann can hardly be thought to be an unbiased independent report.”

The whole SNA analysis seems manufactured to impugn integrity, W.5.

support his theory. As indicated above, the vast majority (86%) of my co-authorships occurred after my 1998/1999 studies. ...

The peer review process ensures only that basic mistakes are not made, that the article acknowledges the existing literature on the subject, and that it contributes in some way to the exploration of important scientific issues. But peer review does not and cannot vouch for the accuracy of the paper. That is the function of the scientific process, by which other scientists test out and question the work of their peers.”

The above commentary is a standard view, hardly unique to Mann.

[BAR2006a, pp.163-164] Mann:

“There is another element of this question which raises a deeply troubling matter with regard to **Dr. Wegman's failure to subject his work to peer review, and Wegman's apparent refusal to let other scientists try to replicate his work.** Professor David Ritson, Emeritus Professor of Physics, Stanford University, has found error in the way that **Dr. Wegman models the "persistence" of climate proxy data.** Interestingly, this is the same error Steven McIntyre committed in his work, which was recently refuted in the paper by Wahl and Ammann, which was in turn vetted by Dr. Douglass Nychka, an eminent statistician. Dr. Ritson has determined that that the calculations that underlie the conclusions that Dr. Wegman advanced in his report are likely flawed. Although Dr. Ritson has been unable to reproduce, even qualitatively, the results claimed by Dr. Wegman, he has been able to isolate the likely source of Wegman's errors. What is so troubling is that Dr. Wegman and his co-authors have ignored repeated collegial inquiries by Dr. Ritson and apparently are refusing to provide any basic details about the calculations for the report (see Attachments 3 and 4 to this Response). **It would appear that Dr. Wegman has completely failed to live up to the very standards he has publicly demanded of others.** Moreover, the errors that Dr. Ritson has identified in Dr. Wegman's calculations appear so basic that they would almost certainly have been detected in a standard peer review. **In other words, had Dr. Wegman's report been properly peer-reviewed in a rigorous process where peer-reviewers were selected anonymously, it likely would not have seen the light of day.** Dr. Wegman has thus unwittingly provided us with a prime example of the importance of the peer review process as a basic first step in quality control.”

This report illustrates many more peer review problems in the WR and later work, **A.1, W.2.3, W.5.**

A.1.4 Response to Stupak

[WEG2006c, pp.7-8]:

“e. Please list all the authors in paleoclimate studies with whom you or your coauthors spoke

Ans: I spoke with no one in paleoclimate studies. To the best of my knowledge neither have my colleagues.”

The WR references, but does not cite Cronin (1999) written by a credible paleoclimate author who was or had been an Adjunct Professor at GMU, which has other credible people in the field as well, Theme-N④. *Surely such people could have been consulted.*

“6. You testified that other scientists or statisticians reviewed your report before it was sent to the Committee on Energy and Commerce, but it was unclear whether you provided a complete list. Please list the people who reviewed your report before it was sent to Committee, including name, title, area of expertise, and university or other affiliation.

- Professor (emeritus) Enders Robinson, geophysics, ColumbiaUniversity, elected member of the National Academy of Engineering
- Professor Grace Wahba, statistics, University of Wisconsin, Madison, elected member of the National Academy of Science
- Professor Noel Cressie, spatial statistics, Ohio State University
- Professor David Banks, statistics, Duke University, Editor of Applications Section, *Journal of the American Statistical Association*
- Professor William Wiecek, geophysics, Buffalo State SUNY
- Dr. Amy Braverman, Senior Scientist¹⁸, remote sensing, datamining, Jet Propulsion Laboratory (CalTech)
- Dr. Fritz Scheuren, statistics, NORC, University of Chicago, the100th president of the American Statistical Association
- In addition, we had two other reviewers who asked that their names not be revealed because of potential negative consequences for them.”

Most are long-time associates, although of varying degrees of closeness, then and now. **Criticism of this review process does not extend to any of these people, of whom this discussion implies no negative connotations whatsoever.** At least some were clearly mis-used, **A.1.5.**

“7. Prior to sending your report to the Committee on Energy and Commerce, was your report peer reviewed, i.e. did someone other than the authors select the reviewers, were reviewers allowed to submit comments

anonymously, was someone other the authors involved in deciding whether the authors’ responses were adequate?

Ans: Our report was not peer reviewed in the sense you ask. The review process we went through was similar to that employed by the National Research Council. At the NRC, the Committee makes recommendations to the Committee Chair and the Study Director. The list is narrowed and a recommendation is made by the Study Director. This list is approved by a higher-level authority and the document is sent out for review. **The reviewers are not anonymous and their names are listed in the document.** This was true of the recent North Study on Paleoclimate Reconstruction that was also the subject of our first round of testimony. Because we did not have the NRC structure, we obviously did not have a higher-level review of our list, but to the best of our ability, we acted in good faith to obtain reviews, **some of which expressed dissenting opinions.**

Subsequently, we have been preparing papers that will be peer reviewed for the Applications Section of the *Journal of the American Statistical Association*¹⁹, another for the journal called *Statistical Science*⁶ published by the Institute of Mathematical Statistics, and finally for a more popular outlet called *Chance*. In addition, we are preparing a paper motivated by our social network studies on the styles of co-authorship.”

“⁶ The *Statistical Science* article will have even more rigorous scrutiny than a normal peer review. It will be a discussion paper meaning that discussants will have an opportunity to comment in writing for the audience to see.”

That was July 2006. Perusal of Wegman’s 2010 C.V. [WEG2010] shows the only result: [SAI2008], a paper with quite serious problems, **W.5.6.** *Were the first 3 papers actually ever written and submitted?* Richard Smith mentioned *Statistical Science* issue, but that seems not to have happened, **A.3.** *Quick comments are not usually called “reviews.”*

Wegman finally clarifies the fact that the WR was not peer-reviewed in any normal sense and its process was not like that of the NRC, Theme-N④.

Click on NRC’s “Our Study Process” at top of page in:

sites.nationalacademies.org/NRC/PoliciesandProcedures/index.htm

“As a final check on the quality and objectivity of the study, all National Academies reports—whether products of studies, summaries of workshop proceedings, or other documents—**must undergo a rigorous, independent external review by experts whose comments are provided anonymously to**

¹⁹ David L. Banks was the editor of that Section of JASA, and coauthored a paper with Said in May 2006 in the next journal, *Statistical Science*, **W.1.5, A.6..**

¹⁸ <e> She was Senior Statistician, an odd error on Wegman’s part.

the committee members The National Academies recruit independent experts with a range of views and perspectives to review and comment on the draft report prepared by the committee..... **The names and affiliations of the report reviewers are made public when the report is released.”**

The WP selected its reviewers, with many strongly connected to Wegman and/or Said. Nothing is wrong with senior researchers knowing each other and asking them to comment on papers. Academe is especially enmeshed in well-interconnected social networks, especially among experienced people. Wegman’s network is demonstrably large, from his C.V., **A.6.**

However, an NRC panel is carefully *not* composed of:

- a senior researcher,
- his young students and
- a long-time associate, barely involved

who together ask a few people concentrated in the same field to do quick reviews, perhaps without previous commitment or reasonable notice. In serious reviews, potential reviewers are given schedules before they commit to do them. Suggestions are taken seriously.

This is nothing like the strict, high-quality NRC process.

One might send a draft research paper to friends for a last quick check, but such a process seems especially inappropriate for a high-profile report to Congress that intensely attacks another discipline’s quality of research and peer review.

A.1.5 WR “Reviewers” or commenters

It is worth checking the list for expertise and WP relationships and process.

No negative connotation is implied by inclusion here. *Some “reviewers” were clearly mis-used in this process, asked to comment on a long report, sometimes on short notice or with comments and suggestions ignored.* The WR and testimony offered not the slightest proof of any meaningful review, and such evidence as exists suggests its absence.

In [WEG2006c, p.8], quoted earlier in A.1.3, Wegman says:

“to the best of our ability, we acted in good faith to obtain reviews, **some of which expressed dissenting opinions.**”

One would like to know:

How was this review presented to the reviewers/commenters?

When did they receive copies?

How much advance notice did they get?

To what extent were their opinions heeded or incorporated?

Little of that is public, so most of the following just documents some relationships of “reviewers.” Some timelines and inputs are known.

NRC reviews are multidisciplinary to avoid the problem in which reviewers naturally tend to focus on their areas of expertise. When given a ~90-page report with much paleoclimate and SNA, most statisticians would naturally focus on the statistics, unless they happened to have specific experience in the other areas.²⁰ *I might expect most statisticians to agree that decentered PCs were wrong, Meme-g🔴. I would be amazed if anyone could spend the effort to redo MBH to know whether or not that made a significant difference.*

In a quick read, the WR looks plausible to many people. It uses the right words, has a long Bibliography, Summarizes papers, has correct math in Appendix A. It either takes strong paleoclimate domain expertise or some expertise plus tedious work to see the relentless pervasiveness of errors, omissions and biases. Even domain experts often skim the introductory material or Summaries, generally seeing expected words and not noticing

²⁰ I have encountered the same problem when running program committees. Most reviewers understood part of paper, thought it seemed fine, but fortunately one reviewer understood the other parts where problems lurked.

the accumulation of issues. How many people would get Bradley (1999) and learn about tree-rings?

Statisticians in particular are accustomed to encountering less-than-perfect statistics or confounding factors. Over-emphasizing familiar general problems can be quite effective, Meme-e🔴, Meme-g🔴.

Of course, it is unlikely that anyone receiving a high-profile report from distinguished statisticians would spend hours searching for plagiarism.

I would expect most named commenters to honestly say “I think you raise some real issues” regardless of any other suggestions or complaints. .

The WP asked associates, and at least some with little notice. Even the statisticians with clear climate-related interests (Braverman, Cressie, Wahba) did not necessarily have paleoclimate expertise. The WP simply asked statisticians they knew, and Wegman knew all of them.

Wegman was Director of the GMU Center for Computational Statistics 1986-2006, for which Scott was a long-time Corresponding Research Faculty member. Senior statisticians might be expected to have huge social networks²¹ with many kinds of relevant relationships, A.6.

The rest of this section goes through the WR list, in order as in A.1.4.

²¹ In the normal sense meant by the public, by SNA researchers and by text in the WR itself from [WAS1994], not by Wegman’s strange view that counted only coauthorship. Of course, one reviewer was a coauthor of Wegman and another was a coauthor of Said. Unfortunately, that question was not asked.

② **Professor (emeritus) Enders Robinson, geophysicist, Columbia University, elected member of the National Academy of Engineering**

web.archive.org/web/20080312161456/www.galaxy.gmu.edu/stats/faculty/robinson.html

Robinson has had a long career involving geophysics and statistics, especially seismic processing for oil exploration.²² He was/is a long-time Corresponding Research Faculty²³ member for Wegman's Center, from the earliest to latest list from the Internet Archive, 1997-2008:

web.archive.org/web/19971015125901/www.galaxy.gmu.edu/stats/center.d.escript.html#CFaculty 10/15/97

web.archive.org/web/20080112102417/www.galaxy.gmu.edu/stats/center.d.escript.html#CFaculty 04/19/08

[WEG2010] shows that Wegman and Chow wrote a chapter for a book edited by David Brillinger, Enders Robinson and Frederic Schoenberg.

Robinson's comments were read into the hearing record:

[BAR2006a, pp.31-32]:

“CHAIRMAN BARTON. I would like to submit for our record an e-mail that was received, and I would be more than willing to share it with the Minority if they have not seen it before. They have it? **It is an e-mail from Yasmin Said to Peter Spencer** and it says, "To whom it may concern: I have read the reports of Chairman Barton and Chairman Whitfield entitled "ad hoc committee report on the hockey stick global climate reconstruction by Edward J. Wegman, David Scott, and Yasmin H. Said" and what follows this work of Wegman, Scott, and Said is simply referred to as Report. The assessment of previous results given in the Report is correct. **The Report is entirely correct in stating that the most rudimentary additive model, the model of a simple temperature signal with superimposed noise, is not adequate to describe the complex relationships involved in climate dynamics.** There is no physical process found in nature that does not involve feedback in one form or another to regulate the action of the system. The statistical methods and models described in the report use more variables and make possible the construction

²² During 1992-2000 at Silicon Graphics, I made many visits to petroleum companies around the world and spent many hours with seismic analysts and reservoir modelers, hence have had some (modest) familiarity with this domain.

²³ I have no information on any benefits or responsibilities of such status, but quite a few people have been willing to be so listed.

of more elaborate reconstructions that allow feedback and interactions. **The report represents the correct way to proceed.** It is especially important to bring the professional statistical community into the picture in order to assure that a sound analytical foundation is secured in the continuing development of this program. Sincerely, **Enders A. Robinson**, member of the National Academy of the USA, fellow of the European Academy of Scientists, professor emeritus and the Maurice Ewing and J. Lamar Rozelle, Chair, Department of Earth and Environment, Columbia University."

[BAR2006a, p.36]

MR. STUPAK. **Did anyone outside your social network peer review your report?**

DR. WEGMAN. Yes.

MR. STUPAK. Who was that?

DR. WEGMAN. **Well, Enders Robinson."**

One might question whether this email is a "review" in any normal sense²⁴. The content also makes little sense, as it confuses models and methods. Reconstructions are not dynamic climate models. The statistics of time series and seismic analysis may be relevant, but Robinson's background shows no sign of interaction with climate research. Robinson was a long-time Wegman associate and had spent much of his career in or around the petroleum industry.²⁵

²⁴ The 13 reviewers for North [NRC20076, NOR2006] wrote 70 single-spaced pages, or 5+ apiece on average.

²⁵ Without unfairly generalizing from groups to individuals, as a group, petroleum geophysicists have not been eager to accept AGW science. This implies nothing about Robinson's beliefs, unknown to me. One might read

en.wikipedia.org/wiki/Scientific_opinion_on_climate_change

"No scientific body of national or international standing has maintained a **dissenting opinion**; the last was the **American Association of Petroleum Geologists**, which in 2007 updated its 1999 statement rejecting the likelihood of human influence on recent climate with its current non-committal position."

en.wikipedia.org/wiki/American_Association_of_Petroleum_Geologists

In 2006, AAPG controversially gave a journalism award to Michael Crichton, for *State of Fear*, and many objected. Robinson was more involved with the Society of Exploration Geophysicists, apparently more willing to accept AGW science, but memberships overlap. I personally know or have met petroleum geophysicists and executives with no trouble accepting AGW, so this is no attempt to generalize too much.

- **3 Professor Grace Wahba, statistics, University of Wisconsin, Madison, elected member of the National Academy of Science**
www.stat.wisc.edu/~wahba

Prof. Wahba is not only a member of NAS for statistics, but is well-respected by climate scientists and has interacted with them over many years. She and her students have published some climate-relevant papers. Her well-cited *Spline Models for Observational Data* (1990), p.xv says:

“For many years my research in splines was supported by the Office of Naval Research while Ed Wegman was Director of Probability and Statistics ... While managing a large program, Ed himself made some important contributions to the development of splines in statistics while prodding me on.”

She was well-qualified to review climate statistics.

But, Gerald North [NOR2006, 22:15] paraphrases 2006 email:

“What about their referee job? They claimed it was refereed but in fact they just sent it out to some friends right at the last minute. And in fact, one of them that it was sent to, **Grace Wahba**, who some of you may know at Wisconsin, she sent me an email and she says: **Hey they used my name and they said I was a referee. He sent it to me about 3 days beforehand and I sent him a bunch of criticisms which they didn't take into account.**”

- **3 Professor Noel Cressie, spatial statistics, Ohio State University**
www.stat.osu.edu/~ncressie

Prof. Cressie is another very well-qualified statistician, with a vast publication record, whose interests include spatial and environmental statistics, i.e., quite relevant. He attended the NCAR workshop of **A.4** and Mann's **A.1.3** testimony mentions him favorably. I would certainly expect such senior statisticians as he and Wegman to have crossed paths professionally, as at Interface and ACAS, **A.6**. For an NRC review, people are asked early to be reviewers and commit to respond via known schedules. It takes rather longer for busy people to review a 91-page report.

A.5 integrates into one chronology public dates, date-stamps from the WR PDF file and email from Cressie, with his feedback to Wegman, kindly posted for me by DC:

deepclimate.files.wordpress.com/2010/08/cressie-email-2006-07-18-with-attachment.pdf

It is exactly what one might expect a busy statistician to say. I would paraphrase the key points as:
I concur with the MBH decentering issue, so compute it the “right” way.²⁶

However, on 07/11/06, Cressie had asked for “another week” and sent his feedback 07/18/06. The WR had been finalized 07/12/06, so Cressie's feedback had zero effect.

- **Professor David Banks, statistics, Duke University, Editor of Applications Section, J. of the American Statistical Association**
www.stat.duke.edu/~banks/banksvitae.pdf

Banks was Editor of book for which Wegman wrote [WEG2010]

110. “Parallel coordinate and parallel coordinate density plots,”
Encyclopedia of Statistical Sciences, Update Volume 2, (Kotz, S., Read, C. B., and Banks, D. L., eds.), 518- 525+color plates, 1998

He was on the NAS CATS committee, 2003-2006, chaired by Wegman. **A.6** shows some participation history of the US Army Conference on Applied Statistics, a fairly small conference that has long involved Wegman, Scott, and Banks, as well as Said and other Wegman students.

Banks and Said were recent coauthors when the WR was finalized:

David L. Banks, Yasmin H. Said, “Data Mining in Electronic Commerce, Statistical Science, May 2006, Vol. 21, No. 2, 234-246.
projecteuclid.org/DPubS/Repository/1.0/Disseminate?view=body&id=pdfview_1&handle=euclid.ss/1154979824

Banks and Said later wrote an article for an encyclopedia whose Editors included Banks and Fritz Scheuren, another WR reviewer.

11. Banks, D., and Said, Y. (2007). “New Issues in Human Rights Statistics,” in *Statistical Methods for Human Rights*, ed. by J. Asher, D. Banks, and F. Scheuren, Springer: New York, pp. 227–240. on *Statistical Methods for Human Rights*.

Wegman and Banks were long associated in various ways, Banks and Said were very recent coauthors, but Wegman claimed Banks was not part of his social network, Theme-N^o.

²⁶ This had already been done, in effect by Wahl, Amman (2006).

A.1.3 showed Wegman had in 2006 promised a peer-reviewed paper to appear in the Section of JASA edited by Banks, although this seems not to have happened. In passing, DC discusses a possible recent connection between McKittrick and Banks, who is now a Senior Editor of a new journal, *Statistics, Politics, and Policy*:

deepclimate.org/2010/04/05/mcclimategate-continued-mckittrick-wrong-on-ipcc

www.bepress.com/spp/

www.bepress.com/spp/vol1/iss1/1 McKittrick article.

Professor William Wieczorek, geophysics, Buffalo State SUNY

www.buffalostate.edu/centers/CHSR/wieczorek.shtml

sphhp.buffalo.edu/spm/faculty/wieczorek_william.php

His webpage says:

“Dr. William F. Wieczorek is the Director of the Center for Health and Social Research as well as a Professor of Geography and Planning. ... Dr.

Wieczorek’s research interests include the epidemiology of substance abuse, prevention of drunk driving, evaluation of educational programs, community health needs assessments, advanced GIS applications, spatial models, and applied social/health research. His teaching interests include the epidemiology of alcohol use, applied geography, physical geography, and medical geography.”

It is difficult to understand how that is called “geophysics.”

He was a member of Said’s Dissertation Committee [SAI2005, p.1].

In 2007, he was involved in Interface, **A.6.3**. In 2008, he coauthored with Wegman and Said, from [WEG2010]:

“193. “Spatial and computational models of alcohol use and problems,” with William F. Wieczorek and Yasmin H. Said, in *COMPSTAT 2008*, (Paula Brito, ed.), 191-202, 2008”

Wegman’s claim that Wieczorek was outside his social network seems wrong, Theme-N⁴, although the earliest evidence of connection is 2004-2005.

③ Dr. Amy Braverman, Senior Scientist²⁷, remote sensing, datamining, Jet Propulsion Laboratory (CalTech)

www-misr.jpl.nasa.gov/about/team/braverman.html

She definitely has climate-relevant experience, as seen from:

journals.ametsoc.org/doi/abs/10.1175/BAMS-85-10-1491

Wegman’s C.V. [WEG12010] says:

She and Wegman coauthored in 2002:

“150. “New applications of the image grand tour,” with Jürgen Symanzik, Amy Braverman, and Qiang Luo, *Computing Science and Statistics*, 34, 500-512, 2002”

They were program co-chairs in 2002:

“Program Chair, Interface 2002, Montreal, Quebec, Canada with Amy Braverman”

They co-edited proceedings in 2003:

“15. *Computing Science and Statistics: Proceedings of the 33rd and 34th Symposia on the Interface* (issued as a CD), edited with Amy Braverman, Arnold Goodman and Padhraic Smyth, Fairfax Station, VA: Interface Foundation of North America, 2003”

She spoke at GMU 01/25/06:

A Probabilistic Approach to Mining Massive Earth Science Data Sets

www.galaxy.gmu.edu/stats/colloquia/ColloquiaSpring2006.html

But [BAR2006a, p.37] testimony in context in **A.1.3**:

“DR. WEGMAN. None of these people have actively collaborated with me in writing research papers.”

Perhaps writing one paper (or an abstract for a presentation) does not count as writing research papers, but a few weeks later [SHA2006, Figure 3] showed Braverman as a Wegman coauthor. She was also, 2004-2008, President of Wegman’s IFNA, **A.6.2** and thus was often involved in Interface, **A.6.3**. To label her in 2006 as outside his network seems *very strange*, Theme-N⁴. *Again, no negative connotation whatsoever can be applied to Braverman for this misrepresentation. It is very likely that she was another person asked to comment and surprised to be called reviewer.*

²⁷ This is quoted from Wegman, but is incorrect, she was a Senior Statistician.

Dr. Fritz Scheuren, statistics, NORC, University of Chicago, the 100th president of the American Statistical Association

www.norc.org/aboutus/norcexpe/scheuren.htm

www.google.com/url?sa=t&source=web&cd=1&ved=0CBUQFjAA&url=http%3A%2F%2Fwww.norc.org%2Faboutus%2Fnorcexpe%2Fscheuren.htm&ei=K8FPTKLgNo2msQPP9MiTBw&usq=AFQjCNEGqb6gOgP9tDZ69IC1QQ77VWVBQ

Scheuren is a distinguished statistician, who also spent many years around Washington, DC. He focuses on statistics in the social sciences, human rights, voting issues. Distinguished statisticians active in societies would know each other, but more specifically, Wegman was President of the Washington Statistical Society 1990-1991, during which time Scheuren was the President-Elect. Societies commonly elect a President-Elect who works closely with the current President for a year, then takes over.

scs.gmu.edu/~wss/02book.pdf

It seems odd that Schuereen would be labeled outside Wegman's network.

Summary

Of the 7 reviewers whose names are known:

- All are well-published, distinguished or even eminent.
- Braverman, Cressie and Wahba have clear experience with at least some areas of climate and interaction with climate scientists.
- Banks, Robinson, Scheuren and Wiczorek show no obvious connection with climate, but most have clear recent connections with Wegman and/or Said.
- Commenters were not listed in the WR, only later, and at least several were surprised.

In questioning Wegman, none of the Representatives happened to ask about coauthorship with other WP members or even better asked the general social networks question: "Can you explain the length and nature of the relationship of each reviewer to anyone on the WP?" They seemed misled by the framing, but in any case, Braverman was a Wegman coauthor, Banks was a Said coauthor. All the commenters would normally be considered members of Wegman's (large) social network, although with widely varying degrees of closeness and current connection.

This was not a serious review in any normal sense. *That is not the fault of the commenters. Most seem busy people asked on short notice to review a long report covering fields outside their expertise, sometimes far outside.*

Wegman tried to describe this to Congress as being "like the NRC,"

Theme-N⁴, whereas commenters were:

- entirely chosen by the authors
- not anonymous, but not listed in the WR either, as their identities were only supplied in response to questioning
- all represented as "outside Wegman's network," although only via strange definitions. Again, that is not the commenters' problem, nothing is wrong with sharing a network with Wegman. The problem is Wegman's misrepresentation to Congress.
- in some cases asked with adequate time, in other cases not
- apparently not asked early in the process, in at least 2 cases
- and whose feedback was not incorporated, in at least 2 cases.

In **A.1.3**, Wegman's own testimony strongly implies that his own review of the WR was not exactly thorough, consistent with the numerous errors.

The reader should evaluate this carefully. A range of opinions is possible:

- A good-faith effort was made to obtain actual reviews, evaluate their input carefully and make changes as needed. OR
- *This process was intended to provide a list of impressive names who could be claimed as "reviewers," even when their (sometimes hurried) comments arrived after the WR was finalized or strong comments ignored. In some cases they may have only discovered themselves labeled "reviewers" later, even by accident.*
- A similar pattern of over-promoting people's roles is seen elsewhere: Scott was labeled 2nd author, despite having just written Appendix A. Braverman and D. Brillinger were later described as having contributed to the WR, **A.3**. Any of these might have been surprises. People might not even notice the attributions, some of which were obscure, but even if they did, it could be awkward to make a fuss.

A.2 GW, but never AGW

The WR and Wegman testimony accepted post-1850AD Global Warming (GW), but managed to evade ever admitting that much is due to humans (AGW). This is strange, given how often it appears in references and Important Papers, Meme-h¹, Theme-B⁴, Theme-C⁴, Theme-N⁴. Wegman obviously did not even understand the simplest physics of the Greenhouse Effect or else took great pains to ever avoid admitting it. People with technical PhDs have to work very hard to “not know, not be sure” about basic physics accepted by every relevant science society and taught these days in high school. WR p.27 writes off the Greenhouse Effect as falling afoul of the “correlation is not causation” fallacy, **W.3**, and AGW repeatedly gets written out of Summaries.

[BAR2006a, pp.40-41]

MS. SCHAKOWSKY. Thank you, Mr. Chairman. I have so many things I want to ask here. Let me start again. Dr. North, I want to confirm what I think you already said. Is Dr. Mann's hockey stick study considered to be the foundation on which all climate change science is based?

DR. NORTH. No.

MS. SCHAKOWSKY. It isn't. And again I want to say, if it never were, if the study simply--the hockey stick, the original and there was a revised in 2003-2004, right, my understanding is, which I guess you disagree, **Dr. Wegman, acknowledged some of the mistakes and made some changes but if it never did, would most scientists essentially arrive at the same conclusion as we are seeing, that we are engaged--that this is a time of global warming attributable in large part to human activity?**

DR. NORTH. Yes, I think that is true.

DR. WEGMAN. By the way, for what it is worth, I think it is true although I would caution you to not say most scientists. **Most climate scientists would probably--**

DR. NORTH. **That is better.** Thank you. I appreciate that.

MS. SCHAKOWSKY. **Okay, most climate scientists. Should we not rely on climate scientists for our information about the climate?**

DR. WEGMAN. The point I was making was that you are saying most scientists, **so the testimony--**

MS. SCHAKOWSKY. Well, let me ask--

DR. WEGMAN. **--of a chemist is irrelevant to--**

MS. SCHAKOWSKY. Exactly. **So would you agree then that climate scientists are those that we should primarily refer to when we are asking questions about climate?**

DR. WEGMAN. **Certainly.**

MS. SCHAKOWSKY. **So you would agree that human activities are not only increasing atmosphere greenhouse gases but that it is attribute would you say in large part mostly in terms of your understanding as not a climate scientist to human activity?**

DR. WEGMAN. **I am in no position to say--**

MS. SCHAKOWSKY. Well, what did you say you did agree with earlier?

DR. WEGMAN. **I said I agree that it is warming. That is what I agreed to. I mean,** I said it several times now that the temperature record from 1850 onwards indicate that it is warming.

MS. SCHAKOWSKY. I also had said earlier that in my question to Dr. North and that most scientists agree that in large part or for your purposes I will say in some part attributable to human activity. **Would you agree with that?**

DR. WEGMAN. **I don't know that for a fact.**

MS. SCHAKOWSKY. Okay. **You don't know that.**

DR. WEGMAN. **Again, it is the connection between carbon dioxide and temperature increase. Now, Mr. Inslee pointed out that he thinks there is a physical explanation based on a blanket of carbon dioxide in the reflection. Carbon dioxide is heavier than air. Where it sits in the atmospheric profile, I don't know. I am not an atmospheric scientist to know that but presumably if the atmospheric--if the carbon dioxide is close to the surface of the Earth, it is not reflecting a lot of infrared back.**

On the next page, Wegman tries to claim he has long understood atmospheric mixing [WEG2006c, p.1], Theme-C⁴. Thankfully, CO₂ is well-mixed (or we might have breathing problems at sea-level). Of course, CO₂ does not “reflect” infrared, as it is not a mirror, but absorbs and emits.

MS. SCHAKOWSKY. Okay. **But are you not really qualified to--**

DR. WEGMAN. No, of course not.

MS. SCHAKOWSKY. --comment on that. I think since we are talking about scientific data, statistics, let us be clear, and you are challenging a report which form what I understand as Dr. North in some part at least you agree with the critique of the Mann data, so--and I am certainly--I am neither, but we are policymakers here so what I--do you believe that your report disproves that climate change is manmade in any way?

DR. WEGMAN. No.

MS. SCHAKOWSKY. And since you think that you are not in a position to make a decision on global warming, are you uncomfortable at all, Dr.

Wegman, that the consequences of what you are saying today to policymakers, I think most of whom, if not all of them, are neither statisticians or climate scientists, could have the impact of saying we don't need to do anything. Does that make you uncomfortable at all?

DR. WEGMAN. **I would hope that our legislators are smarter than that to know that when somebody says that they are using wrong methodology, that does not imply that some fact is not true.** I would hope that you would take my testimony with the idea that if something is wrong with this piece of work, it ought to be discarded as a policy tool, and that is precisely what I am saying.

MS. SCHAKOWSKY. Well, let me ask you this. Dr. Mann has published dozens of study since the original hockey stick study and as I said earlier, beginning in 2003 he reformulated the statistical methods. Do you take into account these later studies in your report?

DR. WEGMAN. **I have read his later studies. I was not asked about his later studies.** I think as science iterates, things do get better, but as I indicated before, one of the unfortunate aspects of this overall situation with Dr. Mann and his colleagues, my attack is not an attack at all. It is simply trying to lay out what I perceive to be a true statement. **I think it is unfortunate that rather than moving on and saying gosh, I made a mistake and here is the better situation, here is a better approach, there continues to be a defense which is captured in his web log called realclimate.org.”**

Meme-h🔴, Theme-N🔵.

[WEG2006c, p.1] Wegman:

“Although some individuals, including individuals writing editorials in the popular matters, I am not. For example, **I have known about mixing of gases in the atmosphere since my high school days¹. But I was asked to testify as a statistician as to the correctness of the Mann-Bradley-Hughes (MBH) methodology and not to offer my beliefs and opinions on anthropogenic global warming (AGW). For this reason, during my oral testimony, I refused to become drawn into the debate about AGW.**

¹The honorable Mr. Waxman addressed a question to Dr. Mann concerning an offhand remark I made about carbon dioxide being heavier than air. **My remark was in response to graphic displayed in the first hearing by the honorable Mr. Inslee showing infrared radiation being reflected by the greenhouse gasses in the upper atmosphere.** My response was not intended as a serious piece of testimony nor intended to represent my state of knowledge of atmospheric mixing.

[WEG2006c, p.6] Wegman:

“Dr. Douglas Nychka at NCAR, Professor Peter Bloomfield at North Carolina State University, and Professor Grace Wahba at the University of Wisconsin, Madison are in my view mainstream statisticians with a demonstrated interest and collaboration in the atmospheric sciences.”

Wegman surely knew **Nychka** from frequent attendance at Interface, **A.6.3**. He would have been an excellent choice as expert reviewer, as would have Wahba, had she gotten more than a few days.²⁸

As Judith Curry writes in defending Wegman, 04/25/10: www.collide-a-scape.com/2010/04/23/an-inconvenient-provocateur/#comment-3198

“Let me say that this is one of the most reprehensible attacks on a reputable scientist that I have seen, and the so-called tsunami of accusations made in regards to climategate are nothing in compared to the attack on Wegman.

Wegman is very unpopular with the warmists because his 2006 NRC report was very critical of the statistics used by mann et al. in the creation of the hockey stick. Prior to summer 2006, Wegman had no apparent interest or involvement in climate science or politics.

He was asked to chair this effort by the NRC since he was Chair of NRCs Committee on Applied Statistics. **When asked to explain the greenhouse effect, he really didn’t know anything about the physics of how it worked. So I don’t think you could have gotten a more unbiased person to do this review.** To see such a respected academic accused in this way (with the accusations so obviously baseless) is absolutely reprehensible.”

As discussed elsewhere, The WR had zero connection with the NRC, although many efforts were made to give that impression. *Wegman does everything possible to avoid admitting the strength of evidence for AGW or knowing anything about the basic physics. Whenever pressed, he retreats to “I was not asked to do anything but look at MBH98/99,”* Meme-h🔴, Theme-A🔵, Theme-B🔵, Theme-H🔵, Theme-N🔵.

Wegman downplays realclimate.org as a defensive website, somehow ignoring the long series of attacks from McIntyre’s ClimateAudit. Of course, this is not surprising.

²⁸ Of course, Nychka was on North’s NRC Panel, **A.1.1**, and as seen in [TEB2005, LI2007] possibly might not have agreed with the WR.

A.3 Time to Move On?

[BAR2006, p.1]

“Questions about the reliability of the Mann studies were of interest because they raised **policy-relevant questions** concerning the objectivity of the IPCC and its reliance upon and “promotional” use of the studies’ ‘hockey stick’ shaped historical temperature reconstruction.”

[BAR2006a, p.134] offers Wegman testimony, echoing WR, p.66, **W.10**:

“We do agree with Dr. Mann on one key point: that MBH98/99 were not the only evidence of global warming. As we said in our report, **“In a real sense the paleoclimate results of MBH98/99 are essentially irrelevant to the consensus on climate change.** The instrumented temperature record since 1850 clearly indicates an increase in temperature.” We certainly agree that modern global warming is real. We have never disputed this point. **We think it is time to put the ‘hockey stick’ controversy behind us and move on. I would like to make it clear that our role as statisticians in the hockey stick game is not as players in the hockey game, but as referees.”**

If MBH98/99 were irrelevant, why did they spend so much effort to discredit it, **Meme-ho?** The only policy-relevant part of the hockey stick is the “blade” not in doubt and not the reconstructed “shaft,” **§1.5.**

The wording is careful: GW is real, AGW is not mentioned. This issue is always avoided, even under repeated questioning, **Theme-Ho, A.2.**

Time to move on?

I have frequently quoted the first part of this as evidence that Wegman thought it was indeed time to move on. This never seemed to convince anyone who wanted to argue about MBH98/99 forever. In any case, I was wrong. Wegman himself certainly did not “move on.”

From his 2010 C.V. [WEG2010], I excerpt and annotate relevant talks and papers, including those just before the WR:

Pre-WR, for context:

“123. “Statistics, Data Mining, and Climate Change,” Keynote Talk, Second NASA Datamining Workshop: Issues and Applications in Earth Science, Pasadena, CA, May, 2006” (context, since that occurred before the WR.)

galaxy.gmu.edu/Interface2006/i2006webpage.html click on NASA.

This was scheduled in conjunction with the entry, but was just a talk in the Opening Session, not exactly a Keynote. Strangely, all presentations provide on-line slides or papers, except this one.

“124. “Statistics, Data Mining, and Climate Change,” Keynote Talk, Symposium on the Interface, Pasadena, CA, May, 2006
galaxy.gmu.edu/Interface2006/i2006webpage.html

Wegman was involved in 3 other sessions, but I could find no trace of this. Usama Fayyad gave the Keynote.

127. “The Kyoto Accord, The 2001 IPCC Third Assessment Report and The Academic Papers Underpinning Them,” Joint Statistics Meeting, Seattle, WA, August, 2006.

www.amstat.org/meetings/jsm/2006 (slightly mis-cited)

www.amstat.org/meetings/jsm/2006/PDFs/JSM06AbstractBook.pdf

“The Kyoto Accord focused on reducing greenhouse gasses and was supported by the report of the Intergovernmental Panel on Climate Change, “2001 Third Assessment Report.” This report featured the “hockey stick” millennium temperature reconstruction based on a number of proxy variables. **The academic papers that developed the reconstruction used a principal components analysis, which has been challenged by critics due to inappropriate use of PCA and a lack of independent verification of the findings.** We discuss the methodology used, the use of potentially problematic data, and the social network of authors in temperature reconstruction. The implications of global warming are important for the financial and human dimensions, and public policy decisions must be made on a statistically sound, uncontroversial basis. **D. Scott, D. Brillinger, Y. Said, J. Rigsby, D. Reeves, and A. Braverman** contributed to this report.”²⁹

The late addition of Brillinger and Braverman seems strange. Of course, they may have made a few comments, and that was enough to list them, although “contributed” usually means much more than that.

Richard L. Smith writes a 3-page summary of that session, of which a few parts are excerpted here, but the reader might study the entire summary: lv-twk.oekosys.tu-berlin.de/project/lv-twk/images/pdfs/ENVR_9_1.pdf (p.2-4)

²⁹ Given people surprised to be labeled “reviewers,” it is unclear who actually did what. *Someone may well have been thanked for making a few comments, possibly even ignored.*

“And in 2006, several statisticians were involved in a well-publicized controversy over the so-called “hockey-stick curve”. This controversy was the central feature of a late-breaking session entitled “What is the Role of Statistics in Public Policy Debates about Climate Change?” **that was organized jointly by Edward Wegman (George Mason University) and myself** at the 2006 Joint Statistical Meetings. The session took place in front of a standing-room-only audience and was chaired by Doug Nychka (National Center for Atmospheric Research).

The three speakers were Ed Wegman, J. Michael Wallace of the Department of Atmospheric Sciences, University of Washington, and myself. Ed and Mike both talked about the hockey stick reconstruction. **Ed focused on statistical flaws that, in his view, render much of the current literature on this subject of doubtful validity.** Mike presented the broader findings of a recent NRC panel that, while acknowledging the statistical issues of Wegman’s report, defended the hockey stick curve based on a broader scientific context.

...

Concerning the “social network” aspects of the Wegman report, the NRC’s written report did not directly address that issue but concluded that this field of research is moving forward in a healthy manner. **No doubt social networks exist in the climate research field but there is no evidence that these result in publication bias in this field of research any more than they do in any other field. As for the alleged lack of involvement by statisticians in paleoclimate research, Mike noted the following points:** that there was a long history of statisticians being involved in this and other areas of climate research; that the Wegman report underestimated the degree of statistical expertise that already exists in this community; and that while there is undoubtedly scope for statisticians to play a larger role in paleoclimate research, the large investment of time needed to become familiar with the scientific background is likely to deter most statisticians from entering this field.

In the end, it’s important not to lose sight of the forest for the trees, where the “forest” refers to the totality of scientific evidence for global warming. ...

To conclude this report, I will mention two follow-up activities. First, we plan a special issue of *Statistical Science*, including specially invited papers from Professors Wegman and Wallace and a number of discussants. This will allow more detailed airing of the issues behind the Wegman report and how both statisticians and climate scientists view them. Second, there will be an ASA workshop of invited participants ...”

Statistical Science is:

www.imstat.org/sts

Wegman mentioned this in **A1.4**. However, I could find no trace of any such special issue. Certainly, no relevant articles by Wegman or Wallace are found from 2006 onward. The ASA workshop did occur, **A.4**. Despite the commentary on SNA by Wallace, [SAI2008] was written anyway, perhaps as justification Wegman’s talk can be called Meme-f^o, resisted by Wallace.

132. “The Hockey Stick Controversy: Lessons for Statisticians,” Army Conference on Applied Statistics, Research Triangle Park, NC, October, 2006
Wegman’s IFNA had taken over management of that conference, **A.6.3**.
www.armyconference.org/ACAS06/default.htm

Unfortunately the Proceedings are not on-line.

135. “Reanalysis of the Hockey Stick Paleoclimate Reconstruction,” Public Lecture, Distinguished Visiting Professor at the American University of Cairo, Cairo, Egypt, March, 2007
www1.aucegypt.edu/academic/math/events.html

This talk was for the Department of Mathematics and Actuarial Science:

“A Reanalysis of the “Hockey Stick” Paleoclimate Reconstruction
Edward J. Wegman, Yasmin H. Said, and David W. Scott

Abstract: One of the most interesting visual graphic to emerge in the last decade was the so-called “Hockey Stick” paleoclimate reconstruction. The papers published in 1998 and 1999 by Mann et al. used tree ring data and other temperature proxies to estimate the temperature over a 1000 year period beginning approximately 1000 C.E. Their reconstruction showed a gradual decrease in temperature from 1000 to approximately 1850 and then a rapid rise. The conclusion of their paper was that the decade of the 1990s was probably the hottest decade in a millennium and that 1998 was probably the hottest year in a millennium. **The graphic was exploited by the Intergovernmental Panel on Climate Change (IPCC) Third Assessment Report published in 2001 and was widely used to support assertions of anthropogenic climate change.** Their methodology incorrectly used a principal component-like analysis. **When the PCA methodology is correctly used, the hockey stick essentially disappears (i.e. the rapid rise from 1850 disappears.** The change in mean global temperature is approximate .7 degrees centigrade over 150 years. **We discuss this and other statistical faults with the paleoclimate**

reconstructions. The analysis of these three authors was presented as testimony in July 2006 before the United States Congress. “*How likely was the presence of climate expertise at that talk?* Meme-f●.

141. “20 Questions a Statistician Should Ask about Climate Change,” ASA Workshop on Climate Change, NCAR, Boulder, CO, October, 2007. [WEG2007]. (also given as talk at GMU, 11/05/07) www.galaxy.gmu.edu/stats/colloquia/AbstractsFall2007/CollNov5.html This talk was the only post-WR talk given to climate experts, and it has serious problems, **A.4**, Meme-f●.

He then gave this talk at GMU, 11/05/07, for which the abstract was: www.galaxy.gmu.edu/stats/colloquia/AbstractsFall2007/CollNov5.html

147. “Paleoclimate Temperature Reconstructions: Implications for Climate Change,” American Public Health Association Annual Meeting, San Diego, CA, October, 2008 www.apha.org/membergroups/newsletters/sectionnewsletters/statis/spring08/default.htm#{71E0836B-9E6A-4BF8-B32E-2CFAE9622732}

I could not find an abstract, but the session was interesting:

”**Statistics and Climate Change: Is There a Consensus?**
(Monday, Oct. 27, 2:30 – 4:00 p.m.)

Organizer: William Pan, DrPH, Department of International Health, Johns Hopkins Bloomberg School of Public Health

A. Paleoclimate Temperature Reconstructions: Implications for Climate Change, Edward Wegman, PhD, Department of Computational and Data Sciences, George Mason University

B. Climate Past and Climate Future, Douglas Nychka, PhD, Institute for Mathematics Applied to Geosciences, National Center for Atmospheric Research

C. Climate Extremes and Global Warming: A Statistician’s Perspective, Richard Smith, Department of Statistics and Operations Research, University of North Carolina, Chapel Hill”

This sounds like the same talk, recycled yet again, and for an audience unlikely to have much climate expertise. If Wegman had learned anything from [NRC2006] or other interactions, it is not apparent, Meme-f●.

178. “Implications of co-author networks on peer review,” with Yasmin H. Said, Walid K. Sharabati, John T. Rigsby in *Classification and Data Analysis*, Macerata, Italy: EUMEdizioni Università di Macerata, 245-248, 2007
This seems likely a talk related to next paper.

179. “Style of author-coauthor social networks,” with Yasmin H. Said, Walid K. Sharabati, John T. Rigsby, *Computational Statistics and Data Analysis*, 52, 2177-2184, 2008; doi:10.1016/j.csda.2007.07.021, 2007.
This is [SAI2008], apparently with title mis-cited. This was accepted July 2007, in 6 days, **W.5.6**, Meme-b●.

Wegman thus gave (generally unrefereed) talks, of which the only one with many climate scientists in the audience would have been 141, at NCAR, **A.4**. The social networks papers were not published in SNA-focused journals such as *Social Networks*. The 3 papers claimed to be in progress in **A.1.4** (or described by Richard Smith) apparently never happened. ***In fact, despite all these talks from 2006 onward, Wegman has yet to produce a credibly peer-reviewed paper on this topic.***

It would be interesting to see the other talks and papers and especially any mentions of research contracts supporting these efforts, **A.7**.

DC found the strangest connection, an Amazon entry for: Edward Wegman, *Controversy in Global Warming: A Case Study in Statistics*, December 21, 2007. www.amazon.com/Controversy-Global-Warming-Study-Statistics/dp/0470147849/ref=sr_1_14?ie=UTF8&s=books&qid=1279606110&sr=8-14

“**Product Description**

Commissioned by the House Committee on Energy and Commerce and the House Subcommittee on Oversight and Investigation, the authors of the book detail time lines, findings, and **interpretations that helped shape the misconception behind the effects of global warming as we know it today. Fraught with the potential for damaging political innuendo and inappropriate social networking overtones**, the authors steer clear of passing personal judgments in favor of outlining the accepted controversies surrounding the topic, this for historical and reconstructive purposes. **The authors’ report to Congress is included in its entirety as an appendix at the rear of the book.”**

Product Details

Paperback: 288 pages
Publisher: Wiley-Interscience (December 21, 2007)
Language: English
ISBN-10: 0470147849
ISBN-13: 978-0470147849”

The reference to “authors” might be a typo, or perhaps this was the book mentioned by Yasmin Said in [SAI2007, p.24]:

“Book
• By Wiley – The Heated Debate – under contract.”

Perhaps she dropped out. Amazon provides no reviews and labels the book “not yet available.” Various bookselling search engines have offered sets of bookseller websites have labeled I (concurrently, to some amusement) “In stock – ship in 2-3 days,” “out of stock” or “not yet printed.”

For at least 2 years following Wegman’s testimony that “it was time to move on,” Wegman and his students kept giving talks and papers derived from this work. In at least one case [SAI2008], Federal research contracts were cited in support, in some cases for work that seems likely to have been done as part of the WP effort, repeatedly labeled as *pro bono*, **A.7.**

Wegman was listed on the BALI2007 petition [MAS2010]:

deepclimate.org/2009/12/10/bali-2007-revisited

www.nationalpost.com/news/story.html?id=164002 open letter

www.nationalpost.com/news/story.html?id=164004 signatures

“It is not possible to stop climate change, a natural phenomenon that has affected humanity through the ages. . . . In particular, it is not established that it is possible to significantly alter global climate through cuts in human greenhouse gas emissions. Consistent with this, and despite computer projections of temperature rises, there has been no net global warming since 1998.”

The petition claims many Memes. [SOL2008] featured Wegman. The 1998 date was a cherry-pick and was not even true if one does a proper regression, which any statistician would do.

After this burst of effort in 2006-2007 ([SAI2008] was accepted July 2007), it seemed that their interests had gone in other directions. I had assumed that perhaps they had realized this was a not a fruitful area for them and they really had moved on.

But, that was a premature conclusion. Wegman and Said were PC Co-Chairs for Interface 2010, and organized two sessions *truly astonishing* to find at a credible statistics conference, **A.6.4.** They invited Fred Singer (SEPP), Jeff Kueter (GMI), and Don Easterbrook (Western Washington State University). Said talked on Climategate, lambasting climate researchers for their bad behavior.

This does not sound like “TIME TO MOVE ON.”

A.4 Wegman at NCAR, October 2007

“20 Questions a Statistician Should Ask about Climate Change,” ASA Workshop on Climate Change, NCAR, Boulder, CO, October 26-27, 2007, [WEG2007].

He then spoke at GMU, 11/05/07, for which the abstract was:

www.galaxy.gmu.edu/stats/colloquia/AbstractsFall2007/CollNov5.html

“The American Statistical Association recently sponsored a Workshop: A Statistical Consensus on Global Warming held at the National Center for Atmospheric Research. The workshop was aimed at understanding what are the areas of agreement from a statistical perspective. In light of the issuance of the IPCC 2007 report this year and the general lack of success of the Kyoto Accord to stem greenhouse gas emissions, this workshop has an important role in developing the consensus on statistical issues. Although both paleoclimate reconstruction and climate modeling have many fundamentally statistical/stochastic issues, the convergence of the perspectives of statisticians and climate scientists is not great. This talk is not an anti-anthropogenic global warming talk, but will probably irritate climate scientists anyway. (It did at NCAR, but discussion is good.) In this talk I seek to raise some of the statistical issues related to inferences about climate change.”

The reader can study that NCAR workshop to see why Wegman’s talk might have irritated people. Meme-f

www.image.ucar.edu/Workshops/2007/ASAclimate/index.shtml

Participants:

www.image.ucar.edu/Workshops/2007/ASAclimate/Participants.shtml

They included both statisticians (listed here) and climate scientists:

www.stat.duke.edu/~berger	Jim Berger
www.stat.osu.edu/people/faculty/mark-berliner	Mark Berliner, A.6.4
www.stat.ncsu.edu/people/bloomfield	Peter Bloomfield, A.1.1
www.stat.osu.edu/people/faculty/noel-cressie	Noel Cressie, A.1.5
research.hopkinsglobalhealth.org/GlobalFacultyPage.cfm?global_faculty_id=876	Francesca Dominici
www.stat.washington.edu/peter	Peter Guttorp
galton.uchicago.edu/~stein	Michael Stein, A.12

Claudia Tebaldi Rand Corporation (no CV found, but many papers)

Yasmin Said, Ed Wegman

- All (but Said) are long-experienced statisticians, an impressive group.
- All (but Wegman and Said) had published (at least several, but usually more) credible peer-reviewed papers on environmental or climate sciences and participated in relevant activities.

The Agenda and the talks, with actual links

www.image.ucar.edu/Workshops/2007/ASAclimate/agenda.html

www.image.ucar.edu/public/Workshops/ASAclimate/markerASA.pdf

1 www.image.ucar.edu/public/Workshops/ASAclimate/trenberthASA.htm

2 www.image.ucar.edu/public/Workshops/ASAclimate/santerASA.htm

3 www.image.ucar.edu/public/Workshops/ASAclimate/bergerASA.pdf

4 www.image.ucar.edu/public/Workshops/ASAclimate/allenASA.htm

5 www.image.ucar.edu/public/Workshops/ASAclimate/dominiciASA.htm

6 www.image.ucar.edu/public/Workshops/ASAclimate/wegmanASA.htm

7 www.image.ucar.edu/public/Workshops/ASAclimate/zwiersASA.htm

Sessions (1, 2, 4, 7) were presented by climate scientists, (3, 5, 6) by statisticians. All Writing Leaders were statisticians.

The talks generally offered straightforward discussions of relevant science, statistical techniques, sources and nature of uncertainties, specific areas where better statistical techniques would help. (Statistician) Jim Berger’s “Statistical Issues Involving (Climate) Computer Models” *seemed a sophisticated, realistic talk on the issues*, including Slide 19:

“How can statisticians become involved?”

The Key: Becoming involved in a ‘team environment’ with scientists.

Facilitating infrastructure:

- NCAR, where teams operate
- SAMSI (and NPCDS), where teams can be formed
- National labs (both LANL and LLNL have climate/stat teams)
- Large interdisciplinary grants available today

Barriers:

- Statistics cannot generally fund involvement of statisticians in other disciplines which, in turn, rarely have much money for statistics.
- Shortage of statisticians
- **The time needed for a statistician to get deeply involved with another science and to also learn the statistics needed for it.**
- **Scientists often have a hard time judging what they can do themselves and when they should seek statistical help.”**

The last comments are akin to the issues of generalists-vs-specialists mentioned in **W.5.2**. Statisticians must learn enough science to be useful, and scientists need to know when to ask for statistical help, if they can get some, which may not always be possible. See related discussion [MAS2010, A.10.4]. The same issues appear in other fields.

Overall, this seemed like a high-quality, interesting workshop of world-class people looking for ways to make improvements. Wegman's talk is given to experts at an invited workshop on statistics and climate science: www.image.ucar.edu/public/Workshops/ASAclimate/wegmanASA.htm

Slide 2: 3 films

The Day After Tomorrow (Earth freezes),
Waterworld (Earth covered by water),
An Inconvenient Truth.

Slide 3: 3 magazines:

? "Global warming – what will the outcome be?"
 Time: "Be worried. Be very worried."
 Vanity Fair: "Green Issue"

Slide 4: 5 book covers

S. Fred Singer: *Hot Talk, Cold Science*
 Michael Crichton: *State of Fear*
 Patrick Michaels, James Balling: *The Satanic Gases*
 S. Fred Singer, Dennis Avery: *Unstoppable Global Warming*
 Patrick J. Michaels, *Meltdown*

No other talk here resembles this. All but one of the latter authors are long-time climate anti-science professionals discussed.³⁰ Singer books would have been especially irritating to this audience.³¹ These slides might make sense as part of a framing discussion for a general audience, but not for experts.³²

Slide 8:

This uses a table from Bradley (1999) (originally Bradley, Eddy (1991)), but adds note "Confounding factors," Meme-e¹. Berger's talk was clear: statisticians have to understand enough relevant science to be useful. Knowledgeable audiences are unlikely to be impressed by insertion of such labels, although it may impress others.

³⁰ Crichton is a deceased fiction author, but his book belongs with this set.

³¹ Singer (in particular) had long been attacking workshop participant Ben Santer (and others) via extra-science routes, as discussed in [MAS2010], possibly to the point of defamation. Wegman and Said invited Singer, later, A.6.4.

³² Just as the WR used Bradley (1999) material, but with selective editing, Wegman slides 2-4 were taken from Mann's September 2007 talk, although with possibly-interesting omissions. See last page of this section.

The following summarizes his 21 questions, with more a few more detailed comments later. Most of his claims or concerns had been answered in testimony by North or Crowley in [BAR2006a].

Slides 14-29: Statistical Questions, Terse Summary, *'d discussed later
Question

1. Studied in dendocronology, Meme-e¹
2. Studied in dendocronology, Meme-e¹
3. Studied in dendocronology, Meme-e¹
4. Studied in dendocronology, Meme-e¹
Slide 15 repeats decentering slide from WR Figure 4.3
5. Studied in ice cores, Meme-e¹
6. Meme-11^e, serious field ignorance.
7. Meme-e¹
8. Plausible statistics to ask, but studied, Meme Meme-e¹
9. Plausible statistics to ask, but they had already moved from PCA.
Why is he still talking about MBH98/99 in 2007, to experts?
10. Plausible statistics to ask
11. Plausible statistics to ask, but studied
12. Plausible to ask, but well-studied, and why people do ensembles
13. Well-studied
14. Misunderstanding. Weather = initial value, climate = boundary value
15. This question confuses models with measurements and seems not to understand the use of emissions scenarios.
16. CO₂ correlates with temperature.
The link is fairly direct, basic physics Theme-C¹.
17. Reasonable question. Trenberth's talk addressed it, as have others.
18. Meaningless questions. All data are valuable, but worth the cost??
 People collect data from meaningful sites, using domain science.
 People do not look for random ice cores or tree-rings in the Sahara.
 Petroleum people drill where they think there is oil, not randomly.
19. Very well studied by NASA-GISS and others, Meme-20²
20. Very well studied by NASA GISS and others
 19, 20 seem to come from Anthony Watts, wattsupwiththat.com
www.ncdc.noaa.gov/oa/about/response-v2.pdf
21. Climate scientists do such comparisons rather often

Slides 16-17: “Paleoclimate Reconstruction”


Slide 16: “Lonnie Thompson’s Ice Cores and Nobel Laureate Al Gore”

Slide 17:

“5. Similar questions exist about ice cores and how representative such data might be. What are the effects of gas diffusion in the ice core layers?”


Did Wegman actually have the expertise to argue about ice core diffusion issues? This issue has been studied heavily.

“6. In the ice core (Vostok) data that Al Gore illustrates in the *Inconvenient Truth*, the temperature time series leads, not lags the CO₂ time series by 800 to 1000 years. It would seem that temperature increases cause CO₂ release, not vice versa. The common answer is that there is an (unspecified) feedback mechanism.”

This attack on AIT was very popular among the unknowledgeable. The ice age termination effect of CO₂-lagging-temperature was predicted around 1990 in a famous paper by Lorius, et al, and repeatedly confirmed as ice core records lengthened, Meme-11 .

Slide 20:

“7. Why are the same proxies used in so many papers? Most recently, Osborn and Briffa (2006), D’Arrigo, et al (2006), Juckes, et al (2007). Mann et al (1998, 1999) was ground breaking in using hundreds of proxies. Most more recent papers focus on the top 15 or so in our two-mode network. Are they chosen because they show the “hockey stick” and those omitted don’t?”

They are re-used because they are thought to be high-quality proxies with useful geographic coverage, reanalyzed just as statisticians re-use important data, Meme-c . This is WR Figure 5.8, **W.5.8**. Following are the two mentioned above shown in the chart, showing total proxies, # in the top 20 (~”15 or so”), i.e., 3-12 uses. I also add the counts from the 44 single-use proxies that the WR omitted.


Name	Total	3-12 uses	2 uses	single use	2+1/total
OB06	14	11	2	1	3/14
DWJ06	19	8	1	10	11/19

In “Are they chosen” Wegman seems to insinuate that paleoclimate scientists cherry-pick their data. In doing so, Wegman:


- Uses a chart that omits the 50% of proxies used only once, those that would argue against his claim.
- Mentions Juckes, not yet published when that chart done.
- Makes a sweeping assertion (“most”) with which DWJ06 disagrees as 11/19 of its proxies were not among the top 20.

“Most” seems to mean 1 (perhaps 2) of 3. I guess 2 of 3 is “most.” Had Wegman continued to follow the literature in detail? A search for “Juckes” at McIntyre’s website yielded ~60 posts from September 2006 through October 24, 2007, including for example:




climateaudit.org/2006/12/18/the-independent-2006-multiproxy-studies

McIntyre thus mentioned Juckes about once a week, on average, Link-M .

Slide 30:

“Where to send climate police.” Meme-f .

The other talks are experts soberly talking to experts.

Wegman’s talk has a few reasonable questions, but many just show ignorance of the state of climate science, Theme-N . If given to an audience unfamiliar with the topic, which Wegman did later, these easily raise doubts, Meme-g . Many of the questions are answerable by reading the IPCC AR4 or a few peer-reviewed papers. Wegman speaks as though experts have never noticed these issues, when some have spent years wrestling with them, Meme-e .

Slide 20 has a clear, misleading impression. How many people are likely able to notice the omission of important data in a minute or two?

This seems like an expert-sounding form of the “Gish Gallop”:

en.wikipedia.org/wiki/Duane_Gish

It is unsurprising that the climate scientists may have been irritated. *Perhaps some of the fine statisticians there were not thrilled either.*

Following are slides from Michael E. Mann, “The Science of Climate Change,” general-audience seminar at Penn State, 09/17/07.

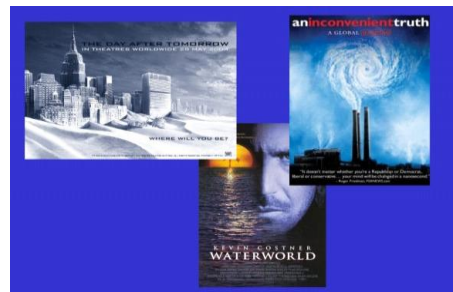
www.meteo.psu.edu/~mann/Mann/lectures/ScienceOfClimateChange07.ppt

This usage is unacknowledged, but is not a copyright issue. Mann used this to describe public framing versus actual science. Given to a general audience, the two slide sets tell very different stories. The choice of the 3 slides from the 6-slide sequence is interesting.

First 7 slides from Mann talk, general; audiences, 09/17/2007

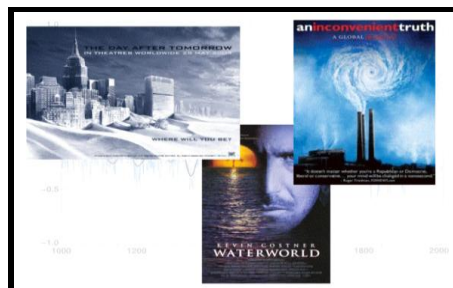
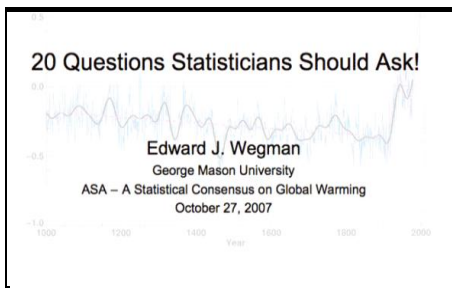
The Science of Climate Change

- Public framing of the issue
- Basic principles
- Observational evidence
- Comparing theory with observations
- Projections of future climate change
- Impacts
- Solutions



Too small to read here, Newsweek says:
 * Or so claim well-funded naysayers who still reject the overwhelming evidence of climate change. *Inside the denial machine*. By Sharon Begley.

First 4 slides from Wegman talk, expert workshop, 10/27/07



A.5 Integrated chronology and related notes

Asterisked (*) dates are from email I got from Cressie, posted at: deepclimate.files.wordpress.com/2010/08/noel-cressie-wegman-timeline.pdf

See [MAS2010] for surrounding background, new names.

- 05/04/05 McK05 presented in Australia
- 05/11/05 MM05x presented at GMI
- 06/23/05 Barton/Whitfield letters to MBH
- 07/14/05 Boehlert letter to Barton, “strenuous objections”
- 07/15/05 NAS President Cicerone offers NRC panel
Rejected as “unlikely” to address all of Mr. Barton’s concerns
- 07/17/05 Ebell (CEI): “We always wanted to get science on trial”
- 09/01/05 Wegman approached by Coffey, agrees to form team
- ??/??/05 Wegman recruits at least Said, maybe others
- ??/??/05 Wegman, Said first meeting with Spencer
- 11/02/05 (Approximately), Boehlert→Cicerone→North, NRC
deepclimate.files.wordpress.com/2010/07/boehlert_2005.pdf
- 11/14/05- US Climate Change Workshop [MAS2010, §5.3]
- 11/16/05 Wegman, Said, Spencer, McIntyre attend. So do others:
Singer, Kueter (GMI) Both spoke at Interface 2010, **A.6.4.**
Mark Herlong (GMI), Horner (CEI).
- 03/01/06 NAS Panel
MM (both), Spencer, Singer, Ebell, Soon, Christy, others.
- 06/22/06* Wegman sends draft to Cressie (*Version A?*)
- 06/23/06* Cressie replies, saying comments “soon after July 4.”
- 07/11/06* Cressie emails Wegman, to say “I need another week.”
- 07/11/06 12:54PM WR PDF is created (*Version B?*)
- 07/11/06* Wegman sends updated version to Cressie (likely *Version B*)
- 07/12/06 5:51PM WR PDF finalized (*Version C?*), *Sent to??*
- 07/13/06 (no later) sent to WSJ, because:
- 07/14/06 WSJ Editorial “Hockey Stick Hokum” [WSJ2006]
- 07/14/06 10AM Barton, Whitfield announce WR [BAR2006]
- 07/14/06 10AM Whitfield announces 07/19/06 10AM hearing
- 07/17/06* Cressie sends email to Wegman, promising comments 07/18

07/18/06* Cressie sends comments to Wegman & Said, 10:30AM³³

07/19/06 1st hearing testimony, [BAR2006a, pp.3-101]

07/27/06 2nd hearing testimony, [BAR2006a, pp.101-171]

??/??/06 (no later than 08/02), [WEG2006c]

On Friday 07/14/06, it was announced in the WSJ, sent out on Newswire. *Apparently that indirect notice was the first notification to Mann that he should appear in Washington, DC on Wednesday 07/19/06, i.e., with 2.5 business days notice.* After 10 months’ effort by the WP, the importance of paleoclimate and tree-rings suddenly escalated, requiring the House to scramble to collect a day-long session, inviting whoever they could get to come within a few days.

It is good to know that the US Government can move very swiftly to deal with urgent problems, like Hurricane Katrina the next month.

[BAR2006a, p.6], July 19, 2006

“PREPARED STATEMENT OF THE HON. ED WHITFIELD, CHAIRMAN, SUBCOMMITTEE ON OVERSIGHT AND INVESTIGATIONS

I’d like to welcome Mr. Steven McIntyre. Mr. McIntyre will testify about attempting to understand just what was behind the hockey stick graphic promoted by the IPCC. His examination of the facts underlying the assessments’ claims really initiated some of the important questions concerning the scrutiny provided by climate change assessments. His work is a testament to the value of open debate and scrutiny. His perseverance should be commended. **Let me add that we did invite Dr. Mann to this hearing, but his attorney explained that he was unavailable, on family vacation.**”

McIntyre is retired *and possibly has a flexible schedule, and perhaps knew about this earlier than 07/14.* Mann has a job, a family and perhaps less flexibility to travel on 2.5 business days’ notice.

All this is good tactics if one wants to “wrong-foot” people.

³³ “I sent my comments to Wegman (and copied Said) at 10.30am. I can’t find any correspondance after that.” Note that Scott is not mentioned..

[**BAR2006a**, p.14-15]

“MR. STUPAK. Mr. Chairman, before we do that, if I may, I would like to put into the record a letter from Georgetown University Law Center Institute for Public Representation explaining why Dr. Mann cannot be here on such short notice from the committee and other dates he was available to testify. I would like to put that in the record, a follow-up of the statements that he is on vacation, which is not true. ...

MR. STEARNS. Oh, sure, sure. Yes. You put a letter that came after the first letter and I thought it would be appropriate if we include that letter too since that is a day earlier in which he said he could not make our committee and for whatever reason he couldn't make it and in fact he suggested that if we do have this hearing, that we should have Dr. Thomas J. Crowley, and indeed we took his advice and we got Dr. Crowley. He is going to be on the second panel, so we took Dr. Mann's advice, we got the people he wanted, and I am sure, **Mr. Chairman, other people had to cut their vacation short to be here, perhaps even Dr. North did.**

So, I mean, for anybody on the other side to say this not a legitimate hearing is incorrect. We have taken people that Dr. Mann wanted and we put them on here as witnesses. We have asked Dr. Mann to come to this hearing. We have asked him to come to the 27th. He won't come. He has hired a lawyer to spar with our people to say why he won't come. **By golly, if he really is interested in solving this problem, I would cut my vacation short and whatever he is doing to say I will be here because I think in the interest of science,** I would like to have an open hearing and talk about it. So I think, one, it is a legitimate hearing. **Two, we have offered Dr. Mann two opportunities and yet his lawyer has indicated he won't show up.”**

MR STEARNS is Cliff Stearns (R-FL), who perhaps belongs on the list of helpers of Barton and Whitfield, i.e., part of CO.

[**BAR2006a**, p.117] Mann:

“I am sorry I could not be here last week but as I had explained to committee staff, I had to take care of my infant daughter while my wife was attending a conference.”

A.6 Wegman social network, a few subnets

Wegman described his social network as the people with whom he had coauthored papers, which he suggested guessed as about 15, **W.5.4**, while criticizing Mann for having 40+. Shortly thereafter, Wegman reported his count as 101, [WEG2006c], **A.5.4**. The WR itself lists examples of multiple relationship types in real social networks, **W.5.1**. Wegman is an energetic, experienced, high-ranking academic with wide interests. For example, every single WR reviewer was clearly a member of Wegman's real network, although some were much closer than others.

The WR, [WEG2006c, SHA2006, SAI2008] attacked Mann (1-year post-PhD in 1999) claiming he then had a powerful network, but then tried to use Wegman's network and relationship with students as a comparison to claim impropriety in paleoclimate. Since they opened that issue, it seems fair to study Wegman's (extensive) real network.

A.6.1 Students and co-authors

Wegman's C.V. [WEG2010 shows the multiplicity of relationships expected of an experienced high-ranking academic, but most must be ignored for now in favor of students and coauthors. In later tables, these students, plus occasional MS students are underlined. The Dissertation Director- PhD student relationship is often very strong, sometimes lifelong. Most Professors list students in their own C.V.s.

Coauthors are extracted from [but only from the "PAPERS" category], which included 201 numbered (*n*) papers. Each coauthorship is assigned a code *n.m*, *m* the coauthor number within that paper, yielding 288 instances. The authors are then sorted alphabetically, with a few adjustments made for spelling or marriage name change. A few names may be mis-cited or cited differently but are left unchanged.

This roughly matches the studies in [SHA2006, SAI2008] and is an impressive record that covers many topics. This study also shows dates and frequencies of coauthorship, although in SNA terminology, it is still limited to being a Wegman-centered "egonet" that omits multi-hop connections. *No negative connotation is implied by anyone's presence here, but compare the nature of the networks here to those of paleoclimate, claimed to have poor review with zero evidence. However, review problems around Wegman might be seen, A.1.5, A.5.6, A.5.7. The 4*

Year Wegman PhD Students

1973	<u>Davies, H. Ian</u>
1974	<u>Gould, Jerren</u>
1989	<u>Le, Hung Tri</u>
1993	<u>Ball, Celesta G.</u>
1993	<u>Hearne, Leonard B.</u>
1993	<u>Morad, Osama</u>
1993	<u>Priebe, Carey E.*</u>
1994	<u>Chow, Winston*</u>
1994	<u>Sullivan, Mark C.</u>
1995	<u>Akuiuobi, Cajetan M.</u>
1995	<u>Martinez (Poston), Wendy L.*</u>
1995	<u>Solka, Jeffrey L.*</u>
1996	<u>Faxon, Don R.</u>
1996	<u>Marchette, David J.*</u>
1996	<u>Wei, Shumei</u>
1997	<u>Li, Shan-Chuan</u>
1998	<u>Ahn, Sung</u>
1999	<u>Vandesluis, J. Patrick</u>
2000	<u>Khumbah, Kkem-Ami "Martin"</u>
2001	<u>Champaneri, Amrut</u>
2001	<u>Moustafa, Rida E. A.*</u>
2002	<u>Martinez, Angel R.*</u>
2004	<u>Reyen, Salem S.*</u>
2004	<u>Sikali, Emmanuel</u>
2005	<u>Alotaiby, Fahad</u>
2005	<u>Caudle, Kyle Allman</u>
2005	<u>Noh, Eun Young</u>
2005	<u>Said, Yasmin H.*</u>
2006	<u>Alshameri, Faleh Jassem</u>
2007	<u>Sharafi, Homayoun</u>
2008	<u>Belayneh, Sirak</u>
2008	<u>Hohman, Elizabeth Leeds*</u>
2008	<u>Sharabati, Walid K.*</u>
2009	<u>Reeves, Denise M.</u>
2009	<u>Rezazad, Hadi*</u>

<i>n.m</i>	Year	Coauthors from Wegman C.V.	<i>n.m</i>	Year	Coauthors from Wegman C.V.	<i>n.m</i>	Year	Coauthors from Wegman C.V.	<i>n.m</i>	Year	Coauthors from Wegman C.V.
166.2	2004	Adams, M.L.	23	1979	<u>Davies, H. Ian</u>	31.4	1982	Kibblewhite, E.J.	156	2003	<u>Marchette, David J.</u>
123	1998	<u>Ahn, Sung</u>	31.2	1982	Deepak, A.	67	1990	King, R. Duane	157.2	2004	<u>Marchette, David J.</u>
182.1	2007	Alnoshan, Abdullah	134.2	1999	DeJong, Ken	107.2	1997	King, R. Duane	158	2004	<u>Marchette, David J.</u>
167.1	2004	<u>Alotaiby, Fahad</u>	145.1	2001	Dorfman, Alan H.	164.2	2004	King, R. Duane	162.02	2004	<u>Marchette, David J.</u>
171.1	2005	<u>Alotaiby, Fahad</u>	154	2003	Dorfman, Alan H.	128.07	1999	Kumbah, N-A.	168.1	2005	<u>Marchette, David J.</u>
100	1997	<u>Ball, Celesta G.</u>	176.1	2006	Dorfman, Alan H.	69	1990	<u>Le, Hung Tri</u>	95.2	1992	<u>Martinez (Poston), Wendy L.</u>
31.1	1982	Baylis, P.	128.05	1999	Dzubay, A.	71	1991	<u>Le, Hung Tri</u>	90.2	1995	<u>Martinez (Poston), Wendy L.</u>
164.4	2004	Bernard, Steve	164.1	2004	<u>Faxon, Don R.</u>	95.1	1992	<u>Le, Hung Tri</u>	92.1	1996	<u>Martinez (Poston), Wendy L.</u>
63	1988	Bolorforoush, Masood	31.3	1982	Francis, C.R.	95.4	1992	<u>Le, Hung Tri</u>	93.1	1996	<u>Martinez (Poston), Wendy L.</u>
150.2	2002	Braverman, Amy	128.06	1999	Fu, X.	80	1993	<u>Le, Hung Tri</u>	94.2	1996	<u>Martinez (Poston), Wendy L.</u>
170.2	2005	Bryant, Avory C.	132.2	1999	Fu, X.	145.3	2001	Leaver, Sylvia G.	96.2	1996	<u>Martinez (Poston), Wendy L.</u>
190.2	2008	Bryant, Avory C.	53	1987	Gantz, Donald T.	176.3	2006	Leaver, Sylvia G.	97.1	1996	<u>Martinez (Poston), Wendy L.</u>
128.04	1999	Camelli, F.	20	1977	Glaser, Anton	145.2	2001	Lent, Janice	104.2	1997	<u>Martinez (Poston), Wendy L.</u>
81.1	1993	Carr, Daniel B.	8	1972	Gore, Brewster	176.2	2006	Lent, Janice	107.4	1997	<u>Martinez (Poston), Wendy L.</u>
82	1993	Carr, Daniel B.	17	1976	<u>Gould, J.</u>	105	1997	<u>Li, Shan-Chuan</u>	108.3	1997	<u>Martinez (Poston), Wendy L.</u>
107.1	1997	Carr, Daniel B.	24	1980	<u>Gould, J.</u>	188.1	2008	Lin, Chien-Chih	109.1	1997	<u>Martinez (Poston), Wendy L.</u>
19	1977	Carroll, Raymond J.	162.08	2004	Guglielmi, R.	162.1	2004	Lin, D.	111.1	1998	<u>Martinez (Poston), Wendy L.</u>
175	2006	<u>Caudle, Kyle Allman</u>	78	1992	Habib, M.K.	81.2	1993	Luo, Qiang	114.1	1998	<u>Martinez (Poston), Wendy L.</u>
196	2009	<u>Caudle, Kyle Allman</u>	56	1988	Hayes, Annie	101	1997	Luo, Qiang	118.3	1998	<u>Martinez (Poston), Wendy L.</u>
125.1	1998	Chen, J. X.	162.11	2004	Healey, D.M.	112.1	1998	Luo, Qiang	121.1	1998	<u>Martinez (Poston), Wendy L.</u>
126.2	1998	Chen, J. X.	76	1991	<u>Hearne, Leonard B.</u>	128.03	1999	Luo, Qiang	124.3	1998	<u>Martinez (Poston), Wendy L.</u>
132.1	1999	Chen, J.X.	79	1992	<u>Hearne, Leonard B.</u>	146	2002	Luo, Qiang	120.1	1999	<u>Martinez (Poston), Wendy L.</u>
143.1	2000	Chen, J.X.	87	1994	<u>Hearne, Leonard B.</u>	149	2002	Luo, Qiang	137	2000	<u>Martinez (Poston), Wendy L.</u>
112.2	1998	Chen, Jim X.	114.2	1998	Holland, O. Thomas	150.3	2002	Luo, Qiang	163.2	2004	<u>Martinez (Poston), Wendy L.</u>
167.2	2004	Chen, Jim X.	162.12	2004	Jacob, M.Q.	94.3	1996	<u>Marchette, David J.</u>	181	2007	<u>Martinez (Poston), Wendy L.</u>
171.2	2005	Chen, Jim X.	160.1	2004	Johannsen, D.A.	103.2	1997	<u>Marchette, David J.</u>	189.1	2008	<u>Martinez (Poston), Wendy L.</u>
159	2004	<u>Chow, Winston</u>	85	1994	Jones, C.A	106	1997	<u>Marchette, David J.</u>	151	2002	<u>Martinez, Angel R.</u>
162.07	2004	Church, K.W.	161	2004	Kafadar, Karen	113	1998	<u>Marchette, David J.</u>	152	2003	<u>Martinez, Angel R.</u>
162.09	2004	Coifman, R.R.	173	2006	Kafadar, Karen	115.2	1998	<u>Marchette, David J.</u>	163.1	2004	<u>Martinez, Angel R.</u>
26.2	1980	Cukuk, Chris	162.06	2004	Karakos, D.	116.2	1998	<u>Marchette, David J.</u>	189.2	2008	<u>Martinez, Angel R.</u>
14	1975	<u>Davies, H. Ian</u>	155	2003	<u>Khumbah, Kkem-Ami "Martin"</u>	117.2	1998	<u>Marchette, David J.</u>	185.1	2007	Mburu, Peter

<i>n.m</i>	Year	Coauthors from Wegman C.V.	<i>n.m</i>	Year	Coauthors from Wegman C.V.	<i>n.m</i>	Year	Coauthors from Wegman C.V.	<i>n.m</i>	Year	Coauthors from Wegman C.V.
42	1985	McDonald, G.C.	182.2	2007	Rotenstreich, Shmuel	92.2	1996	<u>Solka, Jeffrey L.</u>	119.2	1998	Symanzik, Juergen
50	1987	Miller, John J.	174	2006	<u>Said, Yasmin H.</u>	93.2	1996	<u>Solka, Jeffrey L.</u>	127.2	1999	Symanzik, Juergen
74.2	1991	Miller, John J.	178.1	2007	<u>Said, Yasmin H.</u>	94.1	1996	<u>Solka, Jeffrey L.</u>	128.01	1999	Symanzik, Juergen
107.3	1997	Miller, John J.	179.1	2007	<u>Said, Yasmin H.</u>	96.1	1996	<u>Solka, Jeffrey L.</u>	144	2001	Symanzik, Juergen
191.2	2008	Miller, John J.	180	2007	<u>Said, Yasmin H.</u>	97.2	1996	<u>Solka, Jeffrey L.</u>	147	2002	Symanzik, Juergen
115.1	1998	Minnote, Michael C.	182.3	2007	<u>Said, Yasmin H.</u>	103.3	1997	<u>Solka, Jeffrey L.</u>	150.1	2002	Symanzik, Juergen
117.1	1998	Minnote, Michael C.	183.2	2007	<u>Said, Yasmin H.</u>	104.1	1997	<u>Solka, Jeffrey L.</u>	86.1	1994	Takacs, Barnabas
128.08	1999	<u>Moustafa, Rida E. A.</u>	184	2007	<u>Said, Yasmin H.</u>	107.5	1997	<u>Solka, Jeffrey L.</u>	99.1	1997	Takacs, Barnabas
130	1999	<u>Moustafa, Rida E. A.</u>	185.2	2007	<u>Said, Yasmin H.</u>	108.1	1997	<u>Solka, Jeffrey L.</u>	162.13	2004	Tsao, A.
134.1	1999	<u>Moustafa, Rida E. A.</u>	186	2007	<u>Said, Yasmin H.</u>	109.3	1997	<u>Solka, Jeffrey L.</u>	128.02	1999	<u>Vandesluis, J. Patrick</u>
141	2000	<u>Moustafa, Rida E. A.</u>	187	2007	<u>Said, Yasmin H.</u>	111.2	1998	<u>Solka, Jeffrey L.</u>	128.09	1999	Wall, R.
172	2006	<u>Moustafa, Rida E.A.</u>	192	2008	<u>Said, Yasmin H.</u>	118.1	1998	<u>Solka, Jeffrey L.</u>	102.2	1997	Wallet, Bradley C.
188.2	2008	<u>Noh, Eun Young</u>	193.2	2008	<u>Said, Yasmin H.</u>	121.2	1998	<u>Solka, Jeffrey L.</u>	103.1	1997	Wallet, Bradley C.
22	1978	Nour, El-Sayed	194	2008	<u>Said, Yasmin H.</u>	124.1	1998	<u>Solka, Jeffrey L.</u>	104.3	1997	Wallet, Bradley C.
26.1	1980	Nour, El-Sayed	195	2008	<u>Said, Yasmin H.</u>	120.2	1999	<u>Solka, Jeffrey L.</u>	116.1	1998	Wallet, Bradley C.
162.03	2004	Park, Y.	197	2009	<u>Said, Yasmin H.</u>	133	1999	<u>Solka, Jeffrey L.</u>	107.6	1997	Wallin, John
109.2	1997	<u>Priebe, Carey E.</u>	198.2	2009	<u>Said, Yasmin H.</u>	148	2002	<u>Solka, Jeffrey L.</u>	125.2	1998	Wang, J.
118.2	1998	<u>Priebe, Carey E.</u>	199	2009	<u>Said, Yasmin H.</u>	157.1	2004	<u>Solka, Jeffrey L.</u>	126.1	1998	Wang, J.
160.3	2004	<u>Priebe, Carey E.</u>	200.2	2010	<u>Said, Yasmin H.</u>	160.2	2004	<u>Solka, Jeffrey L.</u>	143.2	2000	Wang, J.
162.01	2004	<u>Priebe, Carey E.</u>	201.2	2010	<u>Said, Yasmin H.</u>	162.04	2004	<u>Solka, Jeffrey L.</u>	86.2	1994	Wechsler, Harry
168.2	2005	<u>Priebe, Carey E.</u>	178.2	2007	<u>Sharabiti, Walid K.</u>	166.1	2004	<u>Solka, Jeffrey L.</u>	99.2	1997	Wechsler, Harry
182.4	2007	Rajput, Adil	179.2	2007	<u>Sharabiti, Walid K.</u>	169	2005	<u>Solka, Jeffrey L.</u>	167.3	2004	Wechsler, Harry
124.2	1998	Reid, L.	183.1	2007	<u>Sharabiti, Walid K.</u>	170.1	2005	<u>Solka, Jeffrey L.</u>	171.3	2005	Wechsler, Harry
191.1	2008	<u>Reyen, Salem S.</u>	198.1	2009	<u>Sharabiti, Walid K.</u>	190.1	2008	<u>Solka, Jeffrey L.</u>	193.1	2008	Wieczorek, William F.
164.3	2004	<u>Rigsby, John T. (MS 2005)</u>	200.1	2009	<u>Sharabiti, Walid K.</u>	41	1985	Solomon, H.	119.1	1998	Wilhelm, A.F.X.
178.3	2007	<u>Rigsby, John T.</u>	201.1	2010	<u>Sharabiti, Walid K.</u>	59	1988	Solomon, H.	127.1	1999	Wilhelm, A.F.X.
179.3	2007	<u>Rigsby, John T.</u>	83	1993	Shen, J.	167.4	2004	Sprague, Debra	25	1980	Wright, I.W.
21	1978	Robertson, Tim	61	1988	Shull, C.	171.4	2005	Sprague, Debra	39	1983	Wright, I.W.
108.2	1997	Rogers, G.L.	162.05	2004	Socolinsky, D.A.	88	1995	<u>Sullivan, Mark C.</u>	74.1	1991	Xu, Mingxian
118.4	1998	Rogers, G.W.	95.3	1992	<u>Solka, Jeffrey L.</u>	89	1995	<u>Sullivan, Mark C.</u>	188.3	2008	Yan, Younggping
102.1	1997	Rogers, George	90.1	1995	<u>Solka, Jeffrey L.</u>	98	1997	<u>Sullivan, Mark C.</u>	128.1	1999	Zhu, Y.

A.6.2 IFNA – Interface Foundation of NA

www.interfacesymposia.org says:

“Interface is a membership society of computational scientists, statisticians, mathematicians and individuals from related discipline areas interested in the interface between computing science and statistics. Interests include topics such as computational statistics, statistical software, exploratory data analysis, data mining, pattern recognition, scientific visualization and related fields. Interface holds an annual meeting. Members of Interface enjoy reduced rates at the annual meeting, a subscription to the Interface published *Computing Science and Statistics* and reduced subscription rates to several journals; the *Journal of Computational and Graphical Statistics* and *Computational Statistics and Data Analysis*.”³⁴

“The Interface Foundation of North America, Inc., in addition to being the sponsor of the Symposia on the Interface, has also taken responsibility for the Army Conference on Applied Statistics (ACAS). The details of recent and future programs of the Army Conference on Applied Statistics can be found at the ACAS Website.”

Interface was started in 1967 as an informal organization. Wegman was the founder of the current organization, and heavily involved since 1988:

www.interfacesymposia.org/interface/history.html

Societies run conferences whose organizers often vary greatly from year to year. Sometimes a small group of people organizes conferences, sometimes in cooperation with societies, government agencies or companies. Such groups often include some core people in rotating roles, with occasional additions and deletions. They can literally run 20 years or more with some of the same key people, whether they coauthor or not.³⁵

Recent history for IFNA can be found by searching:

foundationcenter.org/findfunders/990finder Interface Virginia

This sort of 501 (c)3 entity exists to run conferences from year-to-year, with enough funds to pay deposits for venues, do publicity, cover some travel expenses, etc. Some have participated in both conferences, others (like Braverman) seem to have been involved only with Interface conferences.

³⁴ *These conferences seem worthy and interesting interdisciplinary efforts.* I might have attended during the 1990s when working at Silicon Graphics, a 1997 sponsor.

³⁵ I’ve been involved in the yearly Hot Chips Conferences one way or another since 1989, as have a dozen or so others. While very different from Interface conferences in some ways, similarities are recognizable.

IFNA 990 Forms:	2004	2005	2006	2007	2008
BoD=Board Member	GA Tech	NPG	Duke	Rice U	VMI
Ex-Of = Ex Officio	Atlanta	Monterey	Durham	Houston	Lexington
,# = hours/week	GA	CA	NC	TX	VA
Interface	X	X	X	203	170
ARMY ACAS				78	72
Braverman, Amy	Pres,5	Pres,5	Pres,5	Pres,10	Pres,2
Hesterberg, Tim	Secty,5	Secty,5	Secty,5		
Izenman, Alan				PC,BoD,10	BoD,0
Kafadar, Karen				Chair,5	Chair,1
Karr, Alan				BoD,1	PC,BoD,10
<u>Marchette, David</u>				Secty,5	Secty,5
<u>Said, Yasmin</u>				Ex-Of BoD	BoD,
				Editor, 10	Editor,5
Scott, David W.			Chair,5	Board,1	Board,1
Shannon, William				BoD,1	BoD,1
Smyth, Padharic				BoD,1	BoD,0
Wegman, Edward	Treas,10	Treas,10	Treas,10	Treas,10	Treas,10

Wegman C.V.: President 1987-1993, Treasurer 1987-1996, 1999-present "Elected Chairman of the Board, Interface, 1997-2000"

Scott C.V. : Scott C.V. : IFNA: President 1998-2001

Wegman has been the long-term driver of IFNA, with others committing varying levels of effort. For example, each of the two Program Chairs (PCs) was busy when running a conference. IFNA took on ACAS organization and occasionally runs other conferences as well. Unlike huge conferences, these have relatively few (Interface) or no (ACAS) parallel sessions. People can hear most of the talks and interact often with others.

The following tables summarize involvement of some (small) subsets of people involved in ACAS and Interface, sandwiched between commentaries to keep the tables together. Many names reappear, as one would expect. *As elsewhere, no negative connotations are implied of anyone.* It just illustrates the absurdity that an influential senior member of a large network repeatedly accuse the paleoclimate community of misconduct merely by their association.

A.6.3 ACAS, Interface Conferences

DC mentioned Wegman's involvement with the US Army Conferences on Applied Statistics (ACAS) and possible interactions there with WR reviewer David Banks. That incited (tedious) rummaging through the various parts of the following websites:

www.armyconference.org Fall meetings summarized on next page

www.interfacesymposia.org Spring meetings, 2nd following page

A quick perusal of the ACAS website and Agendas from 1997 showed:

- *This generally seems a reasonable conference*, with many familiar names from year to year, a common occurrence in small conferences with continuity among organizers. Some people run several sessions and give several talks in various overlapping combinations. *Many topics seem relevant to Army or intelligence issues.*³⁶ *A few are not.*
- Scott and various Wegman students have long had a strong presence there. Wegman was involved every year from 1999, except 2007, when the ACAS conference was the week before the ASA climate workshop at NCAR, **A.4**. Other GMU people (not shown) attended.
- IFNA became a "Cooperating Institution" in 2003.
- Said replaced Wegman as Co-Editor (with Barry A. Bodt, ARL) in 2003, 2 years before her first ACAS paper "Adaptation of an Alcohol Ecological Agent-Based to Homeland Security."
- No negative connotations are implied by anyone's presence on these charts. The first merely shows that Wegman, Scott, Banks (and Bruce West, a new name here, possibly relevant) have likely known each other for years just through this (~80-person) conference alone.

But concerns may be raised regarding the two talks shown in grey on next page. Wegman, Said, Scott (given by Wegman, 2006, I1) and West (2008, C5) surely seem climate-related talks, delivered to an audience of people whose expertise in that area is unobvious, as climate is not generally an ACAS focus.³⁷

Wegman's views are clear in WR and related testimony in 2006, and seemed unchanging [SAI2008, WEG2007, signing BALI2007, INT2010].

³⁶ I claim no great expertise, but I have worked with or spoken at the NSA, CIA, Australian DSD, ARL, NSWC, UK GCHQ, etc, etc.

³⁷ Noel Cressie has spoken at ACAS, but not on climate.

Unfortunately, the later ACAS Proceedings are unavailable, so I have not located these talks. *However, it seems fair to speculate that perhaps:*

- *A highly respected statistician, well-known to the particular audience*
- *used an invited talk to deliver a climate anti-science talk to an audience unfamiliar with the issues and unlikely to challenge him*
- *in a conference paid for by the US Army.*

Climate anti-science talks often are given in non-peer-reviewed venues unlikely to face well-informed challenge. The reception of [WEG2007] by experts was not positive, even from Wegman's own later comments, **A.4**.

The second concern is Bruce West's talk. With Nicola Scafetta, he has striven for years to try to make AGW disappear into statistical noise, nonphysical cycles or especially Meme-01^e. Both signed the 2009 petition to the APS [MAS2009]. West was/is Chief Mathematical Scientist, US Army Research Laboratory, and Scafetta has gotten funding from ARL, as has Wegman, although without obvious connection. The Scafetta/West papers have not seemed to hold up well, but they seem to keep appearing.

www.24-7pressrelease.com/press-release/bruce-j-west-recognized-for-excellence-in-mathematics-107528.php

www.wired.com/dangerroom/2008/06/army-vs-global/

www.fel.duke.edu/~scafetta/pdf/opinion0308.pdf

This appears to be a description of the same talk, in 2009:

mechatronics.ece.usu.edu/foc/event/FOC_Day@USU/2009_files/Bruce-West_Talk090424-flyer.pdf

rabett.blogspot.com/2010/04/eli-can-retire-part-vi-going-where-sun.html

This is purely speculative, but it is possible that Wegman was exposed to West's views either via this conference or via Army contracts, A.7. Of course, many other reasons are possible and a more plausible origin might be Coffey [COF2009], given longer-term contact. Wegman was with Strategic Defense Initiative Organization 1985-1986 ("Star Wars") and might have encountered GMI founders., Perhaps repeated contact with West reinforced this, or perhaps those are just coincidence. The use of Coffey to contact Wegman argues against a then-strong GMI connection.

US Army Conference on Applied Statistics, Fall, typically October

<http://www.armyconference.org>

Year: Site:	Weg Studer	Weg Coaut	1997 GMU Fairfax VA	1998 NMSU Las Cruces NM	1999 USMA West Point NY	2000 Rice U Houston TX	2001 LANL Santa Fe NM	2002 NCSU Raleigh NC	2003 UC Davis Napa CA	2004 GA Tech Atlanta GA	2005 NPG Monterey CA	2006 Duke Durham NC	2007 Rice U Houston TX	2008 VMI Lexington VA	2009 ASU Tempe AZ	2010 SAS Cary NC
Banks, David (Duke)					I					I		O1		S		
Carr, Dan (GMU)		X	P							P						
Cressie, Noel (OSU)						I		S								
Kafadar, Karen (IU)		X					C	C,S	C	C,C2	C3	C	I	C	C	
<u>Marchette, David (NSWC)</u>	X	X						I,C		O	C3					
<u>Martinez, Angel R.</u>	X	X						C								
<u>Martinez, Wendy (Poston) (ONR)</u>	X	X								C1				O2		
<u>Priebe, Carey (JHU)</u>	X	X						C		O						
Rigsby, John T. (NSWC)	X	X						C				S2				
Said, Yasmin (GMU)	X	X							E	E	C7	I1			S4,O3, O5	
Scott, David (Rice)						C	K		C	A		I1	S	C		
<u>Solka, Jeff (NSWC)</u>	X	X				S		P,C		S1,C1				O2	S3	
Wegman, Ed (GMU)			P,C		A, K	E	E	P,E	O	S1,C2,O	C3,C4	I1,O1		I2	C6,O3,O5	O4
West, Bruce (ARL)									C			C	I	C5	C	

IMPORTANT CAVEAT. WHEN COAUTHORS ARE LISTED, IT IS NOT ALWAYS CLEAR WHO ACTUALLY DELIVERD THE TALK.

It would be very interesting to see these 2 talks. Did the US Army in effect pay for any of this?

O: Session Organizers
(or Chairs)

- O1 **Wegman**, Banks Session: Social Network Analysis
- O2 Solka, Martinez Session: *Mitigation of Technology Surprise*
- O3 **Wegman & Said** Session: *Text and Data Mining with Applications; has S3, S4*
- O4 **Wegman** Session: Social & Cultural Terrain
- O5 **Wegman & Said** Session: *Agent Based Modeling and Simulation*
- I: invited Speaker
 - I1** **Wegman** (GMU), **Said** (JHU), **Scott** (Rice), *The Hockey Stick Controversy: Lessons for Statisticians*. Note change of author order. This talk sounds like an early version of **[WEG2007]**.
 - I2 **Wegman**, *Text-mining and Social Networks: Some Unexpected Connections*
- S: Special Session speaker
 - S1 Solka, Bryant, **Wegman**, *Recursive Bipartite Spectral Clustering for Document Categorization*
 - S2 John E. Gray, John T. **Rigsby**, *The Need for a Tensor Theory of Social Networks*
 - S3 Solka, Tukey, Bryant: *Text Data Mining for Better Understanding of the Science and Technology Landscape, in O3*
 - S4 **Said**, *Approaches to Text Mining that Preserve Semantic Content, in O3*
- C: Contributed Speaker
 - C1 **Wegman**, Kafadar, *Visual Analytics for Streaming Internet Traffic*
 - C2 Martinez, Solka, *Research Directions in Adaptive Mixtures and Model-Based Clustering*
 - C3 Kafadar, Marchette, **Wegman**, *Graphical Displays and Methods for Analyzing Internet Traffic Data for Potential Cyberattacks*
 - C4 **Wegman**, Faleh Alshameri, *Automated Metadata*
 - C5** Bruce **West**, *Global Warming as a Problem in Statistical Physics*
With Nicola **Scafetta**, he has a history of trying to disprove AGW. He signed the 2009 Petition to the APS [**MAS2009, West** entry].
 - C6 **Wegman**, Rezazd, Shores, *Optimizing Computer Networks for Robustness and Efficiency*
 - C7 **Said**, *Adaptation of an Alcohol Ecological Agent-Based (sic) to Homeland Security*

A: Wilks Medal Award
K: keynote speaker
P: Presenter
E: (Co-)Editor of Proceedings

Interface Symposia, publishes Computing Science & Statistics (yearly proceedings),

<http://www.interfacesymposia.org>

Person	Year: Site:	Weg Student	Weg Coautho	1997 Rice Houston TX	1998 unk	1999 NIU Schaumburg IL	2000 New Orl LA	2001 Costa Ms CA	2002 Montrea CA	2003 SLC UT	2004 Baltimore MD	2005 WUSTL St.Louis MO	2006 JPL,GMU Pasadena CA	2007 Temple Philadelphia PA**	2008 Duke Durham NC	2009* WUSTL StLouis MO	2010 Seattle WA
Alshameri, Faleh (GMU)			X										X,N				
Banks, David (Duke)				X						X	X	PC	X		X	X	
Braverman, Amy (JPL)							X	E	E	E		X	PC, N	X	X		
Carr, Dan (GMU)			X	X		X	X		X	X	X						
Cressie, Noel (OSU)													N				
Easterbrook, Don (W WA St)																	I
Hesterberg, Tim (various)						X						X	X				X
Kafadar, Karen (IU)			X	X									X		X		
Kueter, Jeff (GMI)																	I
Marchette, David (NSWC)		X	X	X			X	X	X	X	E	X	X		X		X
Martinez, Angel R.		X	X						X			?					
Martinez, Wendy (Poston) (ONR)		X	X	X		X						?			X		
Moustafa, Rida (GMU)		X	X			X	X						X				X
North, Gerald (TAMU)									X								
Nychka, Doug (NCAR)				X		X	X		X				X				
Priebe, Carey (JHU)		X	X	X		X	X		X			X			X		
Rigsby, John T. (NSWC)		X	X							X	X	X	X				
Said, Yasmin (GMU)		X	X							E	E	X	PC	X	X		PC,X
Scott, David (Rice)				PC,X		X	X		X		X	X	X				X
Sharabati, Walid (GMU)		X	X												X		
Singer, Fred (SEPP)																	I
Solka, Jeff (NSWC)		X	X	X			X	X	X	X	E	X	X		X		X
Symanzik, Juergen (IA State)			X	X				X	X	X			X	X			
Wegman, Ed (GMU)				X		X	E	E	E	X	X	X	X,N	X	X	X	PC
Wieczorek, William (Buf State NY)			X											X			

IMPORTANT CAVEAT. WHEN COAUTHORS ARE LISTED, IT IS NOT ALWAYS CLEAR WHO ACTUALLY DELIVERD THE TALK.

PC: Program Chair

N: NASA Workshop

? Speaker only says "Martinez" and it could be either

E: Editor (usually 2 Co-Editors)

X: Speaker or other involvement

I Very unusual invited speaker

* Systems biology emphasis, atypical

*2009 with Classification Society, URL broken, but DC found some details elsewhere.

Interface Symposia seem interesting conferences with a much wider variety of papers, whose benefit is the interdisciplinary interaction, and whose weakness might be weak review, given the wide range of topics.

www.interfacesymposia.org

The 2009 link is broken, but DC finds:

www.classification-society.org/csna/CS_Newsletter_Apr_2009.pdf

“Four outstanding keynote addresses cover a wide range of topics:

-Symbolic Data Analysis by Lynne Billard, University of Georgia

-Document Clustering and Social Networks by Ed Wegman, George Mason University

-Quantitative Marketing by Erin Tanenbaum, Nielsen Claritas Company

-Statistical Issues in Agent Based Models by David Banks, Duke University”

www.classification-society.org/ifcs/ifcs37.pdf for more information

Wegman (naturally, given his role in IFNA) was involved every year for which data is available, often with multiple talks and sessions. Several of Wegman’s earlier students (Marchette, Priebe, and Solka) are often involved, unsurprisingly given their organizations. As with ACAS, Wegman’s newer students flow through, often coauthoring talks. This is business as usual in academe, as professors help their students.

The WP leapt into SNA, a discipline mostly new to them, **W.5**. ACAS saw a burst of SNA-related papers from Wegman’s students starting in 2006. Wegman and Said also started doing talks on climate and started claiming expertise in it, another new area. Wegman’s C.V. lists 2 keynote talks in May 2006, but the one at the NASA Workshop was not a keynote. The one at Interface seemed not to occur. The WR tried strongly to claim poor review practices in paleoclimate, merely by showing a coauthorship network. At least one SNA researcher was rather unimpressed, **W.5.6**.

Association is not guilt in either paleoclimate or statistics.

A.6.4 Interface 2010

This is [INT2010], June 16-19, 2010.

www.interfacesymposia.org/Interface2010 > Program Book OR:

www.interfacesymposia.org/Interface2010/ScheduleforInterface2010.pdf

The greyed boxes on the previous page show **invited** talks organized by Program Co-Chairs Said and Wegman, *delivered by people who are generally professional climate anti-science advocates*, [DEE2010m]. The sponsors and 24 Program Committee members are:

“Financial Sponsors:

ASA Section on Statistical Computing

ASA Section on Statistical Graphics

Salford Systems

Cooperating Organizations:

ASA, CSNA, ENAR, IASC, IMS, INFORMS, SIAM, WNAR”

“Interface 2010 Program Committee

Edward J. Wegman,

George Mason University, Program Co-Chair

Yasmin H. Said,

George Mason University Program Co-Chair

Georgiy Bobashev,

RTI International

Barry Bodt*,

Army Research Laborator

Hamparsum Bozdogan,

University of Tennessee

David van Dyk,

University of California, Irvine

Arnold Goodman,

Collaborative Data Solutions

Mark Handcock,

University of California, Los Angeles

Tim Hesterberg**,

Google

David J. Marchette,

Naval Surface Warfare Center, Dahlgren Division

Rida Moustafa,

dMining Technology, LLC

Rebecca Nugent,

Carnegie Mellon University

Adrian Raftery,

University of Washington

Stephan Sain,

National Center for Atmospheric Research

C. Shane Reese,

Brigham Young University

Michael Schimek,

Danube University, Austria

David Scott,

Rice University

Simon Sheather,

Texas A&M University

Jeffrey L. Solka,

Naval Surface Warfare Center, Dahlgren Division

William Szewczyk

National Security Agency

Michael Trosset,

Indiana University

Antony Unwin,

University of Augsburg, Germany

Roy Welsch,

Massachusetts Institute of Technology

Adalbert Wilhelm,

Jacobs University, Bremen”

* Bodt was long involved with ACAS, co-edited with Wegman.

** Hesterberg was IFNA Secretary, at least 2004-2006, **A.6.2**.

Distinguished academics know many others, so relationships are unsurprising. Nothing is improper with this PC, *but 2 sessions in the final program are very strange*. . It is very unclear who knew what and when they knew it.

PC members or sponsors may have been unpleasantly surprised.

The conference schedule was:

www.interfacesymposia.org/Interface2010 > Important Dates

“April 15, 2010 Last day to submit Refereed Papers ...

May 1, 2010 Last day to submit Contributed Paper Abstracts

May 1, 2010 Last Day for Invited Paper Abstracts³⁸

June 17, 2010 Keynote, Technical Sessions, Conference Banquet”

The last pre-conference announcement³⁹ was created 04/16/10:

www.interfacesymposia.org/Interface2010 > 2nd Announcement

That listed 22 Invited Sessions, all recognizable later in the Program Book:

www.interfacesymposia.org/Interface2010/ScheduleforInterface2010.pdf

It also showed others, *’d items as sessions INV-29, 10, 8, 29:

“Additional Sessions are being organized:

***Learning from Proximity Data**, Michael W. Trosset;

***New Developments in Statistical Data Integration**, Michael G. Schimek

* **21st Century Applications of Agent-Based Models**, Yasmin H. Said
Computational Epidemiology Applications to Public Health, Yasmin Said;

***Visualizing Intrusion Detection Data**, Rida E. Moustafa; and

Non-professional Practices of Data Collection and Analysis, Mark H. Hansen”

One more unsurprising session was added INV-27, run by Hamparsum Bozdogan, who may just have had trouble getting the session together earlier. Statisticians present current work to statisticians in 27 of 29 Invited sessions, whose topics and speakers were almost entirely announced earlier.

³⁸ Depending on the conference, invited papers may well be accepted with minimal review, or the PC may recruit papers through a standard review process. In this relatively-informal kind of conference, papers are accepted by Abstract, not by full papers, which come later if at all, as sometimes proceedings just include the talks. Some sessions get set early, but such conferences may have relatively late dropouts or speaker changes. Here, almost everyone on the PC organized one or more sessions, and most presented as well. Most of the conference is invited, with PC members recruiting speakers.

³⁹ Typically, for this kind of conference, the first announcement (in this case, January 2010) offers a preliminary schedule and sketches the topics. The last has a much more detailed schedule so uncommitted people can decide.

But, with no obvious warning, 2 new and very strange sessions appeared, and if people made the May 1 deadline, all this happened in just 2 weeks.

Following are the new sessions:

“Inv – 4 Perspectives on Climate Change

Organizer: Yasmin H. Said, Session Chair: Edward J. Wegman

Testing the hypothesis of anthropogenic global warming: A continuing controversy,

S. Fred Singer, Science & Environmental Policy Project

Extracting information from large-scale computer model output,

Mark Berliner, Ohio State

Discussant: Edward J. Wegman, George Mason”

Inv – 15 Policy Issues on Climate Change

Organizer: Yasmin H. Said, Session Chair: Edward J. Wegman

Global warming: Nexus of politics, economics and science,

Jeff Kueter, The Marshall Institute

Global warming--fact, fiction, and fraud,

Don Easterbrook, Western Washington University

Climate change policy and the climategate scandal,

Yasmin H. Said, George Mason

Of the 4 Invited speakers, Mark Berliner is a quite-credible statistician involved with climate science. None of the others are statisticians or climate scientists. All have histories of active climate-anti-science advocacy. These are quite familiar names to those who follow this topic. But first, the abstracts for these sessions follow:

“INV-4 Perspectives on Climate Change

Testing the Hypothesis of Anthropogenic Global Warming: A Continuing Controversy

S. Fred Singer, Science and Environmental Policy Project

The preferred test compares observed temperature trends with those derived from (greenhouse) climate models. I will discuss the statistical and other uncertainties of both sets of data.”

Singer presents ideas advocated for 20 years. This is likely similar to a talk given May 2010 for the Heartland #4 Conference:

www.heartland.org/events/2010Chicago/PowerPoints/Monday%20-%20Session%203/Track%201%20-%20Science%201/Fred_Singer.ppt

**“Extracting Information from Large-Scale Computer Model Output
Mark Berliner, Ohio State University**

Massive computer models are used in a variety of science and engineering applications. For example global atmospheric models have state spaces on the order of 10,000,000 variables. Earth system models, combining atmospheric, oceanic, cryospheric, and land surface process models, produce massive output. The scales of such models prohibit the production of many runs (ensembles), so establishing the statistical properties of their output is challenging. I review options for incorporating model output into Bayesian statistical analyses. I present two examples in the context of climate change analysis: (1) a simplified approach to detection and attribution of climate change, and (2) using multi-model ensembles in the projection of future climate.”

This seems a straightforward talk by a climate-knowledgeable statistician. Of course, this gets followed by Wegman as discussant. Unfortunately, no transcripts are available.

**“INV-15 Policy Issues on Climate Change
Global Warming: Nexus of Politics, Economics and Science
Jeff Kueter, President, George C. Marshall Institute**

The United States Congress is actively considering legislation to cap greenhouse gas emissions. Independently, the Environmental Protection Agency is moving to impose regulations on emissions as well. Pursuit of an international agreement to limit emissions continues. The belief that anthropogenic activities are negatively transforming the Earth’s climate motivates each of these efforts. Debate over the certainty of that conclusion as well as the economic cost and consequences of proposed mitigation efforts is generating opposition to these legislative, regulatory and international efforts. The presentation will review the economic and scientific aspects of the ongoing public policy debate.”

Kueter repeats GMI climate anti-science Memes, used for 20 years. He did not speak at Heartland#4, but he did at Heartland#3 in 2009, the topic is similar, and the video is available:

www.heartland.org/events/WashingtonDC09/proceedings.html

**“Global Warming, Fact, Fiction and Fraud
Don Easterbrook, Western Washington University**

The global warming debate is filled with facts, fiction, and fraud. The facts are that (1) the Earth has experienced natural global warming and cooling 4 times in the past century, 40 times in the past 500 years, and 60 times in the past 5000 years, long before CO# could possibly have been a factor, (2) at least 10

warm/cool climate fluctuations between 10,000 and 15,000 years ago were far more intense than recent warming, including warming of 15°F in 40 years, (3) from 1945 to 1977, while CO# was soaring, we had 30 years of global cooling, (4) although we’ve had global warming (1977 to 1999), Antarctic ice is not melting, (5) nothing that humans are doing can significantly affect global climate. The fiction is that (1) CO# is capable of producing warming of the atmosphere 10°F by the end of the century, (2) sea level will rise 20 feet this century, (3) global warming is causing extinction of polar bears, (4) carbon cap and trade will reduce atmospheric CO#, (5) carbon cap and trade will affect global warming. The fraud is (1) faking data, (2) changing climate data to make it appear warmer, (3) lying about Himalayan glacier retreat, (4) deliberate suppression of data that doesn't support CO₂ as the cause of global warming.”

Easterbrook gives a talk likely similar to one he gave at Heartland #4, although there it was called “The Looming Threat of Global Cooling” www.heartland.org/events/2010Chicago/PowerPoints/Monday%20-%20Session%201/Track%20%20-%20Science%202/Don_Easterbrook.ppt

**“Climate Change Policy and the Climategate Scandal
Yasmin H. Said, George Mason University**

The release of emails from the East Anglia University Climate Research Unit just before the Copenhagen Climate summit has had a damaging effect on public support for action on global warming. The lack of transparency by some climate researchers, the willingness to bend the peer review process, and the willingness to destroy data rather than share it with researchers of a different perspective all raise fundamental issues of climate change policy. Perhaps the best thing to come from the climategate scandal is the formal recommendation of engaging statisticians. In this talk I will discuss some of the implications of climategate on climate change policy.”

Singer and Kueter have substantial entries in [MAS2010]. Singer, of course has been doing climate anti-science advocacy for 20 years, often in conjunction with GMI, of which Kueter is the current President. GMI history is well-described in [ORE2010]. See also:

www.sourcewatch.org/index.php?title=Jeff_Kueter
www.sourcewatch.org/index.php?title=S._Fred_Singer

They are basically Washington-vicinity climate anti-science pros. Kueter’s background was mainly in Political Science, and was Research Director at

the National Coalition for Advanced Manufacturing (NACFAM). Both attended the meeting at GMI described in [GMI2003], for which GMI paid for an MM trip to Washington.

McIntyre had already been in contact, as shown by Singer, p.26:

“Steve McIntyre has been very helpful in sending me a whole bunch of data.”

This meeting included Aloysius Hogan, Inhofe counsel showing a strong interest in tree-ring statistics, unexpected for a lawyer, p.26-27:

“*Question:* Aloysius Hogan. I have heard questioning of the statistical and methodological practices associated with a number of papers and I would like to get an opinion from you both about the level of statistical and methodological analysis among normal peers. Are the people who are doing the peer review really qualified in those areas as statisticians or they are just educated laymen?”

Finally, it seems likely that Meme-b⁴⁰, Meme-c⁴⁰ may have started with Michaels⁴⁰ [GMI2003, p.10]:

“*Question:* Pat Michaels, University of Virginia. I think what you’re really uncovering here is a larger and pervasive problem in science, which is the peer-review process seems to be missing important and obvious issues, perhaps failing because of the sociology of global warming science.”

Other leadup to the WR is covered in [MAS2010], but it is worth knowing that Wegman, Said, Spencer, McIntyre, Singer, Kueter all attended a climate workshop November 14-16, 2005:

www.climatescience.gov/workshop2005/participants.htm

Although I do not know if they met, it certainly seems likely.

Easterbrook is a bit different, but certainly connected with Singer. He is an Emeritus geology professor of Western Washington State University, although “Emeritus” is lost here, as often happens for climate anti-science talks. Some geologists simply do not believe in AGW. He says we face global cooling, soon. He has spoken for Heartland#4, including a talk in May 2010, likely similar to that presented at Interface 2010 a month later. myweb.wvu.edu/dbunny

myweb.wvu.edu/dbunny/research/global/glocool.htm
www.sourcewatch.org/index.php?title=Don_Easterbrook
en.wikipedia.org/wiki/Don_Easterbrook
scienceblogs.com/deltoid/2010/05/don_easterbrook_hides_the_incl.php

It would be very interesting to hear from the PC when they found out about this. *It certainly seems that Wegman and Said invented these sessions in the last few weeks after the 2nd announcement was sent out.* These speakers would be ecstatic to speak at a credible-sounding conference. Singer has already described his Interface talk:

www.sepp.org/Archive/weekwas/2010/TWTW%202010-06-26%20_June%2026,%202010_.pdf

A.6.5 Odd loose ends at Wiley Interscience

Thanks to DC, this is a strange loose end likely related somehow to Wegman connections.

Wiley Interscience Reviews: Computational Statistics is a new journal, first issue was Jul/Aug 2009.

www3.interscience.wiley.com/journal/122458798/issueyear?year=2009

(Click on Editorial Board.)

“Edward J. Wegman, Bernard J. Dunn Professor of Data Sciences and Applied Statistics, George Mason University

Yasmin H. Said, **Professor**, Oklahoma State University, Ruth L. Kirschstein National Fellow, George Mason University *this is very strange.*

David W. **Scott**, Noah Harding Professor of Statistics, Rice University”

www.okstate.edu/registrar/Catalogs/E-Catalog/2009-2010/Faculty.html, and the associated PDF, created 08/05/09 both list Yasmin H. Said as an Assistant Professor in Statistics, still there 08/12/10, but not at statistics.okstate.edu/people/faculty.htm

There may have been some period when both Said and OSU thought she was coming there.

This appears to publish only “commissioned reviews.”

⁴⁰ By interesting coincidence, Michaels has been teaching a course at GMU Summer 2010 and Singer has long had various connections with GMU.

A.7 Funding, pro bono, or not

The WP's work was repeatedly touted as *pro bono*, but that may well mean nothing more than the lack of any direct payment to the WP for its work.

en.wikipedia.org/wiki/Pro_bono

When a lawyers work *pro bono*, they typically consume otherwise-billable time for public service, often for little obvious reward.

Of course, not all payment is in money, as per

[SAI2007, p.24]:

p. 24 “Some reactions”

“Writing Invitations

– Papers

- Statistical Science – on the hockey stick – not yet completed.

- Chance – on the Al Gore film, Inconvenient Truth – not yet completed.

- Computational Statistics and Data Analysis – on coauthor social networks – accepted for publication. *That is* [SAI2008], W.5.6.

– Book

- By Wiley – The Heated Debate – under contract.”

For 2 years past PhD, this was not bad, although only one happened.

It is less obvious to understand the meaning of *pro bono* for:

- University professors with multiple grant contracts.
- Graduate students, especially part-time students employed elsewhere.
- In general, researchers with flexible funding⁴¹.

[SAI2008, p.2184] acknowledges:

“The work of Dr. Yasmin Said was supported in part by the **National Institutes on Alcohol Abuse and Alcoholism under grant 1 F32 AA015876-01A1**. The work of Dr. Edward Wegman was supported in part by the **Army Research Office** under contract **W911NF-04-1-0447**. The work of Dr. Said and Dr. Wegman was also supported in part by the **Army Research Laboratory** under contract **W911NF-07-1-0059**. The content is solely the

⁴¹ R&D work in commercial industry or government contractors is sometimes tied to specific development contracts or accounts. Sometimes people have much more flexibility, to the point of being expected to generate “appropriate research” while charging some general account, perhaps only reporting in detail a year or two later. Some grants to university researchers work that way. I have sometimes managed efforts with specific deliverables and schedules, but other roles offered much more flexibility. For example, when I was at MIPS, I cofounded the SPEC benchmark group. I never got paid by SPEC for that, but I would not call that *pro bono*.

responsibility of the authors and does not necessarily represent the official views of the National Institute on Alcohol Abuse and Alcoholism or the National Institutes of Health.”

First, W.2.3 displays clear plagiarism. The Office of Research Integrity takes this seriously, and it certainly covers the NIAAA.

ori.dhhs.gov

ori.dhhs.gov/policies/plagiarism.shtml

“As a general working definition, ORI considers plagiarism to include both the theft or misappropriation of intellectual property and the substantial unattributed textual copying of another's work.”

Second, one must wonder about the NIAAA contract. Quite reasonable research can be made to sound silly, as per Sen. Proxmire's “Golden Fleece” awards. *But why does the NIAAA fund low-quality SNA that seems exists mostly to attack the paleoclimate research community?*

Third, the same question arises regarding the first Army contract, although it is hard to know without seeing it as Information Technology research can be fairly broad.

Fourth, in addition to the plagiarized material, much of the actual work seems to have been done by Rigsby and Sharabati, which is fine, as they are coauthors. Grad students often do much of the work.

Much of this work had been done in 2006 as part of [WR, WEG2006c, SHA2006]. That is also fine, as people certainly can build on earlier research. However it does lead to an awkward, if slightly subtle issue.

WR p.1 says:

“This Ad Hoc Committee has worked pro bono, has received no compensation, and has no financial interest in the outcome of the report.”

That seemed plausibly true at the time, although of course, not all compensation is financial. However, it seems that some work done as part of the WR effort yielded papers later submitted in partial fulfillment of various government contracts. Money is fungible and academic funding can be complex. Between these contracts and Rigsby's via Federal employment (NSWC), Federal money from unrelated contracts may have in effect covered some salaries and expenses for the WR and [WEG2006c]. Reese worked for MITRE, a government contractor.

During 2005-2006, Said was employed by Johns Hopkins University and that affiliation is the one listed on the WR. Did she do the WR work “on her own time” or was she in effect taking time from JHU teaching or research to work on the WR? If so, was this acceptable?

One can find many hits searching for Edward Wegman at:

www.dtic.mil/dtic

The tangle of research contract write-ups is still untraced, but so far one is located, for W911NF-07-1-0059:

[www.dtic.mil/cgi-](http://www.dtic.mil/cgi-bin/GetTRDoc?AD=ADA495346&Location=U2&doc=GetTRDoc.pdf)

[bin/GetTRDoc?AD=ADA495346&Location=U2&doc=GetTRDoc.pdf](http://www.dtic.mil/cgi-bin/GetTRDoc?AD=ADA495346&Location=U2&doc=GetTRDoc.pdf) :

“Several papers were developed based on research carried out under this project. These papers exploited aspects of the developments here. They are Said et al. (2007, 2008), Said and Wegman (2009), and Wegman and Said (2007). Presentations were given in a number of forums that credited this contract.”

Unfortunately, the PDF file does not actually give references for those⁴², but from [WEG2010] they seem to be:

Said et al (2007):

“178. “Implications of co-author networks on peer review,” with Yasmin H. Said, Walid K. Sharabati, John T. Rigsby in *Classification and Data Analysis*, Macerata, Italy: EUMEdizioni Università di Macerata, 245-248, 2007.”
That seems like a talk equivalent to [SAI2008].
It might be #185, an alcohol talk.

Said, et al (2008):

179. “Style of author-coauthor social networks,” with Yasmin H. Said, Walid K. Sharabati, John T. Rigsby, *Computational Statistics and Data Analysis*, 52, 2177-2184, 2008; doi:10.1016/j.csda.2007.07.021, 2007.
That is the mis-cited [SAI2008].

Said and Wegman (2009)

This might be #197 or #195, of which the first might be related. The second is an alcohol paper.

Wegman and Said (2007)

This is unclear, as the only ones I could find were #180, #184, #186, all of which are alcohol-related talks.

Given vague citations to non-existent references, it is difficult to know who is paying for what. Very little of this work seems obviously related to the

⁴² Problems with references and citations occur often.

Army research contract. It might (or might not) be fine work, but if these vague citations are presented as evidence of progress, one might think someone would ask what they are.

The Army Research Laboratory leads administration for ACAS, **A.6.3**: www.armyconference.org/50YEARS/Documents/Home%20Page%20Documents/About%20ACAS.pdf

Some 2006 and 2008 talks might have been questionable.

The Federal government pays for many things. It is not obvious why {NIAA, ARL, ARO, NSWRC} seem to be paying statisticians and statistical physicists to attack climate science. ARL pays for Bruce West (A.6.3) and has sometimes paid for work by Nicola Scafetta.

This topic remains unclear until we can see talks and any contracts, but:

- Do agencies understand what they are paying for?
- If so, can they explain how some of this research fits?⁴³
- If not, why not? Errors happen, so of more concern is that if agencies think this is an error, how will they tighten procedures to avoid it?
- If this is viewed as mis-use of funds, how do we get the money back?
- Can they explain seeming to pay for (poor) climate anti-science?

I would like to think the NSWRC and US Army spend money on research that actually furthers the defense of the USA. I hope these contracts are not just ways to send money to favored people. I hope the US Government did not, in effect, pay in part for the WR, retroactively.

Of course, the US Government paid during 2005-2006, at least:

- Spencer’s salary, and maybe others
- Cost of several days’ hearings
- Cost of North NRC panel, unnecessary without this.
- Any other expenses paid for people to testify, etc.

Many people wasted much time and effort. *Perhaps members of Congress have no other pressing business and this was just free time.*

⁴³ I understand how good SNA is quite relevant to intelligence-gathering, but low-quality SNA by inexperienced people who plagiarized SNA textbooks to attack climate scientists does not seem very relevant.

A.8 ASA Ethical Guidelines

The American Statistical Association offers clear Ethical Guidelines (II), of which a few relevant ones are excerpted below:
www.amstat.org/about/ethicalguidelines.cfm

Possible applications to the WR and follow-ons

Wegman and Scott are both Fellows of the ASA. I think Said is an ASA member as well. The following mainly apply to Wegman and Said.

<p>“A. Professionalism 1. Strive for relevance in statistical analyses. Typically, each study should be based on a competent understanding of the subject-matter issues, ...</p>	<p>NO: W.2, W.3, W.4, W.5, W.8, W.11. (McShane, Wyner might also have problems, A.12.)</p>
<p>2. Guard against the possibility that a predisposition by investigators or data providers might predetermine the analytic result. Employ data selection or sampling methods and analytic approaches that are designed to ensure valid analyses in either frequentist or Bayesian approaches. ...</p>	<p>Questionable: Did they guard against any predisposition by MM (in statistics) or by MM and/or Spencer in supplying references? <i>Was the WR predetermined?</i> See Biases in W.11.</p>
<p>5. Use only statistical methodologies suitable to the data and to obtaining valid results. For example, address the multiple potentially confounding factors in observational studies and use due caution in drawing causal inferences. ...</p>	<p>Questionable: The WR certainly emphasized uncertainties and confounding factors, but in conflict with A.1. When experts have spent decades disentangling signals from noise, to have people unfamiliar with the field to sprinkle “confounding factors” over everyone’s research is simply useless.</p>
<p>9. Respect and acknowledge the contributions and intellectual property of others. ...</p>	<p>NO: W.2, W.11.</p>
<p>11. Provide only such expert testimony as you would be willing to have peer reviewed. ...</p>	<p>NO: A.1 (Wegman)</p>
<p>C. Responsibilities in Publications and Testimony 1. Maintain personal responsibility for all work bearing your name; avoid undertaking work or coauthoring publications for which you would not want to acknowledge responsibility. Conversely, accept (or insist upon) appropriate authorship or acknowledgment for professional statistical contributions to research and the resulting publications or testimony. ...</p>	<p>A report like this may be signed by all authors, indicating general agreement (if not necessarily agreeing with every word) or it can label sections with specific authors. <i>I think Scott only wrote Appendix A, with little other involvement. He may have been surprised to have been listed as 2nd author. If so, I really wish he had clarified his relative lack of involvement years ago, despite the obvious personal awkwardness of doing so. His fine reputation has been used to add unmerited credibility to the WR, since otherwise it was Wegman + his students.</i></p>
<p>3. In publications or testimony, identify who is responsible for the statistical work if it would not otherwise be apparent. ...</p>	<p>NO: Although Rigsby’s SNA work was explicitly acknowledged later, not in the WR itself (probably acceptable), it is unclear who actually wrote W.4.</p>
<p>5. Account for all data considered in a study and explain the sample (s) actually used.</p>	<p>NO: W.5.8 omitted half the data and <i>likely was sourced from McIntyre.</i></p>
<p>6. Report the sources and assessed adequacy of the data. ... 13 Share data used in published studies to aid peer review and replication, ...”</p>	<p>NO: despite requests from Ritson, W.4.1, as-yet unfulfilled promises of peer-reviewed papers, A.1.4, 4 years later.</p>

A.9 Said dissertation

[SAI2005] received the Outstanding Ph.D. Dissertation Award, 2005. She finished Spring 2005, having started her PhD in Fall 2003.

www.galaxy.gmu.edu/stats/syllabi/IT871/MasterCopyDissertation.pdf

www.galaxy.gmu.edu/stats/awards.html

BUT, “terry” found something interesting:

deepclimate.org/2010/08/03/what-have-wegman-and-said-done-lately/#comment-4755

[SAI2005, pp.6-10] is strikingly similar to pp.1-2 of U Wisconsin Chemistry Professor Bassam X. Shakashiri’s page:

www.scifun.org/CHEMWEEK/PDF/Ethanol.pdf (2009) cited as

Shakhashiri, B. Z. (2005). “The chemical of the week,”

scifun.chem.wisc.edu/CHEMWEEK/ETHANOL/ethanol.html

web.archive.org/web/*/scifun.chem.wisc.edu/CHEMWEEK/ETHANOL/ethanol.html Internet Archive of earlier versions.

Not only is her text strikingly similar to Shakashiri’s, but the style is very similar to that of **W.2.3** - no quotation, no obvious distortions, just straightforward plagiarism. The source is occasionally cited, but without quotation, discouraged by the GMU English Department, **W.11.1**. The first and last paragraphs of pp.6-10 are shown below, the rest is the same, as recently analyzed in [DEE2010p]. It also shows more errors induced by trivial changes, such as turning corn and potatoes into “wheat plants.”

The whole section seems out of place, unrelated to the rest of the dissertation. It is unclear how this got by her Committee. Said later co-directed Sharabati’s dissertation, also an award-winner, but which also has plagiarism and bibliography-padding issues, **W.5.7**.

This is one of the files that disappeared August 2010.

[SAI2005, p.6], **Paragraph 1**

Ethanol is a clear liquid with a fairly sweet taste in dilute solutions, but can result in a burning taste at higher concentrations. Ethanol, CH₃CH₂OH, is classified as an alcohol, which is characterized by a hydroxyl group attached to a carbon atom. Although the term alcohol originates from the Arabic al-kuhul, or a fine powder used as eye makeup, medieval alchemists later applied the word to products of distillations, which is where the current term gets its usage (Petrucci, 2001; Shakhashiri. 2005).

Shakashiri, p.1.

Ethanol (ethyl alcohol, grain alcohol) is a clear, colorless liquid with a characteristic, agreeable odor. In dilute aqueous solution, it has a somewhat sweet flavor, but in more concentrated solutions it has a burning taste. Ethanol, CH₃CH₂OH, is an alcohol, a group of chemical compounds whose molecules contain a hydroxyl group, -OH, bonded to a carbon atom. The word alcohol derives from Arabic al-kuhul, which denotes a fine powder of antimony used as an eye makeup. Alcohol originally referred to any fine powder, but medieval alchemists later applied the term to the refined products of distillation, and this led to the current usage.

[SAI2005, p.10], **Paragraph 2**

Because directly measuring alcohol levels in the brain is rather difficult, monitoring of blood alcohol level serves as a good substitute. Impairment of brain functions for most people begin to become noticed at around a blood alcohol percentage of 0.05, while clearly noticeable physical impediments become visible at around a percentage of 0.10, and slurred speech becomes evident at 0.15 percent. Most people lose consciousness at a blood alcohol level of 0.4 percent, and at a level of 0.5 percent the brain’s breathing center and the pumping of the heart can become anesthetized resulting in their impediment. While reaching such an extreme degree of intoxication is quite improbable, a 150 pound human may attain this level after the quick ingestion of a fifth of a gallon of 100 proof alcohol.

Indeed, death is usually considered a serious impediment.

Shakashiri, p.2.

Alcohol levels in the brain are difficult to measure, and so blood alcohol levels are used to assess degree of intoxication. Most people begin to show measurable mental impairment at around 0.05 percent blood alcohol. At around 0.10 percent, mental impairment will show obvious physical signs, such as an unsteady walk. Slurred speech shows up at around 0.15 percent. Unconsciousness results by 0.4 percent. Above 0.5 percent, the breathing center of the brain or the beating action of the heart can be anesthetized, resulting in death. Reaching this level of blood alcohol by ingestion is unlikely, however. In a 150-pound human, it would require rapid consumption of a fifth gallon of a 100-proof spirit.

A.10 Possible legal issues

I am certainly no lawyer, but the Internet offers good access to legal documentation. *Following are some that might be relevant, although I have no idea how courts actually interpret these.* The first part is from [MAS2010, A.14], to which are now added **18.U.S.C §1512, §1519.**

18.U.S.C §1001 Misleading Congress is a felony

codes.lp.findlaw.com/uscode/18/I/47/1001

(a, 1), (c, 2) felony (up to 5 years)

18.U.S.C §4 , as is not reporting it

codes.lp.findlaw.com/uscode/18/I/1/4

Misprision of felony (up to 3 years)

18.U.S.C §371 : Conspiracy to commit felony is also a felony...

codes.lp.findlaw.com/uscode/18/I/19/371

conspiracy (up to 5 years)

www.justice.gov/usao/eousa/foia_reading_room/usam/title9/crm00652.htm

If I interpret this correctly, an “unfulfilled conspiracy” may not be affected by usual statutes of limitations.

Sometimes people can be involved in a conspiracy without even knowing it.

criminal.lawyers.com/federal-criminal-law/blogs/archives/629-Federal-Criminal-Conspiracy-Law.html

www.juryinstruction.com/members/content/national/ncjic_documents/chapter083/83_2.htm

I have no idea how widely applied that actually is.

Defamation is complex, especially Internet & international

www.expertlaw.com/library/personal_injury/defamation.html#4

www.ibls.com/internet_law_news_portal_view.aspx?s=latestnews&id=1874

4

www.article19.org/advocacy/defamationmap/map

www.thebarcode.net/pdf/CheatSheetSamples.pdf

en.wikipedia.org/wiki/Defamation Can be criminal some places .

This is complex to a legal layman (like me) and of course in the USA, defamation law varies by state. I am not sure exactly how that interacts with the Internet Era. *Much current activity seems to me like purposeful organized defamation, often using the Internet as an amplifier. The Internet and even email have not encouraged civility. I conjecture that this*

sometimes leads to threats of violence by the final consumers of this material or at least mass floods of email from ill-informed people demanding resignations of scientists for no good reason. Of course, in many cases, actions that might be legally ruled defamation never get pursued, due to time or financial limitations.

Even simple plagiarism is a serious problem in academe.

The following might eventually be relevant to **A.11.**

18.U.S.C §1512 Tampering with a witness ...

codes.lp.findlaw.com/uscode/18/I/73/1512

“(c) Whoever corruptly -

(1) alters, destroys, mutilates, or conceals a record, document, or other object, or attempts to do so, with the intent to impair the object's integrity or availability for use in an official proceeding; or

(2) otherwise obstructs, influences, or impedes any official proceeding, or attempts to do so, shall be fined under this title or imprisoned not more than 20 years, or both.”

18.U.S.C §1519 Destruction, alteration, ... of records

codes.lp.findlaw.com/uscode/18/I/73/1519

“Whoever knowingly alters, destroys, mutilates, conceals, covers up, falsifies, or makes a false entry in any record, document, or tangible object with the intent to impede, obstruct, or influence the investigation or proper administration of any matter within the jurisdiction of any department or agency of the United States or any case filed under title 11, or in relation to or contemplation of any such matter or case, shall be fined under this title, imprisoned not more than 20 years, or both.

www.fas.org/sgp/crs/misc/RS22783.pdf

“Obstruction of justice is the frustration of governmental purposes by violence, corruption, destruction of evidence, or deceit. It is a federal crime. In fact, it is several crimes. Obstruction prosecutions regularly involve charges under several statutory provisions. Federal obstruction of justice laws are legion; too many for even passing reference to all of them in a single report.”

en.wikipedia.org/wiki/Spoilation_of_evidence

A.11 Modified and disappeared files

A.11.1 History

Between ~08/16/10 and 08/23/10, files disappeared that had been unchanged for years (*) or at least months (+). Another (#) was edited. The main site is: www.galaxy.gmu.edu

The relevant departments are computer-literate, so one might assume security is adequate to prevent unauthorized file deletions or edits.

*[SAI2005] Said Dissertation, analyzed by [DEE2010p]

www.galaxy.gmu.edu/stats/syllabi/IT871/MasterCopyDissertation.pdf

This disappearance might be related to "terry" 08/03/10 at 9:26am: deepclimate.org/2010/08/03/what-have-wegman-and-said-done-lately/#comment-4755 and August 4 2:10 am comment. A copy is: web.archive.org/web/20060905150733/www.galaxy.gmu.edu/stats/syllabi/IT871/MasterCopyDissertation.pdf

+ [WEG2010] Resume of Edward Wegman, February 2010.

www.galaxy.gmu.edu/stats/faculty/wegman.resume2.pdf

This disappearance seems odd, and might just be coincidence.

*[SAI2007] Said colloquium, 09/07/07 in [GMU2007]

www.galaxy.gmu.edu/stats/colloquia/AbstractsFall2007/TalkSept7.pdf

This disappearance makes sense, as this whole talk was a very bad idea.

As of 08/27/10, the first hit from Google: Experiences with Congressional Testimony: Statistics and The Hockey Stick Yasmin H. Said showed the following, and Quick View still had a copy of the file

“[PDF] [Microsoft PowerPoint - YHS Pro_Bono_Statistics_and_Public_Policy](#)

File Format: PDF/Adobe Acrobat - [Quick View](#)

Sep 7, 2007 ... Experiences with Congressional Testimony: Statistics and The Hockey Stick. Yasmin H. Said. George Mason University ...

www.galaxy.gmu.edu/stats/colloquia/AbstractsFall2007/TalkSept7.pdf”

By 09/02/10 that had expired.

#[GMU2007] 2007 Fall Statistics Colloquia

www.galaxy.gmu.edu/stats/colloquia/ColloquiaFall2007.html

On 08/23/10, I noticed that the file no longer listed [SAI2007], and the last modification time was Friday, August 20, 2010 6:56:03 AM.

web.archive.org/web/20071223215639/www.galaxy.gmu.edu/stats/colloquia/ColloquiaFall2007.html Unchanged from 12/23/07 to ~08/20/10

About that time, the red-marked section disappeared.

“Statistics Colloquium Series

Fall 2007

The Statistics Colloquium Series is sponsored by the [Department of Computational and Data Sciences](#), the Center for Computational Data Sciences, and the [Department of Statistics](#), at [George Mason University](#).

This seminar series can be used to satisfy one of the requirements in the PhD program in [Computational Statistics](#) in the Department of Computational and Data Sciences.

Students may also learn about employment or intern opportunities from speakers in informal discussions before or after the presentations.

The colloquia are open to all.

Colloquia are generally held on Fridays at 10:45 am

Coffee and refreshments are served at 10:30 in the same room as the colloquia.

[Directions to the Fairfax Campus](#) and a [campus map](#) are available.

For Fall of 2007, most seminars are held in Research 1, Room 301

If driving, visitors should use the visitor's parking area in the Parking Deck (near the middle of the map). Signs on campus point the way to the Parking Deck. Visitors using Metro can take a [bus from the Vienna Metro Station](#).

Seminars are canceled whenever classes at GMU are canceled for snow or other reasons. Announcements of cancellations are made in local media, as well as at the [main GMU webpage](#).

September 7, 2007

[Experiences with Congressional Testimony: Statistics and The Hockey Stick](#)
Yasmin H. Said

[Department of Computational and Data Sciences, College of Science, George Mason University](#)

[Abstract](#)

[Slides from Talk](#)

[Location: Research 1, Room 301](#)

September 14, 2007

[Visualizing Cluster-Compressed](#)

[Multivariable and Multialtitude Atmospheric Data](#)

[Daniel B. Carr](#)

[Department of Statistics, George Mason University](#)

[Abstract](#)

[Slides from Talk](#)

[Location: Research 1, Room 301”](#)

Thus, not only did the file disappear, but someone attempted to edit away the record of its existence.

However, the Washington Statistical Society 2007 seminar list still exists, as of 09/16/10

www.scs.gmu.edu/~wss/sem2007.html last changed 11/24/09:

“7 Fri. George Mason University
CDS/CCDS/Statistics Colloquium Series
[Experiences with Congressional Testimony: Statistics and The Hockey Stick](#)”

It links to an informative abstract as well:

www.scs.gmu.edu/~wss/sem2007.html#070907b

“Title: Experiences with Congressional Testimony: Statistics and The Hockey Stick

Speaker:

Yasmin H. Said

Department of Computational and Data Sciences

George Mason University

Time: 10:30 a.m. Refreshments, 10:45 a.m. Colloquium Talk

Date: September 7, 2007

Location:

Department of Computational and Data Sciences George Mason University

Research 1, Room 301, Fairfax Campus

George Mason University, 4400 University Drive, Fairfax, VA 22030

Abstract:

Rarely does the federal government need advice on theoretical statistics. I would like to talk about one exception. **Efforts to persuade Congress to enact legislation that affects public policy are constantly being made by lobbyists who are paid by special interests.** While this mode of operation is frequently extremely effective for achieving the goals of the special interest groups, it often does not serve the public interests in the best possible way. **As counterpoint to this mode of operation, pro bono interaction with individual legislators and especially testimony in Congressional hearings can be remarkably effective in presenting a balanced picture. The debate on anthropogenic global warming has in many ways left scientific discourse and landed in political polemic.** In this talk I will discuss our positive and negative experiences in formulating testimony on this topic. “

The reader might evaluate those comments in light of the rest of this report.

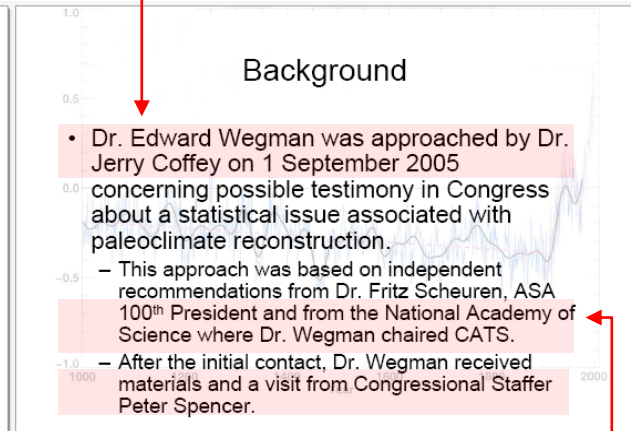
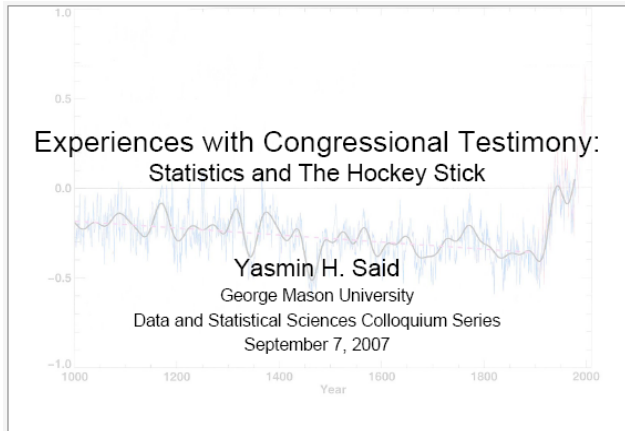
A.11.2 Said 2007 colloquium KEY SOURCE

[SAI2007] Said colloquium, 09/07/07 slides 1-6

www.galaxy.gmu.edu/stats/colloquia/AbstractsFall2007/TalkSept7.pdf originally

This file was so helpful that various people made copies, Winter 2010, such as deepclimate.files.wordpress.com/2010/09/said-talksept7.pdf

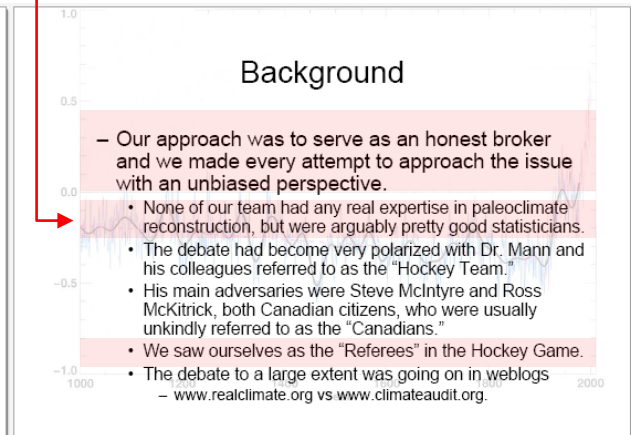
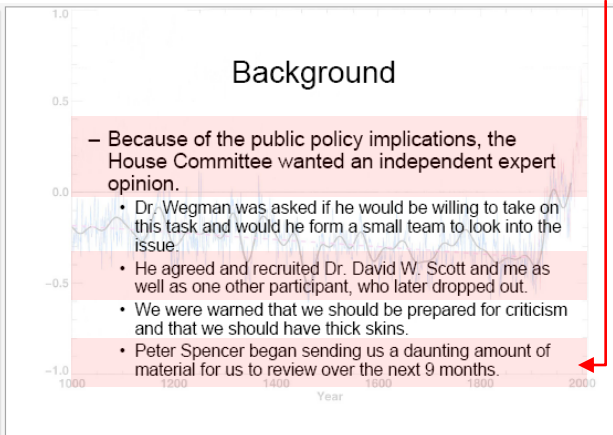
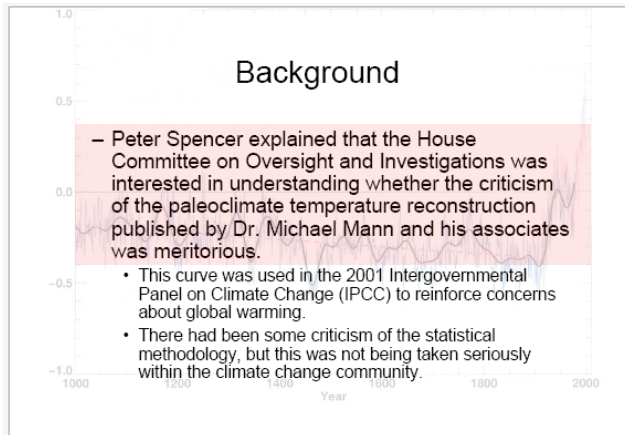
Coffey "Gore global warming boondoggle," recommends books by Solomon, Singer, Michaels.



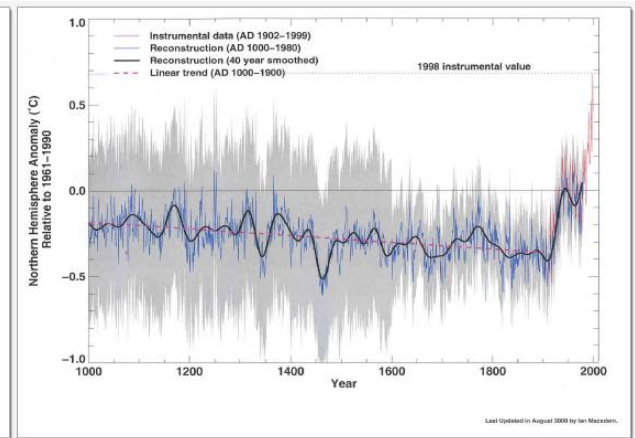
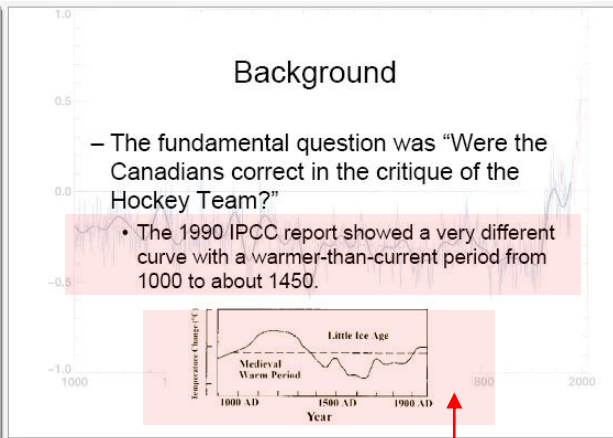
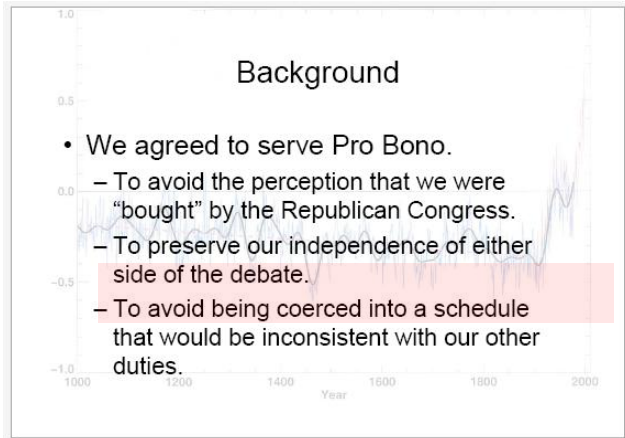
This is simply untrue. A.1.2.

Papers supplied through Spencer

At least the first phrase is clearly true.



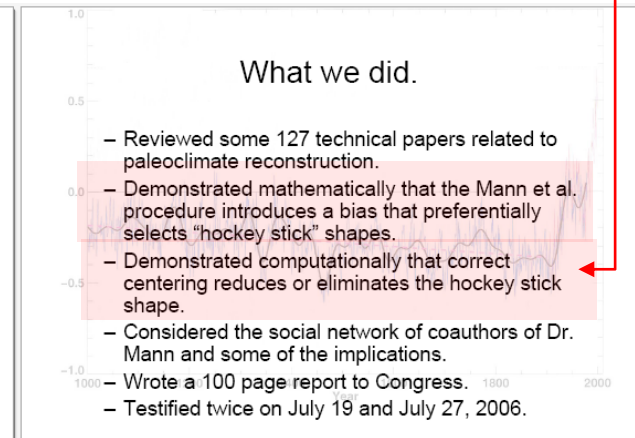
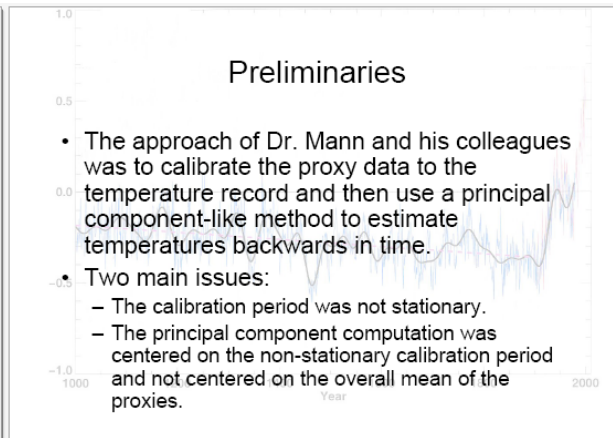
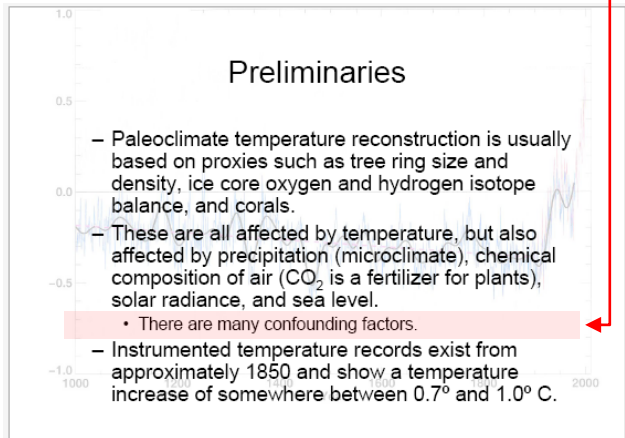
[SAI2007] Said colloquium, 09/07/07 slides 7-12



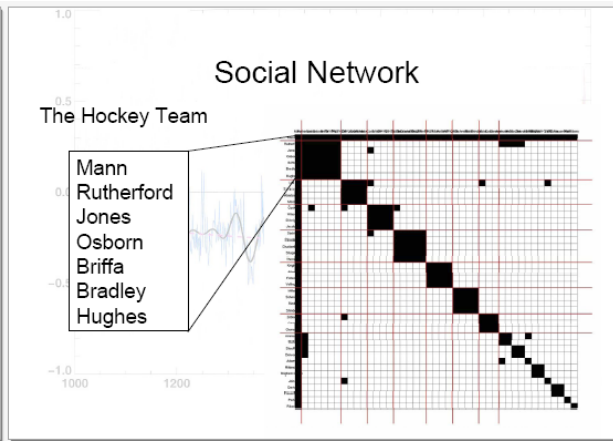
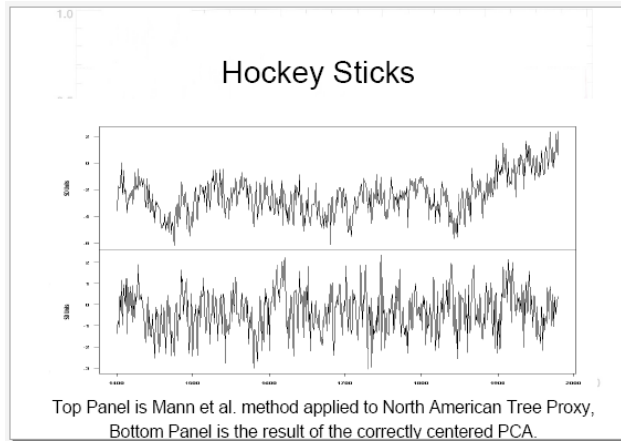
Confounding factors, Meme-e●

Said has the 1990 graph, but the WR shows distorted version, amplifies MWP, W.4.2.

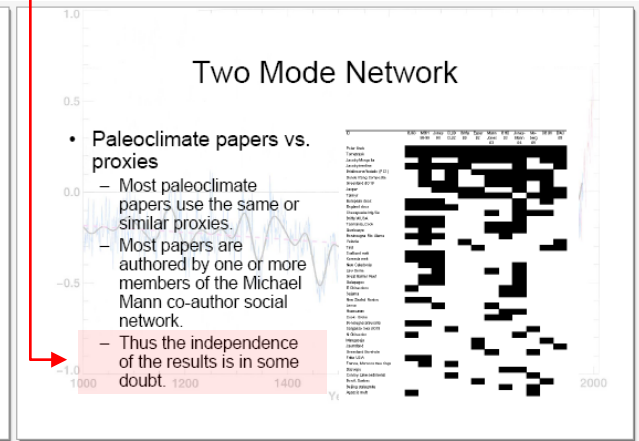
What they did was copy MM, but not redo it correctly as per Wahl, Ammann (2007), W.8.4.



[SAI2007] Said colloquium, 09/07/07 slides 13-18

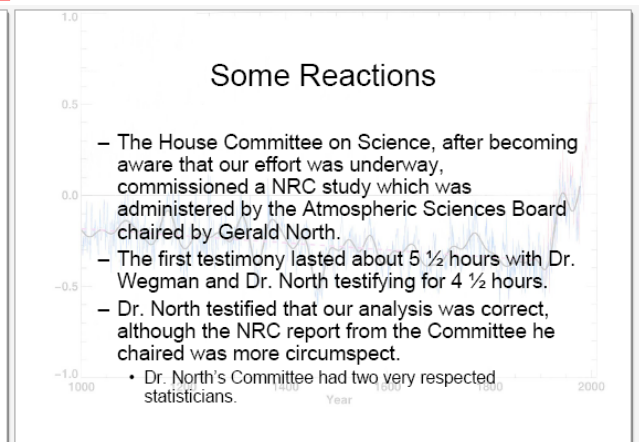
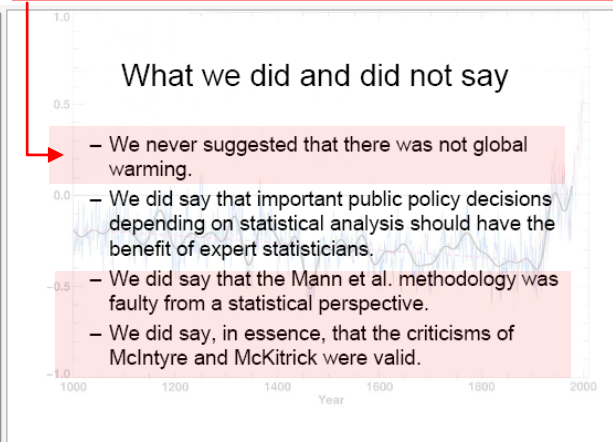
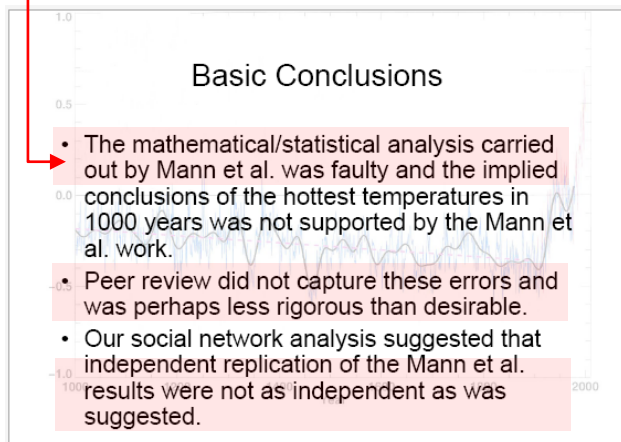


This chart omits the half of the data that disagrees, likely sourced from MM, W.5.8.



Decentering affects only a small part of the analysis. Done right, the charts are almost the same, W.8.4. The WR did not do the actual analysis.

See A.2, GW but never AGW,



[SAI2007] Said colloquium, 09/07/07 slides 19-24

But SNA testimony could not be dropped, as it was really one of the 2 key missions.

Half of the response was more poor SNA, and the response dodged the serious questions. In any case, was the WP "coached" by MM?

Some Reactions

- From Congress
 - The Republicans liked our findings.
 - It was interpreted as vindicating their skepticism on climate change although we never took a position on climate change.
 - We were called great patriots by Congressman Joseph Barton.
 - The Democrats didn't.
 - In preliminary discussions, we were pressed hard not to testify on the social network analysis.
 - Social network analysis was treated with great skepticism, even to the point of questioning us as to whether we had made this science up.
 - We were repeatedly asked to testify on whether anthropogenic global warming was real or not.

Some Reactions

- From Congress
 - The Democrats didn't.
 - During the second hearing, then Democratic Congressman Henry Waxman made a sustained attack on Dr. Wegman's credibility without allowing him to respond.
 - Fortunately, Dr. Ralph Cicerone, President of the National Academies was asked if Dr. Wegman were credible and he answered affirmatively.
 - Subsequent to the second testimony, Democratic Congressman Bart Stupak sent a letter of inquiry to us asking for additional written testimony. This was obviously coached by the "Hockey Team" asking very detailed statistical questions.
 - Our response was an additional 35 pages long.

Some Reactions

- From the Media
 - Pro
 - Wall Street Journal ran an editorial called "Hockey Stick Hokum."
 - Con
 - NPR ran sound bites quoting Dr. Wegman's honest testimony that he was not a climatologist, implying that he was not qualified to speak to the statistical issues.
 - Other negatives included New York Times, Chronicle of Higher Education, Washington Post and Scientific American.
 - All looked at the testimony superficially and assumed that finding statistical flaws was tantamount to denying global warming.

SNA itself is fine, the WR plagiarized the basics, did poor analysis using the wrong network ("egonet") and overstated the results, with no evidence, W.5.

One might like to know more about this. Did GMU know there was a problem in 2006?

Only one happened, and it was very poor. W.5.6. No peer-reviewed statistics analysis was published.

Some Reactions

- From the Media
 - Fun facts
 - The testimony was noted by newspapers as far away as Germany, England and Australia.
 - We were interviewed on Saudi Arabian television on this topic.
 - The report, popularly referred to as the "Wegman Report" was widely commented upon on the Web.
 - Googling "Wegman Report" returned more than 15,000 pages containing that phrase.
 - This testimony resulted in a page in Wikipedia being developed on Dr. Wegman.

Some Reactions

- Invitations
 - Good ones
 - We were invited to participate in a workshop at the National Center for Atmospheric Research – pro anthropogenic global warming.
 - We were invited to participate in a workshop by the Marshall Institute – anti anthropogenic global warming.
 - We were invited by the Annapolis Center for Science-Based Public Policy to participate in a workshop on peer review.
 - Bad Ones
 - We were invited by the Provost, the Dean of the College of Science, and the Vice President for Research at GMU to explain our testimony.

Some Reactions

- Writing Invitations
 - Papers
 - Statistical Science – on the hockey stick – not yet completed.
 - Chance – on the Al Gore film, Inconvenient Truth - not yet completed.
 - Computational Statistics and Data Analysis – on coauthor social networks – accepted for publication.
 - Book
 - By Wiley – The Heated Debate – under contract.

[SAI2007] Said colloquium, 09/07/07 slides 25-27

My Reactions

- Incredibly time consuming for no pay
 - Great visibility
 - No pay
- Almost deliberate misunderstanding by the press
 - Hear what they want to hear, not what we said on both sides of the climate change debate.
- Almost personal attacks on the weblogs and by Congressmen very disappointing
 - Credibility challenged, even personal attributes and manner of speaking remarked on.
- I would do it again.
 - It was most interesting experience, but can't afford to do it too often.

Some Contacts

Contact Information

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This is not a bad outcome for a new PhD.
At right, Said is shown with:

Congressman Barton
Congressman Whitfield
Peter Spencer, Barton staffer (as now)
Mark Paoletta, then a Whitfield staffer

Freeman Dyson (not obvious why he is in this)

Most seem very happy.

However, this talk may have been a serious mistake.

Some Contacts

A.12 McShane, Wyner (2010), WR remake, AOAS

A.12.1 August 2010, a new paper (MW)

[MCS02010, or MW] Blakeley B. McShane and Abraham J. Wyner, *A Statistical Analysis of Multiple Temperature Proxies: Are Reconstructions of Surface Temperatures Over the Last 1000 Years Reliable?* at AOAS: www.e-publications.org/ims/submission/index.php/AOAS/user/submissionFile/6695?confirm=63ebfddf (2nd version, 1st is gone) www.imstat.org/aoas

Some people still reference the WR positively or amazingly even rely on it for paleoclimate knowledge. In this remake, another pair of statisticians start with plagiarism, fabrication, dubious sources and other scholarship issues, then write sweeping conclusions. Fabrication and plagiarism are: www.upenn.edu/academicintegrity/ai_codeofacademicintegrity.html www.upenn.edu/academicintegrity/ai_citingsources.html www.northwestern.edu/provost/students/integrity/rules.html

MW starts with unacknowledged use of text from Wikipedia, interleaved with material from the WR, which derives introductory material from Bradley (1999), but with errors, **W.2**. MW rephrases WR ideas without attribution, obvious from 4 tip-offs not in Bradley (*plagiarism*), Issue-P⁶. MW then vaguely references Bradley (*fabrication*), Issue-F⁶. The 4 tip-offs are shown in context, **A.12.2**:

- “Artifacts” is a very odd word usage. People do not normally call tree-ring growth patterns, coral growth, ice-core data “artifacts.”
- “Ions and isotopes of hydrogen and oxygen” is wrong, uses the WR’s miscopy, but “ions” is not the same as Bradley’s “major ions.”
- “Speleotherms” is an uncommon misspelling of “speleotherms,” miscopied in WR as “speleothrems,” MW fixed that, but incorrectly.
- Finally, MW repeats the WR’s misspelling of Bradley’s book as “Quarternary” in place of “Quaternary.”

After 3 weeks, the two grey tip-offs get fixed, as in the timeline:

08/11/10 12:34AM: the original MW PDF has the 4 tip-offs.

08/16/10: I note “artifacts” and other MW issues at Deltoid:

scienceblogs.com/deltoid/2010/08/a_new_hockey_stick_mcs Shane_and_wyner/#comment-2730170

08/19/10: and at Deep Climate:

deepclimate.org/2010/08/19/mcshane-and-wyner-2010/#comment-5113

08/19/10: Eduardo Zorita discusses issues, including ice versus air bubbles klimazwiebel.blogspot.com/2010/08/mcshane-and-wyner-on-climate.html

08/28/10: discuss “ions and isotopes” and “speleothrems –speleotherms.” deepclimate.org/2010/08/19/mcshane-and-wyner-2010/#comment-5247

08/29/10: There is plagiarism and fabrication discussion

deepclimate.org/2010/08/19/mcshane-and-wyner-2010/#comment-5264

08/29/10: and more on plagiarism and fabrication.

deepclimate.org/2010/08/19/mcshane-and-wyner-2010/#comment-5273

08/31/10 2:02PM “Quarternary” FIRST MENTION (5PM EDT)

deepclimate.org/2010/08/19/mcshane-and-wyner-2010/#comment-5292

08/31/10 4:38PM I confirm Elsevier/Academic Press error in earlier post deepclimate.org/2010/08/19/mcshane-and-wyner-2010/#comment-5301

09/01/10, 10:01AM⁴⁴ new MW PDF fixes the 2 grey tip-offs, A.12.8.

No more than 18 hours after “Quarternary” is posted, the 2 spelling errors are fixed, but not the other 2 tip-offs. Perhaps this is coincidence, perhaps MW follow Deep Climate or have friends who do. Some relatively high traffic blogs *include many people certain that the MBH hockey stick is a fraud perpetrated by a conspiracy of climate scientists, some of whom receive frequent hate mail and threats of violence, not coincidentally.*

On one hand, some instantly hail it as the end of the hockey stick (again):

Steve McIntyre is quick, 08/14/10:

climateaudit.org/2010/08/14/mcshane-and-wyner-2010

“A reader (h/t ACT) draws attention to an important study on proxy reconstructions (McShane and Wyner 2010) in the *Annals of Applied Statistics* (one of the top statistical journals)”

Andrew Montford ([MON2010]), 08/15/10

bishophill.squarespace.com/blog/2010/8/15/here-come-the-cavalry.html

“This thread is for discussion of the [McShane and Wyner](#) paper, which looks as though it is going to be a pretty significant contribution to the Hockey Stick debate.”

⁴⁴Time zones: I assume MW PDFs are either Eastern (if Wyner) or Central (if McShane); Deltoid: Eastern; Deep Climate: Pacific. I recheck some files daily.

Anthony Watts, 08/17/10 (originally), re-dated 08/30/10:
wattsupwiththat.com/2010/08/30/breaking-new-paper-makes-a-hockey-sticky-wicket-of-mann-et-al-99

Oh, my. There is a new and important study on temperature proxy reconstructions (McShane and Wyner 2010) submitted into the [Annals of Applied Statistics](#) and is [listed to be published](#) in the next issue. This paper is a direct and serious rebuttal to the proxy reconstructions of Mann. It seems watertight on the surface... Not only that, this paper is a confirmation of McIntyre and McKittrick's work, with a strong nod to Wegman. I highly recommend reading this and distributing this story widely."

James Delingpole, in **The Daily Telegraph** (UK), 08/17/10:
blogs.telegraph.co.uk/news/jamesdelingpole/100050792/why-conservatives-shouldnt-believe-in-man-made-climate-change

"Jeff Id" at The Air Vent., 08/18/10:
noconsensus.wordpress.com/2010/08/18/mw10-some-thoughts/

"In my opinion it is a landmark paper in its efforts to quantify the uncertainty in the proxies."

Wall Street Journal, 09/02/10, **"Climate of Uncertainty,"**
online.wsj.com/article/SB10001424052748703467004575463433671739148.html

"A forthcoming paper in *Annals of Applied Statistics* details the uncertainties in trying to reconstruct historical temperatures using proxy data such as tree rings and ice cores. Statisticians Blakeley McShane and Abraham Wyner find that while proxy records may relate to temperatures, when it comes to forecasting the warming observed in the last 30 years, **"the proxies do not predict temperature significantly better than random series generated independently of temperature."**

This seems to be the WSJ's first-ever mention of AOAS. Does it now study statistics journals? or just read certain blogs? or get email? *Should this result not stand up, will this piece be retracted clearly? Will readers see that? Or will the meme remain, like the grin of Alice's Cheshire cat?*

In **The Australian**, Marc Hendrickx takes a bit longer, 09/14/10:
www.theaustralian.com.au/news/opinion/aunty-is-mistaken-but-not-malicious/story-e6frg6zo-1225921441996

On the other hand, others quickly note issues with science or statistics: The Policy Lass, 08/15/10, notes early reactions, good zombie picture:
shewonk.wordpress.com/2010/08/15/the-eternal-return [POL2010]
shewonk.wordpress.com/2010/08/19/the-hockey-schtick

Tim Lambert, 08/16/10 finds MW looks more like hockey stick than most:
scienceblogs.com/deltoid/2010/08/a_new_hockey_stick_mcschane_and.php
 "Eli Rabett," 08/16/10, comments on noise vs proxies statistics:
rabett.blogspot.com/2010/08/flat-new-puzzler.html

Deep Climate, 08/19/10, **finds many problems in science and statistics**
deepclimate.org/2010/08/19/mcschane-and-wyner-2010 [DEE2010n]

Eduardo Zorita, 08/19/10 covers many science problems [ZOR2010]
klimazwiebel.blogspot.com/2010/08/mcschane-and-wyner-on-climate.html
 RealClimate 08/20/10, lasso and a few other issues.

www.realclimate.org/index.php/archives/2010/08/doing-it-yourself

And around 09/21/10, scholarly commentary starts appearing:

Schmidt, G. A., M.E. Mann, S. E. Rutherford (2010) in press at AOAS.
pubs.giss.nasa.gov/cgi-bin/abstract.cgi?id=sc06400f

Martin Tingley, NCAR and Harvard:

www.people.fas.harvard.edu/~tingley/Blakeley_Discussion_Tingley_Long_Version.pdf

www.people.fas.harvard.edu/~tingley/Blakeley_Discussion_Tingley_Submitted.pdf

Martin Tingley, et al

statistics.stanford.edu/~ckirby/techreports/GEN/2010/2010-09.pdf

On the third hand,⁴⁵ (not a substitute for statistics, but a complement): MW shows a clear case of plagiarism of 2 sources, although more sophisticated than the WR's pervasive cut-and-paste. **It includes 6 clear fabrications.** MW is another Professor+recent PhD pair with no clear climate expertise, whose errors show that in the first pages. MW amplifies WR and MM, makes sweeping generalizations and cites sources wrongly. Statements from secondary or even tertiary sources are mis-attributed to credible primaries that may contradict those statements. Some citations are dubious or at least rather unlikely reading. Obsolete sources and popular press articles are featured.

As WR tried to ratify MM, MW tries to ratify the WR. MBH98, MBH99 are *still* foci, totaling 21 of 124 citations. Mann, et al (2008) adds 17.

⁴⁵ Called "the gripping hand" (in some science fiction), fits here. Plagiarism or fabrication, if clear, are **really** clear and taken very seriously in academe.

Policy Lass's zombie picture is a good analogy: MW seems a newer remake of the WR, with improved special effects, but a similar plot, replacing adding Mann, et al (2008) as a new target.

A.12.2 Plagiarism, fabrication and confusion

The following page analyzes a few sentences near the start of MW. One would expect an introduction to show clear familiarity with a topic of which one makes strong statements. MW shows just the reverse, in some cases via silly errors that no one familiar with the field would make.

- The first sentence is plagiarized from Wikipedia, and the rest blends phrases from it and the WR's version of Bradley (1999), **W.2.1**.
- The plagiarism works via ideas and a few phrases, not the pervasive WR cut-and-paste. MW clearly write their own words, but include 4 obvious⁴⁶ tip-offs for sourcing from WR, not the cited Bradley (1999), Issue-P⁶, Link-W⁵.
- The green-highlighted items show 4 WR tip-offs - 1 oddity and 3 errors in common with the WR, but *not* Bradley.
- The WR is first mentioned 4 pages later, approvingly cited multiple times, although sometimes incorrectly.
- MW also creates a 2nd oddity, "rocks," a mystery as rocks are irrelevant to the paper's topic. DC recently found the antecedent in Wikipedia, from which MW blends words with the WR to produce a clear error. Paleoclimatologists' idea of long-term is millions or billions of years, not 1-2 mere millennia, and rocks are very long-term. The text has the right sorts of words, but when one reads very closely it is wrong.

One must have sympathy for Bradley (1999):

- WR p.11 copies and cites 2 tables from Bradley (1999), but manages to introduce 3 errors ("ions," "speleothems" and "phonological.")
- WR pp.13-15 not only plagiarize Bradley, but introduce errors, amplify uncertainties, directly invert an important conclusion.
- The WR then misspells the title's "Quaternary" as "Quarternary."
- Finally, MW plagiarizes an oddity and some errors of WR pp. 10-11, then fabricates a citation to Bradley (1999), repeating "Quarternary."

⁴⁶ Side-by-side comparisons are obvious, perhaps not before. Zorita quickly found some science errors ("ions and isotopes") but did not mention WR origin. I only noticed "artifacts" because I had seen the odd usage recently.

The following page is clear but later pages show more fabrication cases. **The rest of A.12 applies to the original MW manuscript at AOAS.** Two tip-offs already have been changed and the manuscript may well change again, but for the purposes of this report, those are irrelevant, as the issue here is the nature of the original document, **A.12.1, A.12.7**. Retroactively fixing tip-off errors does not undo the history.

The list below is a mini Index for **A.12**. The black items, especially Issue-F (fabrication, 6 clear cases) and issue-P (plagiarism, 2 clear sources) *are serious. The reader will have to judge, but so far I think I find, in a quick study, not even touching the statistics:*

- Plagiarism, fabrication
- Obvious errors in basic science, ignoring some Themes
- Strong language and even political statements
- Obsolete and/or "grey" references
- Propagation of a few Memes, although many less than the WR

. Issue-f ⁶, 103, 107
 . Issue-F₁⁶, 99
 . Issue-F₂⁶, 105
 . Issue-F₃⁶, 105
 . Issue-F₄⁶, 106
 . Issue-F₅⁶, 106
 . Issue-F₆⁶, 106
 . Issue-P₁⁶, 99
 . Link-W⁵, 98, 99, 107, 108
 .Link-m¹, 105
 .Meme-03¹, 108
 .Meme-56¹, 101, 107
 .Meme-f¹⁵, 107, 108
 .Meme-j¹, 101, 102, 104, 107, 108
 .Theme-A⁴, 107
 .Theme-B⁴, 103, 111
 .Theme-C⁴, 103, 111
 .Theme-G⁴, 107

The first few paragraphs of MW include 1 oddity and 3 errors from the WR, not Bradley, hence plagiarism of one, followed by fabrication.

MW p.1-2, Paragraph 4

1. Introduction. Paleoclimatology is the study of climate and climate change over the scale of the entire history of earth. A particular area of focus is temperature. Since reliable temperature records typically exist for only the last 150 years or fewer, paleoclimatologists use measurements from tree rings, ice sheets, rocks, and other natural phenomena to estimate past temperature. The key idea is to use various artifacts of historical periods which were strongly influenced by temperature and which survive to the present. For example, Antarctic ice cores contain ancient bubbles of air which can be dated quite accurately. The temperature of that air can be approximated by measuring the ratio of ions and isotopes of oxygen and hydrogen. Similarly, tree rings measured from old growth forests can be dated to annual resolution, and features can be extracted which are known to be related to temperature.

MW p.2, Paragraph 1

The "proxy record" is comprised of these and many other types of data, including boreholes, corals, speleotherms, and lake sediments (see Bradley (1999) for detailed descriptions).

- <e> "focus is temperature" Temperature is certainly important, but Bradley does not write this, the over-emphasis was injected by the WR.
- <E> "rocks"? What? Bradley (1999), 4.1 shows this irrelevant to topic.
- <e> "Artifacts" is as odd here as it is in the WR.
- <E> "Antarctic..." This is wrong, because the ice is accurately datable, not the air. WR p.3, p.14, got this right, mostly by cut-and-paste.
- "ratio of ions and isotopes of oxygen and hydrogen" would be read by most as "ratio of (ions and isotopes) of (oxygen and hydrogen)." That is confused and wrong. It comes from copying the WR gaffe of miscopying Bradley's "major ions and isotopes of hydrogen and oxygen" as "ions and isotopes of hydrogen and oxygen." W.2.1. "ratio" is odd.
- <e> "speleotherms" is a low-use variant of the correct "speleothems." MW likely picked up "speleothems" from WR p.11, realized it was a typo, and fixed it, and finally:
- <E> "Quaternary" is the correct term, not "Quarterly", below, obviously copied from the WR.:

MW p.42

Bradley, R. S. (1999). *Paleoclimatology: Reconstructing Climates of the Quaternary*. Academic Press, San Diego, 2nd edn.

This MW section averages about a problem per sentence.

Issue-P₁ 6, Issue-F₁ 6, Link-W 5.

DC found the Wikipedia antecedents. MW plagiarism is more subtle than the obvious cut-and-paste of the WR, but plagiarizing errors is a bad idea.

en.wikipedia.org/wiki/Paleoclimatology :

Paleoclimatology (also Palaeoclimatology) is the study of climate change taken on the scale of the entire history of Earth.

It uses records from ice sheets, tree rings, sediment, corals, shells and rocks to determine the past state of the climate system on Earth.

Wikipedia is right, but temperature is not a synonym for climate system. Had MW read a bit further in Wikipedia they might have learned about rocks.

en.wikipedia.org/wiki/Temperature_record_of_the_past_1000_years

A reconstruction is needed because a reliable surface temperature record exists only since about 1850.

Some WR seemed fair summary of Bradley pp.1-10, but underlined words are misleading, W.2.1. Bradley lists 8 properties, not just temperature.

WR, p.10 (first 2 sentences follow Bradley, but inject odd "artifacts")

Paleoclimatology focuses on climate, principally temperature, prior to the era when instrumentation was available to measure climate artifacts.

Many natural phenomena are climate dependent and, where records are still available, these phenomena may be used as proxies to extract a temperature signal. Of course the proxy signals are extremely noisy and thus temperature reconstruction becomes more problematic as one attempts reconstructions further back in time. Climate is not solely a matter of (global) temperature, although concerns with the effects of global warming focus primarily on temperature reconstructions. As just suggested, temperature reconstruction is based on proxy signals contained in the historical records of climate dependent natural phenomena. Table 1 based on Bradley (1999) illustrates the wide variety of these natural phenomena that may be used as proxies. The high frequency proxies that could be used on an annual basis include tree rings, ice cores, and corals. In addition to serving as temperature proxies, these measurements are proxies for other climatic variables including, for example, precipitation, chemical composition of air and water, and solar activity.

WR, p.11

"Geochemistry (ions and isotopes of oxygen and hydrogen) ... Speleothems (age and stable isotope composition)" VERSUS:

Bradley (1999) p.5

Geochemistry (major ions and isotopes of oxygen and hydrogen) ... Speleothems (age and stable isotope composition)"

WR p.53

Bradley, R. S. (1999) *Paleoclimatology: Reconstructing Climates of the Quaternary*, 2nd Edition, San Diego: Academic Press.

A.12.3 Bibliography

The Bibliography is better than that of WR, as it actually cites every one of the 51 references. Unfortunately, as shown in the cursory Tally below:

G (3) “Grey” includes popular press articles, other talks. This includes a BBC show, Al Gore’s AIT movie, and a NY Times article about a museum exhibit, none of which are very relevant.

G (8) “Dark grey” clear or long history of climate anti-science. This includes one so labeled for the authors (Green, Armstrong, Soon), all serious climate anti-science advocates, 3 WSJ pieces, 2 Wegman pieces, and the 2 MM pieces in E&E, not a credible journal.

N (6) Not relevant, clearly.

WR #	MCS #	W.12.2(a) Bibliography Tally	MW Page Read	# Cites	Cited on pages	Link#	Source Relevant Bias Memes Themes	Source
4	1	Amman, Joos, Otto-Bliessner, Tomas (2007)	42	1	33			PNAS (Ammann (sic))
4	2	Amman, Wahl (2007)	42	3	19,25,33			Climatic Change (sic)
3	3	BBC (09/14/08)	42 *	1	3		G N	Television
4	5	Bradley (1999)	42 *	1	2			Book
4	5	Christiansen, Schmith, Thejll (2009)	42	1	18			J of Climate
3	6	Gore. A (2006), A. I. T.	42 *	1	3		G N	Movie
4	7	Granger, Newbold (1974)	42	1	9	m		J of Econometrics
1	8	Green, Armstrong, Soon(2009)	42 *	1	19		G B	Int J of Forecasting
4	9	Haslett, Whitley, Bhattacharya, et al (2006)	43	1	35			J Royal Statistical Society
4	64	10 IPCC (2001) TAR	43 *	3	2,27	m		IPCC, [IPC2001]
4	11	IPCC (2007) AR4	43 *	1	2	m		IPCC, [IPC2007]
1	12	Johnson, K. (11/23/09) Climategate	43	1	5		G N B	WSJ
1	13	Johnson, Naik (11/24/09) Climategate	43	1	5		G N B	WSJ
1	14	Jolis, (11/18/09) Climategate	43	1	5		G N B	WSJ
3	15	Ladurie (1971), Time of Feast, Times of Famine	43	1	27			Book
3	16	Lamb (1990), Climate: Past, Present, Future	43	1	27			Book
4	17	Lee, Zweiers, Tsao (2008)	43	1	18			Climate Dynamics
4	18	Li, Nychka, Amman (2007)	43	9	6,32,33,35			Tellus
4	19	Li, Nychka, Amman (2010)	43	3	33			review JASA, Amman(sic)
4	20	Luterbacher, Dieterich, Xoplaki, et al (2004)	43	2	6,11			Science
4	21	Mann (2004) On smoothing...	43	1	26			Geophysical Res. Letters
4	22	Mann (2008) smoothing revisited	43	1	26			Geophysical Res. Letters
4	37	23 Mann, Bradley, Hughes (1998) MBH98	43 *	14	2,5,9,13,16,17,28,31,32,41	m		Nature
4	38	24 Mann, Bradley, Hughes (1999) MBH99	43 *	7	2,3,9,30,31,32	m		Geophysical Res. Letters
4	49	25 Mann, Bradley, Hughes (2004) Corrigendum	43 *		5,6	m		Nature

Theme A (3) for old climate references, Matthes (1939), Ladurie (1971), Lamb (1990 (*really 1972-1977, I think*)). These are famous, but science has progressed. *This seems like Bibliography-padding to claim expertise. Did they actually read these? They do use them to claim current controversy and uncertainty for the MWP*

Of the 51 references, 16 are also found in the WR, and of those 8 were Summarized. The color categorizations were done quickly, but I found more red ones than I would expect for a statistics journal paper. As in the WR, Bibliography oddness hints at possible help from unacknowledged others, especially considering the relative obscurity of some references.

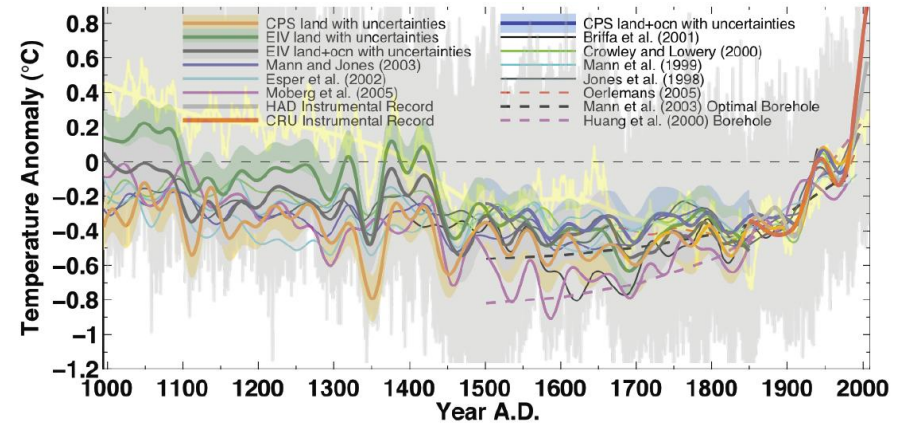
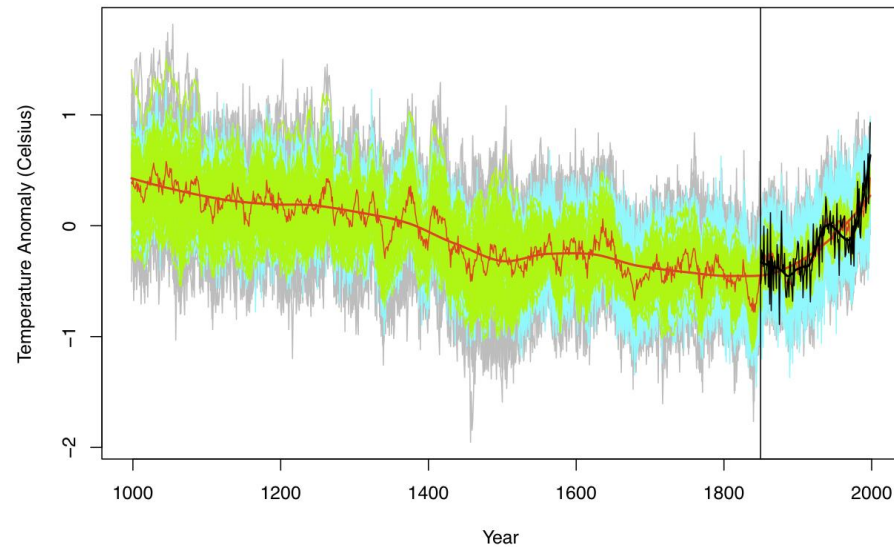
WR #	MCS #	W.10.2(b) Bibliography Tally	MW Page Read	Cited on pages	Link#	Source Relevant Bias Memes Themes	Source
4	26	Mann, Rutherford (2002) Climate...pseudoproxies	43	2	6,16		Geophysical Res. Letters
4	43	27 Mann, Rutherford, Wahl, Amman (2005) Testing	43 *	1	6	m	J of Climate
4	28	Mann, Rutherford, Wahl, Amman (2007) Robustness	43	3	6,25		J Geophys Res
4	29	Mann, Zhang, Hughes, et al (2008) Proxy-based...	43 *	17	2,4,6,7,9,16,32,36,37,38		PNAS
3	30	Matthes (1939) Report of committee on glaciers	43	1	27		A Trans AGU
1	44	31 McIntyre, McKittrick (2003) MM03	43 *	1	5	m	G B E&E
1	45	32 McIntyre, McKittrick (2005a) MM05a	43 *	3	5,6,19	m	G B E&E
1	46	33 McIntyre, McKittrick (2005b) MM05b	44 *	1	5	m	Geophysical Res. Letters
1	34	McIntyre, McKittrick (2005c) Reply to Huybers	44	2	6,19	m	Geophysical Res. Letters
4	35	NRC (2006)	44 *	9	2,4,6,7,13,25,31		North NRC [NRC2006]
4	36	Phillips (1986) spurious regressions in econometrics	44	1	9	m	J Econometrics
3	37	Rothstein (10/17/08), Apocalypse Now, via Diorama	44 *	1	3		G N New York Times
4	49	38 Rutherford, et al (2005)	44 *	1	6	m	J of Climate
4	39	Schneider (2001)	44	1	25		J of Climate
4	40	Tibshirani (1996) Regression...via lasso	44	1	13		J. R. Statist. Soc.
4	41	Tingley, Huybers (2010a)	44	3	33		J of Climate
4	42	Tingley, Huybers (2010b)	44	2	33		J of Climate
4	53	43 von Storch, Zorita (2005)	44	1	18	m	Geophysical Res. Letters
4	54	44 von Storch, Zorita, Jones, et al (2004)	44 *	3	6,16,17	m	Science
4	55	45 von Storch, Zorita, Jones, et al (2006)	44 *	1	18	m	Science
4	56	46 Wahl, Ammann (2006)	44 *	2	6,16	m	Climatic Change
4	57	47 Wahl, et al (2006) (note: really 2007)	44 *	2	6,18	m	Science
1	48	Wegman, Scott, Said (2006)	44 *	3	6,41	m	G WR or [WEG2006]
1	49	Wegman (2006) reply to Stupak	44 *	2	6,41	m	G [WEG2006c, SHA2006]
4	50	Wu, et al (2007)	44	1	22		J ASA
4	51	Yule (1926) nonsense correlations	44	1	9	m	A? J Royal Stat Soc
	16	51 Totals/Avg.		124			11 6 6 0 3 Counts

A.12.4 Comments on text

As elsewhere, comments here avoid serious statistics in favor of general scholarship issues more accessible to most people. When the final paper appears, discussions will be attached, presumably by good statisticians. [DEE2010q] gives a much more detailed analysis (16 pages) on MW sections 1 and 2, showing many more problems than the quick sample here.

MW at least offers much more serious statistical analysis than the WR, but likewise includes:

- Technical discussion accessible only to a relatively small audience.
- Other more accessible material, often suitable even for WSJ OpEd sound bites, not necessarily very well-supported.⁴⁷ Like the WR, it can act as a meme-carrier.
- Graphs are very powerful as they often propagate widely, are easy to understand and can get promulgated independent of the underlying analysis. The key WR graphs are probably Figures 4.5, 5.3, and 5.8. MW Figures 16 (p.36, shown below) looks like a hockey stick, but with a high MWP and large uncertainty limits.



MW Figure 17 (p.37) above is interesting, as their reconstruction (the almost invisible yellow line, red in previous graph) manages to be warmer almost everywhere, even in the 1600s where most reconstructions claim that it was cool, and certainly from 1000AD-1400AD. They expand the error bars so nothing can be concluded. They also raise the pre-1450 line enough to bring the MWP back quite strongly, Meme-j, Meme-56.

In light of the physics and forcing descriptions [IPC2007, pp.476-480], I think that the MW curve is very implausible, and I would guess that serious statistics errors will be found. But for now, some excerpts follow, using same <eEbB> codes as in other parts of the report to signal Errors and Biases, or combinations.

MW p.1, Paragraph 1

“Predicting historic temperatures based on tree rings, ice cores, and other natural proxies is a difficult endeavor. The relationship between proxies and temperature is weak and the number of proxies is far larger than the number of target data points. Furthermore, the data contain complex spatial and temporal dependence structures which are not easily captured with simple models.”

The above might be loosely derived from:

WR, p.3

“MBH98 and MBH99 focus on simple signal plus superimposed noise models for paleoclimate temperature reconstruction. Because of complex feedback mechanisms involved in climate dynamics, it is unlikely that the

⁴⁷ Once again, the Cheshire cat disappears, but the grin may remain.

temperature records and the data derived from the proxies can be adequately modeled with a simple temperature signal with superimposed noise.”

“Predicting historic temperatures” *although not wrong, is slightly odd wording.* “Reconstructing past temperatures” makes more sense.

<EB> “relationship between ... data points.”

MW do not demonstrate the expertise or evidence to make such a strong, all-encompassing claim. Some proxies are weak, some are strong, but of course, all are local. The second part only makes sense if one thinks that a proxy is supposed to respond directly to global (or NH) temperature.

MW p.1, Paragraph 2

“In this paper, we assess the reliability of such reconstructions and their statistical significance against various null models. We find that the proxies do not predict temperature significantly better than random series generated independently of temperature. Furthermore, various model specifications that perform similarly at predicting temperature produce extremely different historical backcasts. Finally, the proxies seem unable to forecast the high levels of and sharp run-up in temperature in the 1990s either in-sample or from contiguous holdout blocks, thus casting doubt on their ability to predict such phenomena if in fact they occurred several hundred years ago.”

 “We find that the proxies do not predict temperature significantly better than random series...” *The reader might think about whether this makes any sense or not. An entire well-established field is overturned by newcomers. This can happen, but is certainly rare, and wins prizes when it does. But more often, this happens when serious experts get new ideas. Alternatively, MW methods might be terribly flawed.*

It does offer a good sound bite, quickly quoted by the WSJ, **A.12.1.**

MW p.1, Paragraph 3

“We propose our own reconstruction of Northern Hemisphere average annual land temperature over the last millennium, assess its reliability, and compare it to those from the climate science literature. Our model provides a similar reconstruction but has much wider standard errors, reflecting the weak signal and large uncertainty encountered in this setting.”

 “weak signal and large uncertainty encountered in this setting.”

Again, this is an assertion by neophytes using new methods, whose paleoclimate introduction comes from Wikipedia and the WR, Meme-j.

MW p.1-2, Paragraph 4 is covered in **A.12.2.**

MW p.2, Paragraph 3

“This effort to reconstruct our planet’s climate history has become linked to the topic of Anthropogenic Global Warming (AGW). On the one hand, this is peculiar since paleoclimatological reconstructions can provide evidence only for the detection of AGW and even then they constitute only one such source of evidence. The principal sources of evidence for the detection of global warming and in particular the attribution of it to anthropogenic factors come from basic science as well as General Circulation Models (GCMs) that have been fit to data accumulated during the instrumental period (IPCC, 2007). These models show that carbon dioxide, when released into the atmosphere in sufficient concentration, can force temperature increases.”

This paragraph is at best confused and casts serious doubt on MW familiarity with the field. In the 2nd version, **A.12.8.1**, “AGW” is replaced by “global warming,” perhaps following the discussion of **A.2.**

“effort ... has become linked” *is very odd.* Climate research has included paleoclimate studies for many years, as MW should know from their citation of Lamb.

<E> “this is peculiar since paleoclimatological reconstructions can provide evidence only for the detection of AGW”

This is wrong. [IPC2001, Ch.12, IPC2007, Ch.9 chapters on Detection and Attribution discuss uses of paleoclimate data. It is not obvious that MW ever read these.

<EB> “principal sources of evidence ... basic science as ... (GCMs) that have been fit to data accumulated during the instrumental period (IPCC, 2007).”

A good short explanation of this common error is found at:

www.realclimate.org/index.php/archives/2008/11/faq-on-climate-models

Many statistics studies are fits to data, but GCMs are physics models, and those unfamiliar with them sometimes do not understand the difference.⁴⁸ After quoting the previous MW paragraph, [ZOR2010] writes::

⁴⁸ See also the footnote for Anderson, et al (2005), in **W.8.8.**

“Although climate models contain parameters that may be tuned, climate models are not really fit to observations. If that were the case, the models would all reproduce perfectly the observed global trend.”

In that same MW paragraph above, the (IPCC, 2007) citation seems likely to be fabricated.: Issue-f⑥:

- This is the only citation of (IPCC, 2007), odd in itself.
- That is a dense, 1,000-page document. MW cite it in support of a claim the IPCC seems very unlikely to make. It is even hard to think of anything that could be credibly be interpreted this way. A page, or even a section number would be helpful.⁴⁹ The most relevant part is - §8.1.3, p.596, and it does not support the MW comment.
- In any case, I have a PDF of the IPCC report, looked at every hit for “fit” or “GCM” and find nothing that supports MW.

MW does not seem to understand even the most basic idea of GCMs.

In any case, people might review §1.5, for reasons why people keep attacking the shaft of the stick and then try to extend that to GCMs. The sketch at lower right there is relevant.

All this could be lack of knowledge or it could be more subtle logic:

- “basic science” is a vague, forgettable phrase.
- That leaves models to show that CO2 can force temperature changes.
- Most of MW is spent casting doubt on “models,” although the term is used in the general sense, not the normal climate science terminology that distinguishes between reconstructions (MBH908, etc) and physics models (GCMs).

Paleoclimate data, modern data and GCMs fit the specific basic physics, Theme-B④, Theme-C④, both at least 100+ years old. One does not need GCMs to know that conservation of energy and greenhouse effect are real. Arrhenius got a fair approximation with paper-and-pencil in 1896, §1.3.

⁴⁹ I know page numbers are not required, but when making a specific contentious claim, as opposed to citing a general text for background, one would like a page or at least a section number, and editors might think about this. MW has Bradley (1999), IPCC (2001), IPCC (2007), Ladorie (1971), Lamb (1990), and some might be fabrications.

MW, p.2, Paragraph 3

“On the other hand, the effort of world governments to pass legislation to cut carbon to pre-industrial levels cannot proceed without the consent of the governed and historical reconstructions from paleoclimatological models have indeed proven persuasive and effective at winning the hearts and minds of the populace. Consider Figure 1 which was featured prominently in the Intergovernmental Panel on Climate Change report (IPCC, 2001) in the summary for policy makers¹. The sharp upward slope of the graph in the late 20th century is visually striking, easy to comprehend, and likely to alarm. The IPCC report goes even further:”

 “world governments ... consent of the governed”

When one Googles the 6 words above,⁵⁰ many hits espouse strong conservative/Libertarian political views. Those are fine in the political arena, but not in statistics papers people expect to be credible. From past experience,⁵¹ strong political/ideological beliefs can cause a few physics PhDs to ignore basic laws of physics.

“featured prominently ... alarm”

See WR p.7, first paragraph, which may help explain why MW discuss (IPCC, 2001), not (IPCC, 2007). The above is likely drawn from the WR, using a few key words.

MW, p.3, Paragraph 1

“Quotations like the above and graphs like those in Figures 1, 2, and 3 are featured prominently not only in official documents like the IPCC report but also in widely viewed television programs (BBC, September 14, 2008), in film (Gore, 2006),⁵² and in museum exhibitions (Rothstein, October 17, 2008), alarming both the populace and policy makers.”

⁵⁰ “World government” and “consent of the governed” are “code words,” related to “dog-whistle politics,” i.e., they have specific extra meaning for some group. en.wikipedia.org/wiki/Dog-whistle_politics

Most thinktanks active in climate anti-science claim conservative or Libertarian political views, [MAS2010], and non-acceptance of AGW science is strongly correlated with this political view. “Alarm” or “alarmist” are also code words.


⁵¹ See [MAS2009], in which many physicists signers of a climate anti-science petition to the American Physical Society showed political leanings.

⁵² [MAS2010] Figure 2.6 lists possible reasons for climate anti-science, one of which is POL2 “Against: Cannot stand X, so anything they say is wrong.”

 Why are these in a statistics paper?

MW, p.3, Paragraph 2, continued p.5

“This sets up the erroneous visual expectation that the reconstructions are more accurate than they really are. A careful viewer would know to temper such expectations by paying close attention to the reconstruction error bars given by the wide gray regions. However, even these are misleading because these are, in fact, pointwise confidence intervals and not confidence curves for the entire sample path of surface temperature. Furthermore, the grey regions themselves fail to account for model uncertainty.”

 Arguments about uncertainty are common, but MW seems to expand uncertainty limits well beyond physical explanation, Meme-j .

MW, p.5, Paragraph 1

“**2. Controversy.** With so much at stake both financially and ecologically, it is not surprising that these analyses have provoked several controversies. While much of this has recently erupted in the popular press (Jolis (November 18, 2009), Johnson (November 23, 2009), Johnson and Naik (November 24, 2009)), we root our discussion of these controversies in the history as it unfolded in the academic and scientific literature.”

 Why does a statistics paper feature Climategate articles from the WSJ, then claim to study academic and scientific literature?

MW, p.5, Paragraph 2

“The first major controversy erupted when McIntyre and McKittrick (M&M) successfully replicated the Mann et al. (1998) study (McIntyre and McKittrick, 2003, 2005b,a). M&M observed that the original Mann et al. (1998) study (i) used only one principal component of the proxy record and (ii) calculated the principal components in a ”skew”-centered fashion such that they were centered by the mean of the proxy data over the instrumental period (instead of the more standard technique of centering by the mean of the entire data record). Given that the proxy series is itself auto-correlated, this scaling has the effect of producing a first principal component which is hockey-stick shaped (McIntyre and McKittrick, 2003) and, thus, hockeystick shaped temperature reconstructions. That is, the very method used in Mann et al. (1998) guarantees the shape of Figure 1. M&M made a further contribution by applying the Mann et al. (1998) reconstruction methodology to principal components computed in

It also mentions “Al Gore” as a common X. Without making any comment about politics here, it is quite common in climate anti-science writings to insert irrelevant references to Gore to create negative views.

the standard fashion. The resulting reconstruction showed a rise in temperature in the medieval period, thus eliminating the hockey stick shape.”

<EB> “The first major controversy erupted”

It “erupted” as a multi-year campaign that started before McIntyre was even involved, driven by Washington thinktanks and key politicians (TT+CO). See the Executive Summary, §1, **W.8.7**, [MAS2010, §5]. While scientists always argue over exact shapes and uncertainty limits, they do so within normal scientific frameworks. Of the 3 cited MM papers, 2 were published in E&E, a social sciences journal of poor reputation. As discussed elsewhere, decentering is wrong, but it made little difference, and in any case, this passage has more problems.

<EB> “successfully replicated the Mann, et al (1998)”

No, they did not, and the passage is very confused about the history, partly by citing the 3 MM papers together. The MM03 reconstruction created a problematical temperature spike in the 1400s, **W.4.4**. The decentering/skew-centered issue was not discovered until 2004, published in MM05a. Some of this discussion seems drawn more from the WR or blogs than from the actual papers themselves, **W.3** and MM03, MM05a, MM05b in **W.11.8**.

<EB> “used only one principal component of the proxy record”

[DEE2010n] says it well:

“This is nonsense – the famous PC1 was the leading principal component of one proxy sub-network (North American tree rings) for one period of time (the 1400 step that represents the start of the original MBH98 reconstruction). And even for that sub-network, two PCs were used, not one.”

as does [ZOR2010]:

“This paragraph, and later other similar paragraphs, tells me that the authors have not really read the original paper by Mann, Bradley and Hughes (1998). MBH never used 'only one principal component of the proxy record'. The authors, again, are probably confused by what they may have read in blogs.... **But other that, the MBH reconstruction is not based on 'principal components of the proxy record'**. It is based on the principal components of *the observed temperature field*. For the millennial reconstruction, MBH estimate that only one PC *of the instrumental temperatures* could be reconstructed. They never used 'only one principal component of the proxy record', they did use only the first principal components of the US Southwest tree-ring network, but not of the 'proxy record'. For instance, for the first part of the millennial reconstruction 1000-1400, MBH used an inverse regression

method with 12 proxy indicators and one principal component of the temperature field. **This point is so clear in the MBH paper that it really shows that McShane and Wyner actually did not read MBH98.**"

And in fact, MBH98 has:

p.781:

"We isolate the dominant patterns of the instrumental surface temperature data through principal component analysis²⁵ (PCA). PCA provides a natural smoothing of the temperature field in terms of a small number of dominant patterns of variability or 'empirical eigenvectors.'"

p.786 describes the method.

This looks like fabrication, Issue-F₂Ⓞ.

MW, p.5, Paragraph 2

"Mann and his colleagues vigorously responded to M&M to justify the hockey stick (Mann et al., 2004). They argued that one should not limit oneself to a single principal component as in Mann et al. (1998), but, rather, one should select the number of retained principal components through crossvalidation on two blocks of heldout instrumental temperature records (i.e., the first fifty years of the instrumental period and the last fifty years). When this procedure is followed, four principal components are retained, and the hockey stick re-emerges even when the PCs are calculated in the standard fashion. Since the hockey stick is the shape selected by validation, climate scientists argue it is therefore the correct one."

[DEE2010n] says:

"Mann et al (2004) is the Corrigendum to MBH98 which fixed some data listing errors (without affecting the actual data or findings). But there was no reference to a changed PCA methodology; there could not have been as the Corrigendum was issued in March 2004, while the differing centering conventions were only identified much later that year! But there was a further explanation of the original PCA methodology, whereby the number of PCs retained for each proxy sub-network at each "step" interval was based on objective criteria combining "modified Preissendorfer Rule N and screen test". (In fact, Mann's methodology involved rebuilding the network with fewer and fewer proxies as one goes back, requiring recomputation of PCA for each large sub-network at each interval)."

The Mann, et al (2004) Corrigendum is short:

www.meteo.psu.edu/~mann/shared/articles/MBH98-corrigendum04.pdf

DC goes on to say in a thread with many other useful comments:

deepclimate.org/2010/08/19/mcshane-and-wyner-2010/#comment-5297

"But the question remains: Where did M&W get the idea that Mann et al 2004 addressed short-centred PCA, when in fact it preceded the raising of that issue?"

In 2004, McIntyre and McKittrick submitted a comment to Nature, raising the issue of short-centred PCA. In their reply, Mann et al asserted that application of the same selection/retention criteria as MBH98 in M&M's particular conventional PCA implementation would result in retention of 5 PCs for the North American 1400 tree ring network, resulting in much the same "hockey stick" reconstruction.

Neither the comment nor reply was ever published, but were described at great length at ClimateAudit. In fact, McIntyre has accused Wahl and Ammann of plagiarizing their Climatic Change article from the Mann et al unpublished reply...." The file DC cites is:

www.climateaudit.info/pdf/mann/2004/Mann%20Reply.pdf

So, the Mann, et al (2004) citation is clear fabrication, Issue-F₃Ⓞ, likely covering Link-mⓄ, and it reappears later.

MW, p.5, Paragraph 3, p.6

"The furor reached such a level that Congress took up the matter in 2006. The Chairman of the Committee on Energy and Commerce as well as the Chairman of the Subcommittee on Oversight and Investigations formed an ad hoc committee of statisticians to review the findings of M&M. Their Congressional report (Wegman et al., 2006) confirmed M&M's finding regarding skew-centered principal components (this finding was yet again confirmed by the National Research Council (NRC, 2006))."

<EB> "furor reached such a level"

As noted earlier, it was a multi-year campaign driven by Washington thinktanks and key politicians (TT+CO). See the Executive Summary, §1, **W.8.7**, or [MAS2010, §5]. The furor was not part of science, but the same sort of controversy manufactured by helpers of the tobacco industry.

<EB> "this finding was yet again confirmed by the National Research Council (NRC, 2006)"

MW writes in such a way that casual reader might think the NRC2006 confirmed the WR in general. This gets history backwards, since the NRC Report preceded the WR by 3-4 months. It did agree with the MM finding on skew-centered components, but not much else.

MW, p.6, Paragraph 2

“In his Congressional testimony (Wegman, 2006), committee chair Edward Wegman excoriated Mann et al. (2004)’s use of additional principal components beyond the first after it was shown that their method led to spurious results:

‘In the MBH original, the hockey stick emerged in PC1 from the bristlecone/foxtail pines. If one centers the data properly the hockey stick does not emerge until PC4. Thus, a substantial change in strategy is required in the MBH reconstruction in order to achieve the hockey stick, a strategy which was specifically eschewed in MBH...a cardinal rule of statistical inference is that the method of analysis must be decided before looking at the data. The rules and strategy of analysis cannot be changed in order to obtain the desired result. Such a strategy carries no statistical integrity and cannot be used as a basis for drawing sound inferential conclusions.’

<EB> “Wegman excoriated Mann et al. (2004)’s use of additional principal components”

In addition to using language not often found in statistics papers, this passage manages 3 distinct fabrications based on [WEG2006c, pp.13-14]:

First, [WEG2006c] never mentions Mann, et al (2004), Issue-F₄⑥.

Second, as noted earlier, Mann, et al (2004) did not discuss centering, so even if Wegman had mentioned it, MW would be wrong, Issue-F₅⑥.

It seems likely that MW got this from some tertiary source, did not check the secondary source [WEG2006c], much less the primary source.

 Third, as [DEE2010n] says of “eschewed in MBH...a cardinal rule” “always check the ellipsis”

MW has the ellipsis, but the actual text from [WEG2006c, p.14] is: “specifically eschewed in MBH. In Wahl and Ammann’s own words, the centering does significantly affect the results.

~~e. Dr. Gullede included in his testimony a slide showing the graph of WA emulation of the MBH and MBH corrected for decentering and the Gaspe tree-ring series. Were you aware of their reanalysis of MBH99 prior to the time you~~

~~finalized your report? Do you agree or disagree with their reanalysis of MBH99? If you disagree, please state the basis for your disagreement.~~

~~Ans: Yes, we were aware of the Wahl and Ammann simulation. We continue to disagree with the reanalysis for several reasons. Even granting the unbiasedness of the Wahl and Ammann study in favor of his advisor’s methodology and the fact that it is not a published refereed paper, the reconstructions mentioned by Dr. Gullede, and illustrated in his testimony, fail to account for the effects of the bristlecone/foxtail pines. Wahl and Ammann reject this criticism of MM based on the fact that if one adds enough principal components back into the proxy, one obtains the hockey stick shape again. This is precisely the point of contention. It is a point we made in our testimony and that Wahl and Ammann make as well. A cardinal rule of statistical inference is that the method of analysis must be decided before looking at the data. The rules and strategy of analysis cannot be changed in order to obtain the desired result. Such a strategy carries no statistical integrity and cannot be used as a basis for drawing sound inferential conclusions.”~~

[DEE2010n] says:

“So this passage has nothing whatsoever to do with the Mann corrigendum, but rather is a discussion of” (Wahl, Amman (2006)).... Here Wegman is attempting to claim that Wahl and Ammann acknowledge that the differing numbers of principal components is itself a “change in strategy”. But this is a gross misrepresentation of Wahl and Ammann’s point, which was that an objective criterion is required to determine the number of PCs to be retained and that number will vary from sub-network and period, as well as centering convention. M&M arbitrarily selected only two because that’s what Mann had done at that particular step and network. They failed to implement Mann’s criterion (as noted previously), or indeed any criterion, and thus produced a deeply flawed reconstruction.”

So, MW makes a bad discussion⁵³ by Wegman of Wahl, Ammann (2006) read like an attack on Mann, et al (2004), which had nothing to do with this. They excise several paragraphs, change “A cardinal rule...” to “a cardinal rule ...,” placing it immediately following MBH, misleading even a careful reader to think this part of the same sentence, Issue-F₆⑥.

MW, p.6, paragraph 3

⁵³ Wegman misrepresents Wahl, Ammann (2006), which is clear that decentering makes a difference, but very little.

“The degree of controversy associated with this endeavor can perhaps be better understood by recalling Wegman’s assertion that there are very few mainstream statisticians working on climate reconstructions (Wegman et al., 2006).”

 This is just repeated, despite multiple refutations otherwise, Meme-f1, Link-W5. The “controversy” was not within science, but outside it.

MW, p.9, paragraph 1

“The problem of spurious correlation arises when one takes the correlation of two series which are themselves highly autocorrelated and is well studied in the time series and econometrics literature (Yule, 1926; Granger and Newbold, 1974; Phillips, 1986).”

This is true, and these are certainly well-cited papers, so are not labeled as fabrications, but I was struck by an odd coincidence. McIntyre’s website references these 3 papers often, and posted 02/10/08:

climateaudit.org/econometric-references

MW, p.19, paragraph

“It is easily observed that short term future temperatures can be predicted by estimates of the local mean and its first derivatives (Green et al., 2009).”

See discussion of Armstrong, A.12.5. The 3 authors are Green (forecaster), Armstrong (forecaster), and Soon (astrophysicist) , all well-engaged with MM+TT. See A.12.5 for further comments on them and this paper,

MW, p.19, paragraph

“It is well known and generally agreed that the several hundred years before the industrial revolution were a comparatively cool “Little Ice Age” (Matthes,1939; Lamb, 1990). What happened in the early Medieval period is much more controversial and uncertain (Ladurie, 1971; IPCC, 2001).”

<EB> This has many problems . It cites 3 famous references, all long outdated except as climate science history.

Lamb (1990) is likely older than it looks, as MW reference:

“Lamb, H. H. (1990). *Climate: Past, Present and Future*. Routledge.”

But Amazon’s image of that book says:

“ First published in 1972 by Methuen, reprinted by Routledge, 2001.”

www.amazon.com/Climate-Present-Future-H-Lamb/dp/0416115306/ref=sr_1_fkmr1_2?ie=UTF8&qid=1284091320&sr=8-2-fkmr1#reader_0416115306

MW cite references from 1939, (1990, but likely reprint of 1972), and then add 1971, and IPCC 2001, They write:

“is much more controversial and uncertain.”

That is a strange definition of “is.”

[IPC2001, pp.133-136] does not support MW, and [IPC2007, pp.468-467-483 supports it less.⁵⁴

All this is Theme-A4, Theme-G4, in support of Meme-j1, Meme-561.

One may plausibly doubt that MW have read the older sources, and I think they have essentially misrepresented [IPC2001], Issue-f6.

MW, p.36, paragraph 3

“FIG 16. Backcast from Bayesian Model of Section 5. ...

The major difference between our model and those of climate scientists, however, can be seen in the large width of our uncertainty bands. Because they are pathwise and account for the uncertainty in the parameters (as out”

MW, p.37, (cont)

“lined in Section 5.3), they are much larger than those provided by climate scientists. In fact, our uncertainty bands are so wide that they envelop all of the other backcasts in the literature. Given their ample width, it is difficult to say that recent warming is an extraordinary event compared to the last 1,000 years. For example, according to our uncertainty bands, it is possible that it was as warm in the year 1200 AD as it is today.”

Meme-j1, Meme-561

MW, p.39

“As mentioned earlier, scientists have collected a large body of evidence which suggests that there was a Medieval Warm Period (MWP) at least in portions of the Northern Hemisphere. The MWP is believed to have occurred from c. 800-1300 AD (it was followed by the Little Ice Age).”

Once again, the MWP returns, Meme-561.

MW, p.41, Paragraph 1

⁵⁴ One might just as well say “there is controversy and uncertainty about the relationship of smoking and disease.” MM+TT+CO and WP certainly wanted the MWP to be warmer than today, W4.2, but their opinions are irrelevant to science.

“While the literature is large, there has been very little collaboration with university level, professional statisticians (Wegman et al., 2006; Wegman, 2006). Our paper is an effort to apply some modern statistical methods to these problems.”

 This is just repeated, despite multiple refutations, Meme-f¹⁶, Link-W⁶, with the only authority being Wegman. *There is room for doubt.*

MW, p.41, Paragraph 1

“While our results agree with the climate scientists [sic] findings in some respects, our methods of estimating model uncertainty and accuracy are in sharp disagreement.

On the one hand, we conclude unequivocally that the evidence for a “long-handled” hockey stick (where the shaft of the hockey stick extends to the year 1000 AD) is lacking in the data.”

Meme-j¹.

MW, p.42, Paragraph 1

“Climate scientists have greatly underestimated the uncertainty of proxybased reconstructions and hence have been overconfident in their models.

Natural climate variability is not well understood and is probably quite large. It is not clear that the proxies currently used to predict temperature are even predictive of it at the scale of several decades let alone over many centuries. Nonetheless, paleoclimatological reconstructions constitute only one source of evidence in the AGW debate.

Our work stands entirely on the shoulders of those environmental scientists who labored untold years to assemble the vast network of natural proxies.

Although we assume the reliability of their data for our purposes here, there still remains a considerable number of outstanding questions that can only be answered with a free and open inquiry and a great deal of replication.”

“Natural climate variability is not well understood” Certainly, this paper shows little evidence of understanding. Possibly climate scientists know a bit more, although of course it is hardly perfectly understood, Meme-j¹.

“AGW debate” What AGW debate do they mean? Meme-03¹. Perhaps they mean the same kind of debate that tobacco companies have with medical researchers?

As with the WR, few people are likely to read or follow the statistical arguments and discussion of proxies. Takeaway messages are wrapped

around those, mostly found at the front and back of the paper, often packaged as easy sound bites.

A.12.5 Background, possible connections

Past studies that have often highlighted a strong climate anti-science social network, [MAS2009, MAS2010]. In practice, efforts like the WR and MW rarely appear with no connection to that network. It is always worth exploring the surrounding social network to explore possible connections. MW says:

“**Acknowledgements.** We thank Editor Michael Stein, two anonymous referees, and Tilmann Gneiting for their helpful suggestions on our manuscript. We also thank our colleagues Larry Brown and Dean Foster for many helpful conversations.”

Brown and Foster are both Professors of Statistics at Wharton:

www.wharton.upenn.edu/faculty/brown.cfm (a NAS member)

www.wharton.upenn.edu/faculty/foster.cfm

[www-](http://www-stat.wharton.upenn.edu/statweb/Faculty/Faculty.html#StandingFaculty)

[stat.wharton.upenn.edu/statweb/Faculty/Faculty.html#StandingFaculty](http://www-stat.wharton.upenn.edu/statweb/Faculty/Faculty.html#StandingFaculty)

Judith Curry labeled the authors as “leading statisticians”:

www.collide-a-scape.com/2010/08/04/gavins-perspective/#comment-14404

But it is always worth checking, so I did.

Blakeley B. McShane

w4.stern.nyu.edu/emplibrary/B%20McShane%20CV.pdf

www.blakemcshane.com/cv.pdf says:

“Blake McShane is a faculty member in the Marketing Department of the Kellogg School of Management at Northwestern University. As a statistical methodologist, he has developed models for problems in a variety of fields including internet advertising, sleep- and neuro-science, **paleoclimatology**, law, and baseball. His specific research interests include Bayesian hierarchical modeling, statistical learning, and generalized Markov models. More generally, he seeks to develop statistical methods to accommodate the rich and varied data structures encountered in business problems and to use these methods to glean insight about individual behavior so as to test and supplement existing theories.”

“Education:

The Wharton School, University of Pennsylvania May 2010

Ph.D. in Statistics

Thesis: Machine Learning Methods With Time Series Dependence

Advisor & Committee: Abraham Wyner, Eric Bradlow, Shane Jensen and Abba Krieger” (Bradlow was Marketing Advisor)”

He has an eclectic mix of publications, nothing previous in climate:

“Publications:Kiser, R., Asher, M., and McShane, B. B. (2008), “Let’s Not Make a Deal: An Empirical Study of Decision Making in Unsuccessful Settlement Negotiations.” *Journal of Empirical Legal Studies*, Vol. 5, No. 3, 551-591.McShane, B., Adrian, M., Bradlow, E.T., and Fader, P.S. (2008), “Count Models Based on Weibull Interarrival Times.” *Journal of Business and Economic Statistics*, Vol. 26, No. 3, 369-378.Jensen, S.T., McShane, B., and Wyner, A.J. (2009), “Hierarchical Bayesian Modeling of Hitting Performance in Baseball.” *Bayesian Analysis*, Vol. 4, No. 4, 631-652.Jensen, S.T., McShane, B., and Wyner, A.J. (2009), “Rejoinder: Hierarchical Bayesian Modeling of Hitting Performance in Baseball.” *Bayesian Analysis*, Vol. 4, No. 4, 669-674.McShane, B. (2009), “Exploring a New Method for Classification with Local Time Dependence.” *Transactions of the Deming Conference on Applied Statistics*.Piette, J., Braunstein A., McShane, B. B., and Jensen, S. T. (2010) “A Point-Mass Mixture Random Effects Model for Pitching Metrics.” *Journal of Quantitative Analysis in Sports*, Vol. 6: Iss. 3, Article 1.

“Affiliations:

American Marketing Association
Institute for Mathematical Statistics
American Statistical Association”

Wyner organized this session and McShane gave a talk they coauthored:

“Are Reconstructions of Surface Temperatures Over the Last 1000 Years Reliable?” Presented February 2009 at Information Theory and Applications Workshop, San Diego, CA.”

ita.ucsd.edu/workshop.php?submitted=1ita.ucsd.edu/workshop/09/talksThis large conference spans a huge range of topics, although not generally climate, so it seems unlikely that many of the attendees would be familiar with the topic. *This is slightly strange. When doing a PhD, usually one is not giving talks on topics far removed from one’s dissertation area.***Abraham J. Wyner**www.wharton.upenn.edu/faculty/wyner.cfmwww-stat.wharton.upenn.edu/~ajw/pubwyner.html papers through 2003
priml.upenn.edu/People/AdiWyner

He is an Associate Professor of Statistics at Wharton.

“Research Areas

Probabilistic modeling; information theory; entropy; data compression; estimation

Current Projects

The thrust of current research projects is the theoretical development of probabilistic models from information theory towards applications in statistics.”

He has been at Wharton since 1998. No climate-related publications were found, but he has done a few talks recently. Like Wegman, who often gave talks to audiences unlikely to have expertise, he spoke, March 2010:

stat.wharton.upenn.edu/~gadam/seminar_files/Abraham%20Wyner%20-%20Title%20and%20Abstract.pdfHe contributed (as "Adi") to a group blog, now dormant, occasionally touching upon climate, as did Dean Foster. Shane Jensen was also listed: picstat.blogspot.com “Politically Incorrect Statistics”

Following posts are by “Adi” unless otherwise noted.

picstat.blogspot.com/2005/11/greenhouse-gases-increasing-but-still.html (Dean Foster) 11/25/05“One of the motivator for this blog was the battle over greenhouse gases. Well, it appears that the **greenhouse worry warts** finally have some convincing evidence on their side. Still to be addressed are

- * what the implications will be
- * what the economics choices are
- * is the benefit of doing something larger than the cost.

This last question is the line that I personally draw in the stand. If the benefit is 100 years away, we need almost a 100 times return before it is a positive value project. There are so many other things that sound more important, that **I truly have trouble getting excited about this one.**”picstat.blogspot.com/2006/08/global-warming-in-pictures.html (Dean Foster)“Hence this represents the "average" belief of the internet. Probably not a good way to do science, but **a pretty cool way to generate graphs about a cause.**”picstat.blogspot.com/2008/05/southern-hemisphere-sea-ice.html 05/01/08

“On the news page of the National Snow and Ice Data Center (NSIDC) there is an article about the possible collapse of the Wilkins Ice Shelf

The article is all very interesting, but I wanted more information since I am not sure why we should care particularly about one ice shelf that is comparatively small. I was surprised to discover that this March has seen the [largest increase in Southern Hemisphere sea ice](#) since we started measuring:

So which is it? Is global warming causing BOTH the melting of the Wilkins AND the increase in total Southern Hemisphere Ice? How on earth can the NSIDC justify such a misleading headline on their news page? A better headline would be "**Antarctic Sea Ice increase undermines Warming World**"

picstat.blogspot.com/2008/05/sea-ice-continued.html 05/04/08

More on sea ice.

picstat.blogspot.com/2008/07/vanishing-temperature-trends.html 07/28/08

“Back in 1975, when we are at the end of a 30 year period of declining global temperatures, the consensus among the climate scientists was a coming ice age. How they could have come 180 degrees in such a short time frame is another story. Suffice it to say that forecasting is difficult, especially of the future. **I have been trying to figure out how global average temperature data comes to be. I know that there has been attempts to revise the historically record. I think the revisions have been really huge.** Consider the graph from Newsweek Magazine, April 28, 1975, page 64:” (chart is as follows):

www.denisdutton.com/newsweek_coolingworld.pdf

“Now this is a smoothed version but it shows that the peak temperature anomaly of .9 degrees (compared to 1880) occurred in the mid 1940s. By 1970 the anomaly was down to less than .25 degrees. ...

The Red line is the smooth version of the average global temperature. Notice how the huge decline from the mid 1940s to the mid 1970s has.....
DISAPPEARED!!!”

Wyner worries about 1975 Newsweek. Zeke Hausfather (Yale) explains that NH and global temperatures are different.⁵⁵

www.yaleclimatemediaforum.org/2008/05/common-climate-misconceptions-solar-influences-on-global-temperature/

This Newsweek article is Gwynne (1975), referenced in the WR **The Newsweek chart is Fahrenheit, Hausfather’s chart is in Centigrade.** The not-well-labeled Newsweek chart may well just cover the USA, which had a sharper dip than the NH as a whole, unsurprising

⁵⁵ Cooling was due to sulfate emissions in NH, much larger than SH.

given land-vs-ocean and heavy industrial growth. So, nothing is odd, but this does not indicate even minimal knowledge for Wyner.

data.giss.nasa.gov/gistemp/graphs/

picstat.blogspot.com/2008/08/is-it-really-so-simple.html 08/27/08

Wyner seems not to believe Hausfather:

“On the other hand, it may be that the degrees of freedom can easily be manipulated to produce the result you want. As a professional statistician I am predisposed to the latter view. ...

“As you can see the post war cooling period that was so dramatic in the NCAR graph published in Newsweek has been flattened out. So here it is again: the trend, which is so dramatic in 1975, is no longer even part of the historical record. How can that be?

My sense from this blog is of a small group of colleagues, sometimes (perhaps deservedly) dismissive of people for poor statistical reasoning. None show obvious climate expertise, and when someone knowledgeable (as Hausfather certainly is) appears, people seem not to listen much. People mention common Memes (#8 [ice70s] and #10 [Antarctica])
www.skepticalscience.com/fixednum.php

Foster and Wyner seem to share skeptical attitudes towards climate science, but neither showed great knowledge in 2008. Perhaps McShane and Wyner have spent much time since mid-2008 studying this topic, because MW has many references not casually obtained overnight. One wonders if they have spent time consulting climate scientists, or like the WP, managed not to do that. In addition, just as P.Spencer was feeding references to the WP, perhaps MW were getting help.

This can be only speculation, but the MW reference to Green, Armstrong, and Soon (2009) might be a hint. All 3 are very active in climate anti-science advocacy efforts, Soon for many years. As it happens, Wyner and Armstrong work in the same building at Wharton:

J. Scott Armstrong, Professor of Marketing at Wharton

www.wharton.upenn.edu/faculty/armstroj.cfm

marketing.wharton.upenn.edu/documents/cv/Resume%20JSA7-13.pdf

He was the Founder or cofounder of the Intl. Institute of Forecasters:

www.forecasters.org

and its journal. *Int. J of Forecasting.*

The Editorial Board includes Kesten Green:

www.elsevier.com/wps/find/journaleditorialboard.cws_home/505555/editorialboard

Armstrong is quite active in climate anti-science. Had he been listed in [MAS02010], his entry would have been:

Armstrong, J. Scott, Wharton School, U Pennsylvania, Marketing
Activities: Heart#1, Manhat2008, Heart#2, CATO2009, NIPCC2009, HeartExp#1, EPA#1, Heart#4.

Organizations: Heartland, E&E

People: Singer (via NIPCC), MM (via Heartland conferences)

He would certainly have crossed paths with MM. It is at least conceivable that he encouraged MW.⁵⁶ Heartland is basically a PR/lobbying entity that grew with tobacco but has recently has grown into climate anti-science, and most core climate anti-science advocates are involved with it. The codes above show that Armstrong has spoken at 3 Heartland conferences, written a section of the NIPCC report edited by Singer for Heartland, is labeled a Heartland “expert,” signed petitions and published in E&E.

The 2009 paper by Green, Armstrong, Soon basically ignores all climate science to claim no year in the next century will be more than 0.5C warmer than 2008, and that their forecasts cannot be improved upon. The paper is: mpira.ub.uni-muenchen.de/13592/2/Naiveclimate.pdf

Just based on its 10 references alone, it is unimpressive:

- 2001 Book section by Armstrong
- 1992 paper by Armstrong
- 2008 online article in local newspaper.
- 2007 Green & Armstrong article in E&E, of which RealClimate commented rather strongly, and much applies as well to MW: www.realclimate.org/index.php/archives/2007/07/green-and-armstrongs-scientific-forecast
- 1990, and 1992 IPCC, rather obsolete

⁵⁶ The climate anti-science groups described in [MAS2010] have a long history of continually recruiting “new voices” as some older ones become too recognizable. This strategy was developed long ago for the cigarette companies.

- 2007 IPCC, finally, a relevant reference
- 2007 McKibben in New York Review of Books
- 2007 McKittrick and Michaels paper in J. Geophysical Research, on which RealClimate comments in detail, Theme-B⁴, Theme-C⁴. www.realclimate.org/index.php/archives/2007/12/are-temperature-trends-affected-by-economic-activity-ii
- 1990 article in New York Times

This paper would not survive peer review in any serious science journal, but Armstrong did help start it, ah published 70+ papers here, and Green is an Associate Editor:

www.sciencedirect.com/science?_ob=ArticleListURL&_method=list&_ArticleListID=1473633462&_sort=r&_st=13&view=c&_acct=C000050221&_version=1&_urlVersion=0&_userid=10&md5=0ec2ec919400da9ab51c39ad4db9402b&searchtype=a

One last connection might be relevant, see also **W.8.8**, papers #1, #2.

Bruce McCullough is at Drexel (~ 1 mile away)

www.pages.drexel.edu/~bdm25

“Associate Editor, International Journal of Forecasting (1999 –) Associate Editor, Computational Statistics and Data Analysis (2003 –) “ He would likely know Armstrong from the first and Wegman from the second. He coauthored a report with McKittrick for the Fraser Institute (Canadian ally of the Washington thinktanks). He coauthored the paper with extraneous MM material, **W.8.8**, paper #2, referenced in the WR. www.pages.drexel.edu/~bdm25/DueDiligence.pdf

Forecasting and backcasting

The 9-page paper by Green, Armstrong, Soon uses “forecast” 99 times. The 45-page MW uses “backcast” 64 times and “forecast” 30. “Forecasts” of the past seem odd. Paleoclimate reconstructions are not often labeled *casts, because they are extractions of signal from noise from past data, not use of climate models to backcast or forecast. Both WR and MW often seem to confuse reconstructions and climate models.

I’d combined many of the papers from **W.8.8** into a 429-page PDF, which together never use “backcast” and use “forecast” 15 times, but 10 are

found in a McCullough co-authored economics paper ② (#1), 2 are in Lindzen ① (#41), and 1 in MM05x ① (#79). That was a quick sample, and maybe some were missed. These terms are often used by those attacking paleoclimate, rarely by those actually doing it.

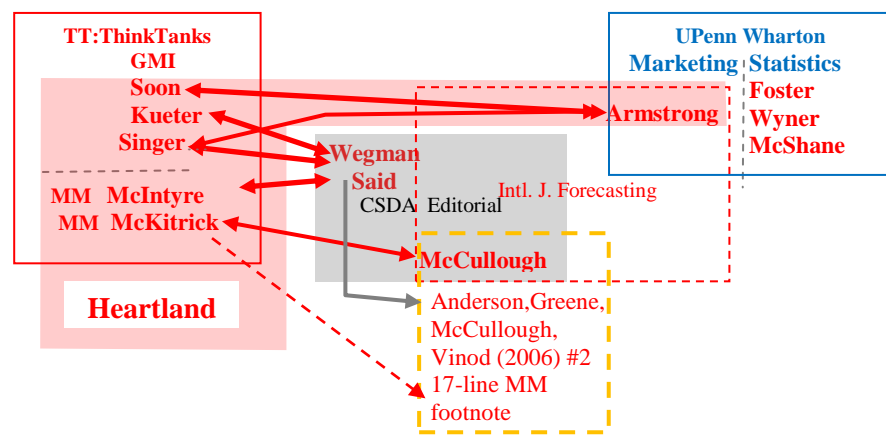
Sometimes people fruitfully bring their terminology into a field, but sometimes their usage just shows poor knowledge of that field. People who plagiarize the WR for paleoclimate background probably should study more, at least to read Wikipedia a little further about rocks.

Relationships

Of course, the paper will rise or fall on its own merits, and should there be academic misconduct charges, they will get judged on their merits.

However, it is worth studying the network relationships, because “new faces” rarely appear from nowhere.

The following consolidates known relationships. **Of course, nothing is proved by Armstrong’s co-location with MW and their citation of a poor, irrelevant article by him, but much would be explained by a closer connection.** Through Armstrong they would have immediate access to key members of the climate anti-science network well-practiced to finding “new faces,” helping them with references, helping with publicity in media like the WSJ, etc.



A.12.6 MW, behind the new façade

As in §3.6, this section consolidates my opinions on a plausible scenario for this paper, which in many ways really is a remake of the WR.

- A clear attitude emerges from earlier blog postings by Wyner and colleague Dean Foster, who is thanked in the MW Acknowledgements. They are very self-confident in their own knowledge, dismissive of others, and this viewpoint persists in MW.
- Some of the wording simply does not belong in a serious statistics paper.
- Just as the WR takes shortcuts, so does MW. The Bradley (1999) reference is clearly fabricated to cover use of Wikipedia and the WR, and Mann, et al (2004) is repeatedly mis-used. Text cites [IPC2001, IPC2007] for assertions that are contradicted there by any reasonable reading short of careful cherry-picking.
- Errors are pervasive, as shown here and even more in [DEE2010q].
- Just as in the WR, obsolete and grey references are cited.
- MW shows little familiarity with the basic underlying science, plagiarizing the WR for erroneous paleoclimate introduction.
- Combined with that, one finds very detailed discussions of obscure proxies. Just as in W5.8, W.5.9, the combination of minimal knowledge and great detail elsewhere hints at help from others, especially McIntyre, who posts frequently on proxies.
- It is rare to find not-finalized statistics papers quickly lauded in the WSJ.

I do not know how this paper came to exist. Small groups of smart, self-confident people dismissive of others’ expertise can encourage each other in ideas far outside the mainstream. The real oddity is the acceptance in a credible journal run by credible people. That may be a hint of a structural problem for conferences and journals in applied statistics, which inherently covers a huge range of application areas. They may well have occasional difficulty getting adequate application-field referee coverage, as opposed to methods coverage. [DEE2010q] discusses some of the combinations of expertise required to do this well. I sympathize with the difficulties. Climate anti-science advocates often try to slip marginal papers into journals this way, although I have seen them more with physics or other publications. Some editors are accustomed to such tactics, but many are not. From past experience, reasonable people can be mis-used.

A.12.7 Changes between 1st, 2nd versions

The original version was dated 08/11/10 and was changed 09/01/10.

I used a PDF-comparison program (“Diff Doc”):

www.softinterface.com/MD/Document-Comparison-Software.htm

Although it sometimes generates extraneous results, it does show:

p.2

“including boreholes, corals, ~~speleotherms, spelcothems~~, and lake sediments”

“provide evidence only for the detection of ~~AGW~~ global warming “

p.19

“Finally, the empirical AR1 process and Brownian Motion both substantially outperform the proxies. They have a lower average holdout RMSE and lower variability than that achieved by the proxies. This is extremely important since these ~~three two~~ classes of time series”

p.30

“FIG 14. Backcasts to 1000 AD from the various models considered in this section are plotted in grey. CRU Northern Hemisphere annual mean land temperature is given by the thin black line with a smoothed version given by the thick black line. Three ~~forecasts~~ are featured: regression on one proxy principal component (red), regression on ten proxy principal components (green), and the two stage model featuring ~~one five~~ local temperature principal ~~component components~~ and ~~ten five~~ proxy principal components (blue).”

p.30

“model given in green or the two stage model featuring ~~one five~~ local temperature principal ~~component components~~ and ~~ten five~~ proxy principal components ~~featured given~~ in blue)

p.43:

Bradley, R. S. (1999). Paleoclimatology: Reconstructing Climates of the ~~Quaternary. Quaternary.~~ Academic Press, San Diego, 2nd edn

As I am avoiding the statistical analysis, I have not looked into the 1/10 vs 5/5 changes, but those might be interesting. The change from AGW to “global warming” is akin to “GW, but never AGW,” **A.2.**

W.0. Executive Summary

WR p.2 Paragraph 1.

“The MBH98 and MBH99 papers are focused on paleoclimate temperature reconstruction and conclusions therein focus on what appear to be a rapid rise in global temperature during the 1990s when compared with temperatures of the previous millennium.”

 “what appear to be”: climate scientists would write “is”. Most of these papers were about reconstruction (the “shaft”) not the “blade,” §1.5.

“These conclusions generated a highly polarized debate over the policy implications of MBH98, MBH99 for the nature of global climate change, and whether or not anthropogenic actions are the source.”

 No they didn’t, Meme-a¹, Meme-b¹, Theme-H⁴, §1. The “polarization” was mainstream science versus people who did not want restrictions on CO₂, the same “polarization” of public health versus tobacco, A.2.

“This committee, composed of Edward J. Wegman (George Mason University), David W. Scott (Rice University), and Yasmin H. Said (The Johns Hopkins University), has reviewed the work of both articles, as well as a network of journal articles that are related either by authors or subject matter, ...”

It is unclear how much “review” was actually done by Scott. In fact, it is actually unclear how much real review was done by Wegman. As noted elsewhere, many (very likely most) of the papers were provided through Spencer, many likely were selected by MM+TT.

“This Ad Hoc Committee has worked pro bono, has received no compensation, and has no financial interest in the outcome of the report.”

This seems likely to have been true at the time, but see A.7, A.3.

WR p.2 Paragraph 2

“MBH98, MBH99 use several indicators to measure global climate change. Primarily, these include historical records, tree rings, ice cores, and coral reefs. The width and density of tree rings vary with climatic conditions (sunlight, precipitation, temperature, humidity, and carbon dioxide and nitrogen oxides availability), soil conditions, tree species, tree age, and stored carbohydrates in the trees. However, tree ring density is useful in paleoclimatic temperature reconstructions because in mature trees, tree rings vary approximately linearly with age. The width and density of tree rings are dependent on many

confounding factors, making it difficult to isolate the climatic temperature signal. It is usually the case that width and density of tree rings are monitored in conjunction in order to more accurately use them as climate proxies.”

Most of this is a straightforward summarization and rewording of WR pp.10-14, especially pp.13-14 on tree-rings, taken from Bradley (1999) with weakening and odd changes, [DEE2010a, DEE2010j].

“Carbon dioxide” and “nitrogen oxides” are added to Bradley’s text. W.6 discusses the confusion over this, search for “nitrates” to see the likely source for these ideas in MM05a or McIntyre’s Climate Audit.

 The WR reader is quickly introduced to the idea of difficulty, without the corresponding idea that researchers deal with the issues. “Confounding factors” thus manages to get onto the first page, Meme-e¹.

WR p.3 Paragraph 1-2

“Ice cores are the accumulation of snow and ice over many years that have recrystallized and have trapped air bubbles from previous time periods. ... Coral is similar to trees in that the growth and density of the coral is dependent upon temperature.”

<e>This is not obviously from WR p.14 or Bradley (1999), and likely has another antecedent somewhere, as “Ice cores are the accumulation” is likely an editing error, as can be seen by comparison with:

en.wikipedia.org/wiki/Ice_core

Wikipedia is not the source, as that was later adapted from the WR.

WR p.3 Paragraph 3-4

“Principal component analysis is a method ... However, in MBH98, MBH99, the proxy data are incorrectly centered, which inflates the variance of certain proxies and selectively chooses those decentered proxies as the temperature reconstruction. ...

Because of complex feedback mechanisms involved in climate dynamics, it is unlikely that the temperature records and the data derived from the proxies can be adequately modeled with a simple temperature signal with superimposed noise. We believe that there has not been a serious investigation to model the underlying process structures nor to model the present instrumented temperature record with sophisticated process models.”

 Some seems straightforward discussion, corresponding to WR pp.15-17, but see [DEE2010j, DEE2010k] for discussion of PCA issues not addressed. The first underlined section omits that.

 “Because of ...” Given the lack of expertise elsewhere and the plagiarization found in the section from which this seems summarized, the underlined comment seems unsupported. This seems based on the Cohn, Lins (2005) reference, a favorite of McIntyre, *but which has problems*, Meme-05, *perhaps* Theme-E, **W.8.8**.

WR p.4 Paragraph 1 (extract from WR pp.28-29)

“Two principal methods for temperature reconstructions ...”
This is about PCA decentering, which all agree is an error, but as Wahl, Ammann (2007) showed, it makes little difference.

WR p.4 Paragraph 2-4, p.5, Paragraph 1

This is an extract, with some different wording from Findings, **W.6**, which should be consulted for more detailed explanations of Issues.

In general, we found MBH98 and MBH99 to be somewhat obscure and incomplete and the criticisms of MM03/05a/05b to be valid and compelling. We also comment that they were attempting to draw attention to the discrepancies in MBH98 and MBH99, and not to do paleoclimatic temperature reconstruction. Normally, one would try to select a calibration dataset that is representative of the entire dataset. The 1902-1995 data is not fully appropriate for calibration and leads to a misuse in principal component analysis. However, the reasons for setting 1902-1995 as the calibration point presented in the narrative of MBH98 sounds reasonable, and the error may be easily overlooked by someone not trained in statistical methodology. We note that there is no evidence that Dr. Mann or any of the other authors in paleoclimatology studies have had significant interactions with mainstream statisticians.”

 “obscure”.

 “no evidence” had they asked paleoclimate people or the relevant statisticians they might have gotten different answers. Wegman certainly knew some of the latter. This comment is especially ironic given that the **Page tally, §2.7**, shows that at least half of the WR is either paleoclimate or SNA, two disciplines with which the WP apparently lacked “significant interactions.”

In our further exploration of the social network of authorships in temperature reconstruction, we found that at least 43 authors have direct ties to Dr. Mann by virtue of coauthored papers with him. Our findings from this analysis suggest that authors in the area of paleoclimate studies are closely connected and thus ‘independent studies’ may not be as independent as they might appear

on the surface. This committee does not believe that web logs are an appropriate forum for the scientific debate on this issue. It is important to note the isolation of the paleoclimate community; even though they rely heavily on statistical methods they do not seem to be interacting with the statistical community. Additionally, we judge that the sharing of research materials, data and results was haphazardly and grudgingly done. In this case we judge that there was too much reliance on peer review, which was not necessarily independent. Moreover, the work has been sufficiently politicized that this community can hardly reassess their public positions without losing credibility. Overall, our committee believes that Mann’s assessments that the decade of the 1990s was the hottest decade of the millennium and that 1998 was the hottest year of the millennium cannot be supported by his analysis.

 “independent studies”

 “isolation of the paleoclimate community”

 “too much reliance on peer review”

All these are Meme-b, Meme-c, using overly simple SNA to make claims with no evidence. People pushed back and Wegman/Said responded by writing [SAI2008], **W.5.6**.

WR p.6 Paragraph 1-4

This just copies the Recommendations paragraphs, **W.7**.

“Recommendation 1. Especially when massive amounts of public monies and human lives are at stake, academic work should have a more intense level of scrutiny and review.”

 IPCC and NRC have intense reviews. IPCC logs every question and answer. For discussion of the review of the WR, manufactured *specifically* to affect policy, see **A.1**.

W.1 Introduction

WR p.7:

This is fairly straightforward, no comment needed.

WR pp. 8-9:

This simply copies the Barton/Whitfield letters sent July 2005.

W.2 Background

W.2.1 Background on Paleoclimate Temp.

WR. p.10 is adapted from Bradley (1999), although the title of this key reference is misspelled, **W.8.8**. The first page is reproduced here, not claimed plagiarism as it seems a fair summary of Bradley, pp.1-10, although some wording is rather close, and it at least cites Bradley in the paragraph. The cyan mostly locates the text being summarized. Bradley's book spends 600 pages carefully explaining noise and techniques for

WR, p.10

Paleoclimatology focuses on **climate**, **principally temperature**, **prior to the era** when instrumentation was available to measure climate **artifacts**. Many **natural phenomena** are climate dependent and, where records are still available, these phenomena may be used as proxies to extract a temperature signal. *Of course the proxy signals are extremely noisy and thus temperature reconstruction becomes more problematic as one attempts reconstructions further back in time. Climate is not solely a matter of (global) temperature, although concerns with the effects of global warming focus primarily on temperature reconstructions. As just suggested, temperature reconstruction is based on proxy signals contained in the historical records of climate dependent natural phenomena. Table 1 based on Bradley (1999) illustrates the wide variety of these natural phenomena that may be used as proxies. Some proxies measure very low frequency (slowly varying) climatic variables and thus are not useful for measuring average annual temperature changes. Table 2 found in Bradley (1999), which was reproduced from Bradley and Eddy (1991) summarizes a variety of proxies and also indicates their minimum sampling interval as well as the range of years for which they could reasonably be used for temperature reconstruction. The high frequency proxies that could be used on an annual basis include tree rings, ice cores, and corals. In addition to serving as temperature proxies, these measurements are proxies for other climatic variables including, for example, precipitation, chemical composition of air and water, and solar activity.*

1. <e> "**principally temperature**" strongly shrinks the paleoclimate field.
2. <e> "**artifacts**" is strange terminology for natural effects. See also **A.12**.
3. <ECB> "**Of course...more problematic as one attempts reconstruction further back in time..**" **Meme-e**.

extracting signal. In issue 3, the WR mis-uses a comment Bradley makes regarding >1Million years ago ("earlier periods"). That is irrelevant to discussion of MBH's 1000AD- reconstructions, where the main issue is the lower number of suitable, geographically distributed, high-frequency proxies going back in time. A casual WR reader might think that proxies automatically get less useful, a serious over-generalization. Ice cores cannot be dated to specific year and are geographically limited, but they stretch very far back. Bradley writes about these issues in careful detail.

Bradley (1999), p.1

Paleoclimatology is the study of **climate prior to the period** of instrumental measurements. Instrumental records span only a tiny ... study of **natural phenomena** **which are climate-dependent, and** which incorporate into their structure a measure of this dependency. Such phenomena provide a proxy record of climate and it is the study of proxy data that is the foundation of paleoclimatology. ...

Bradley (1999), p.2

an overview of the climatic record during the late Quaternary (the last ~1 Ma) is also provided. The climate of earlier periods can be studied...but the farther back in time one goes, the greater the problems of dating, preservation, disturbance and hence interpretation. ...

Bradley (1999), p.4

Many natural systems are dependent on climate; where evidence of such systems in the past still exists, it may be possible to derive paleoclimatic information from them. By definition, such proxy records of climate all contain a climatic signal, but the signal may be relatively weak, embedded in a great deal of extraneous "noise" arising from the effects of other (non-climatic) influences.

Bradley, p.7, Table 1.2 lists the many different kinds of information studied. pp.1-10 hardly says "primarily temperature."

While some results may be artifacts of analysis processes, the phrase "climate artifacts" it is not something generally seen in the literature and actually makes little sense. Tree rings, ice cores, etc are natural, not artifacts.

Pages 11-12 are acceptably cited tables from Bradley (1999), although p.11 contains several silly errors compared to the original. It is copied almost exactly from Bradley, with a few changes of format, including odd numbering of 2.b that slightly changes logical structure. It looks impressive, but most is irrelevant. In copying a table, 3 errors occur:

WR.p.11	vs	Bradley, p.5	
<ec> ions		major ions	“Major ions” are different
<e> Speleothrems		Speleothems	Speleothrems is typo
<e> Phonology		phenology	No sound records from plants

Bradley correctly writes “major ions and isotopes of hydrogen and oxygen,” but WR has “ions and isotopes of hydrogen and oxygen.” Most would read that as (ions and isotopes) of (hydrogen and oxygen), wrongly. “Major ions” specifically means Na+, Cl-, etc and they are more used to help date cores, Bradley (1999), p.145.

www.ldeo.columbia.edu/~martins/eda/major_ions_lec1.html
www.oceanplasma.org/documents/chemistry.html#6_major_ions

“Speleothrem” is a typo. The erroneous “speleotherm” appears occasionally, but never in Bradley. In Google Scholar, hits for “speleothem” outnumber those for “speleotherm” by about 12,000 to 250.
en.wikipedia.org/wiki/Speleothem

“Phenology” and “phonology” are rather different. Although paleoclimate researchers might find uses for 1000-year-old recordings of sounds from plants and animals, such seem unlikely to be found, whereas some cherry and peach blossom date records reach back before 1000AD.

en.wikipedia.org/wiki/Phenology
en.wikipedia.org/wiki/Phonology

The WR contains text *seemingly intended to convey expertise*, but when examined closely, text is copied from elsewhere, with little understanding, but with errors, sometimes very silly ones. Then, McShane and Wyner start from the WR and propagate errors, but cite Bradley, **A.12**.

[DEE2010j, DEE2010b] show that most of WR pp.13-14 on tree-rings is **SS** text from Bradley (1999), but with added distortions, errors and a major inversion. DC counted underlined issues as (**8 major**, 3 minor).

deepclimate.files.wordpress.com/2010/07/wegman-bradley-tree-rings-v20.pdf

[DEE2010j, DEE2010a] show that WR pp. 14-15 on ice and corals is straightforward **SS**., with (**0**, 1) issues.

deepclimate.files.wordpress.com/2010/07/wegman-bradley-ice-cores-corals-v2.pdf

Much of the WR’s version later appeared in [RAP2008], as shown in [DEE2009, DEE2009a, DEE2010], where all this started.

<**ECB**> [DEE2010j] finds 8 major and 4 minor issues, counted only as one each in the **Page tally, §2.7**, for simplicity, although one of the <**B**> issues inverts a major conclusion of Bradley’s, a strong Bias.

W.2.2 Background on Princip. Components

[DEE2010j, DEE2010k] show that this section, pp.15-17 is straightforward **SS** assembled from 9 different sources, but introduces (**0**, 4) issues.

deepclimate.files.wordpress.com/2010/07/wegman-principal-components-and-noise-models.pdf

<**ec**> The issues are minor Errors that Change meanings, possibly substantially but without obvious Bias. Some show misunderstanding of the topic. Scott wrote Appendix A, **W.9**, *but surely not this*.

[DEE2010j] expressed surprise that an introduction to basic statistical concepts would be handled this way. Statistics is a huge field with many subspecialties, so perhaps the writer was unfamiliar with these, even though they were quite relevant to the statistical discussions of the WR:

“Finally, the PCA and noise model section discussed above clearly contains the least “strikingly similar” material. **But the surprise here is that there is any at all. Not only that, but changes made by Wegman et al have apparently introduced errors.** Moreover, the sheer number of apparent sources and relative brevity of the antecedent passages means that additional antecedents cannot be ruled out.”

Together with WR §2.3 discussed next, WR pp. 13-22 (10 pages total) are clearly **SS** text, plus noticeable Errors and occasional Biases. WR p.9 is marginal, and I give it the benefit of the doubt.

W.2.3 Background on Social Networks

[DEE2010j, DEE2010f] show the extensive striking similarities found in WR pp.17-22 to antecedents in Wikipedia, Wasserman, Faust [WAS1994] and DeNooy, Mirvar, Batagelj [DEN2005]. DC rates this as [0,8] issues. Their flows are studied here, as this text propagated several times.

DC did all the difficult work to find this and show side-by-side comparisons in [DEE2010g, 1st version], kindly providing me the file from which this is derived. Others people contributed useful comments to the ensuing discussion. [DEE2010h] showed the striking similarities of [SAI2008] and [SHA2008] to the same unacknowledged sources, with a recent consolidated analysis in [DEE2010p].

So this section is about 90% DC, 5% others, and 5% mine.

The following consolidates [DEE2010g] and comments from that thread:

- Column 3: originals: [WIK2006a, WAS1994, DEN2005]
- Column 2: WR
- Column 1: [SAI2008, SHA2008] are shown together, as the latter is a subset of the former, except for a few highlighted words.
- no column, but in Hadi Rezazad PhD, **A.5.10**, marked: Rezazad p. nn→

This uses the same conventions as **W.11.8**, extended for the extra column. Columns 2 and 3 here correspond to [DEE2010g, newer version], so the reader can compare the algorithmic differences of our approaches.

ID (IDentical) text uses **cyan-highlighted regular font** for identical words extracted in order from the antecedent.

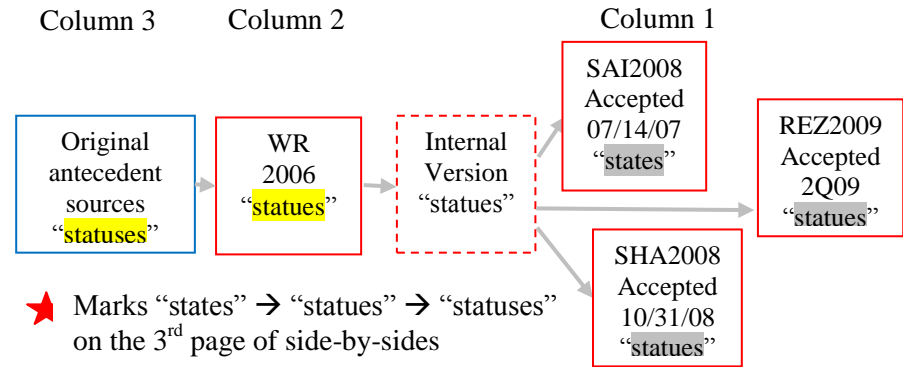
SS (Striking Similarity) text is shown in regular font, thus including all **ID**, but adding text seemingly done by local paraphrasing needing little understanding. Whole blocks of text are marked **SS** if they include substantial **ID** text and mostly minimal changes. **Yellow-highlighted SS words** (TC) make truly trivial changes. *Italics text* seems reasonable summarization and text whose wording seemed inescapable. It also includes text for which no antecedent was found. *Italic text might* not be plagiarism of the antecedent, but anything in Regular font should be fairly clear. Obvious text movements are *not* highlighted.

Total words = # **SS** + # *Italics*. # **SS** = # **ID** + # **TC** + # unhighlighted

Columns 1 and 2 use cyan highlights relative to the original antecedents, so those words are identical in all. In addition, used only here are:

Green highlight text is identical in Columns 1 and 2, but not in 3.

Grey highlight text differs between [SAI2008] and [SHA2008, REZ2009]. Very few words are grey, but “states” turns out to be a clue. Given multiple authorships, *it remains unclear exactly who did the original copy-and-edits and who knew about it then*. However, the 4-way comparison may help, especially given dates and unusual edits:



One might think that [SHA2008] was derived from the earlier [SAI2008], but strange wording implies otherwise. As described in [DEE2010f], “moving between places or **statuses**” in [WAS1994] became “moving between places or **statues**” in the WR and that became “moving between places or **statuses**” in [SAI2008] but was still “moving between places or **statuses**” in [SHA2008] about a year later. DC suggests that OCR may have misread “statuses” as “statues.” Finally, someone noticed “statues” and changed it in [SAI2008], most likely in last-minute proofreading. It is hard to imagine anyone changing “states” back to “statues.” *It seems more likely that someone other than Sharabati changed this in [SAI2008].* “Statues” appears once again, **A.5.10**.

Wegman and Said co-directed Sharabati, **W.5.7**. Wegman directed Rezazad, **W.5.10**. If Said did the original WR plagiarism, one might think she would notice it in [SHA2008] and Wegman notice it in [SHA2008, REZ2010]. If Sharabati did the original, that was 6 uncredited pages, and one would think Wegman would notice it in [REZ2010]. Rezazad re-used the WR. It is hard to find a scenario in which all this was done by one person with no one else knowing. In any case, the plagiarism “style” is certainly compatible with that of Said’s dissertation, **A.9**. *There is a serious PhD supervision problem here.*

[SAI2008, p.2177, SHA2008, p.1]

A *social network is an emerging tool* frequently *used on quantitative social science* to understand how individuals or organizations are related.

The basic mathematical structure for visualizing the social network is a graph. A graph is a pair (V, E) where V is a set of nodes or vertices and E is a set of edges or links.

The text above is just the standard definition of a graph, but this seems odd placement, strange editing.)

Social network analysis (also called network theory) has emerged as a key technique and a topic of study in modern sociology, anthropology, social psychology and organizational theory..

[SAI2008, p.2177, SHA2008, p.2]

The shape of the social network helps determine a network's usefulness to its individuals. Smaller, tighter networks can be less useful to their members than networks with lots of loose connections (weak ties) to individuals outside the main network. More "open" networks, with many weak ties and social connections, are more likely to introduce new ideas and opportunities to their members than closed networks with many redundant ties.. See *Granovetter (1973)*.

"A social network is an emerging tool": Strange wording, usually social network analysis is the tool.

"used on quantitative social science": "on" strange.

"on": [SAI2008] → "in": [SHA2008]

"Granovetter (1973)" in [SAI2008], not [SHA2008]. The latter references, never cites it. This is a famous sociology paper, but clearly not the antecedent. DC notes: "Attribution of this paragraph in Wikipedia is now to John Scott Social Network Analysis. (1991, London, Sage). There was no attribution in 2006, although Granovetter is usually credited with originating the idea of "weak ties." This seems Bibliography-padding.

[WR, p. 17]

2.3 Background on Social Networks
Networks, Relations and Structure

A social network is a mathematical structure made of nodes, which are generally taken to represent individuals or organizations.

A network graph illustrates how the nodes are connected.

Social network analysis (also called network theory) has emerged as a key technique and a topic of study in modern sociology, anthropology, social psychology and organizational theory.

Research has demonstrated that social networks, operating on many levels, from families up to the level of nations, play a critical role in determining

WR –§2.3, p. 18

the way problems are solved, organizations are run, and the degree to which individuals succeed in achieving their goals.

The shape of the social network helps determine a network's usefulness to its individuals. Smaller, tighter networks can be less useful to their members than networks with lots of loose connections (weak ties) to individuals outside the main network. More "open" networks, with many weak ties and social connections, are more likely to introduce new ideas and opportunities to their members than closed networks with many redundant ties. In other words, a group of friends who only do things with each other already share the same knowledge and opportunities. Yet a group of individuals with connections to other social worlds is likely to have access to a wider range of information. It is better for individual success to have connections to a variety of networks rather than many connections within a single network. Similarly, individuals can exercise influence or act as brokers within their social networks by bridging two networks that are not directly linked (called filling social holes).

"A social network is a mathematical structure"
Strange definition.

[WIK2006a] – Social Networks – 01/02/06

[Head section 1]

A social network is a social structure between actors, mostly individuals or organizations. [Sentence omitted]

Social network analysis (also sometimes called network theory) has emerged as a key technique in modern sociology, anthropology, Social Psychology and organizational studies, as well as a popular topic of speculation and study. Research in a number of academic fields have demonstrated that social networks operate on many levels from families up to the level of nations, and play a critical role in determining

the way problems are solved, organizations are run, and the degree to which individuals succeed in achieving their goals.

The shape of the social network helps determine a network's usefulness to its individuals. Smaller, tighter networks can be less useful to their members than networks with lots of loose connections (weak ties) to individuals outside the main network. More "open" networks, with many weak ties and social connections, are more likely to introduce new ideas and opportunities to their members than closed networks with many redundant ties. In other words, a group of friends who only do things with each other already share the same knowledge and opportunities. A group of individuals with connections to other social worlds is likely to have access to a wider range of information. It is better for individual success to have connections to a variety of networks rather than many connections within a single network. Similarly, individuals can exercise influence or act as brokers within their social networks by bridging two networks that are not directly linked (called filling social holes).

[SAI2008, p.2177, SHA2008, p.2]

None

Rezazad p.10→

Rezazad p.11→

[WR, p. 18] (cont)

Networks operate anywhere that energy and information are exchanged: between neurons and cells, computers and people, genes and proteins, atoms and atoms, and people and people. Social theories are built on more than just metaphors.

Social network analysis assumes that interpersonal ties matter, whether they exist among individuals, organizations or countries. Interpersonal connections matter because they are conduits for the transmission of information, goods, behavior and attitudes.

Ties and connections form networks, which can be analyzed.

The main goal of social network analysis is the detection and interpretation of patterns of social ties among people, nations, or organizations involved in social relationships.

“That interpersonal ties matter” Usually, ties among organizations and countries are not called interpersonal ties...

[DEN2005, p.3]

No antecedent found.

Social network analysts assume that interpersonal ties matter, as do ties among organizations or countries, because they transmit behavior, attitudes, information, or goods....

[DEN2005, p. 1

Social network analysis focuses on ties among, for example, people, groups of people, organizations, and countries. These ties combine to form networks, which we will learn to analyze.

[DEN2005, p. 5

In this book, the word actor refers to a person, organization, or nation that is involved in a social relation. We may say that social network analysis studies the social ties among actors.

The main goal of social network analysis is detecting and interpreting patterns of social ties among actors

[SAI2008, p.2177, SHA2008, p.2]

Social network analysis is concerned with understanding the linkages among social entities and the implications of these linkages. The social entities are referred to as actors that are represented by the vertices of the graph.

Most social network applications consider a collection of actors that are all of the same type. These are known as one-mode networks

Social ties link actors to one another. The range and type of social ties can be quite extensive. A tie establishes a linkage between a pair of actors.. Linkages are represented by edges of the graph. Examples of linkages include the

[SAI2008, p.2178, SHA2008, p.2]

evaluation of one person by another (such as expressed friendship, liking, respect),
 transfer of material resources (such as business transactions, lending or borrowing things),
 association or affiliation (such as jointly attending the same social event or belonging to the same social club),
 behavioral interaction (talking together, sending messages),
 movement between places or states ★
 (migration, social or physical mobility),
 physical connection (a road, river, bridge connecting two points),
 formal relations such as authority and biological relationships such as kinship or descent.
 A linkage or relationship establishes a tie at the most basic level between a pair of actors.
 The tie is an inherent property of the pair.

Rezazad p.12→

Many kinds of network analysis are

concerned with understanding ties among pairs and are based on the dyad as the unit of analysis.

“states”: [SAI2008], “statues”: [SHA2008] ★

It seems [SHA2008] used same text, someone edited ‘statues to ‘states’ there, but not in [SHA2008].

[WR, p. 18] (cont)

There are several key concepts at the heart of network analysis. We outline these concepts next and then define a social network.

Actor:

Social network analysis is concerned with understanding the linkages among social entities and the implications of these linkages. The social entities are referred to as actors.

Actors do not necessarily have the desire or the ability to act.

Most social network applications consider a collection of actors that are all of the same type. These are known as one-mode networks.

Relational Tie: Social ties link actors to one another.

The range and type of social ties can be quite extensive.

A tie establishes a linkage between a pair of actors.

Examples of ties include the

evaluation of one person by another (such as expressed friendship, liking, respect),
 transfer of material resources (such as business transactions, lending or borrowing things),
 association or affiliation (such as jointly attending the same social event or belonging to the same social club),
 behavioral interaction (talking together, sending messages),
 movement between places or statues ★
 (migration, social or physical mobility),
 physical connection (a road, river, bridge connecting two points),
 formal relations such as authority and biological relationships such as kinship or descent.

Dyad: A linkage or relationship establishes a tie at the most basic level between a pair of actors.

The tie is an inherent property of the pair.

Many kinds of network analysis are

[WR –§2.3, p. 19]

concerned with understanding ties among pairs and are based on the dyad as the unit of analysis.

“movement between places or statues” is very strange. DC suggests OCR, missed in WR, fixed slightly in [SAI2008], missed in [SHA2008].

[WAS1994, p.17]

There are several key concepts at the heart of network analysis ... we define some of these key concepts and discuss the different levels of analysis in social networks.

Actor.

social network analysis is concerned with understanding the linkages among social entities and the implications of these linkages. The social entities are referred to as actors. Our use of the term “actor” does not imply that these entities necessarily have volition or the ability to “act”. Further most social network applications focus on collections of actors that are all of the same type We call such collections one-mode networks...

[WAS1994, p.18]

Relational tie. Actors are linked to one another by social ties.

...the range and type of ties can be quite extensive. The defining feature of a tie is that it establishes a linkage between a pair of actors. Some of the more common examples of ties employed in network analysis are:

- Evaluation of one person by another (for example expressed friendship, liking, or respect)
- Transfers of material resources (for example business transactions, lending or borrowing things)
- Association or affiliation (for example jointly attending a social event, or belonging to the same social club)
- Behavioral interaction (talking together, sending messages)
- Movement between places or statues ★
- (migration, social or physical mobility)
- Physical connection {a road, river, ⦿ bridge connecting two points}
- Formal relations (for example authority)
- Biological relationship (kinship or descent)

Dyad. At the most basic level, a linkage or relationship establishes a tie between two actors. The tie is

inherently a property of the pair and therefore is not thought of as pertaining simply to an individual actor.

Many kinds of network analysis are

concerned with understanding ties among pairs. All of these approaches take the dyad as the unit of analysis

[SAI2008, p.2178, SHA2008, p.2]

“Triad” is never mentioned again. In general, this just copies standard definitions, then makes little use of them. One would expect that anyone familiar with this discipline would write a short paragraph, enough for the needs of the WR, rather than copying all this and doing trivial changes. It seems akin to bibliography-padding.

Rezazad p.13 →

A social network consists of a finite set or sets of actors and the relation or relations defined on them. The presence of relational information is a significant feature of a social network.

[WR, p.19]

Triad:

The analysis of a subset of three actors (a triad) and the possible ties among them is motivated and informed by balance theory.

Balance theory asks whether or not a triad is transitive or balanced. A transitive triad is characterized by transitive relations such as if actor i likes actor j, and actor j likes actor k, then actor i also likes actor k. A balanced triad means that if actors i and j like each other, then i and j should have similar evaluations of a third actor, whereas if they dislike each other then they are expected to differ in their evaluations.

Subgroup: Dyads are pairs of actors and associated ties, triads are triples of actors and associated ties. We can define a subgroup of actors as any subset among actors with associated ties. Locating and studying these subgroups using specific criteria is one of the primary objectives of social network analysis.

Group: Network analysis is not only concerned with collections of dyads, triads, or subgroups. Social network analysis has the ability to model the relationships among systems of actors.

A group is a collection of actors on which ties are measured.

Relation: The collection of ties of a specific kind among members of a group is called a relation, for example, the set of friendships among pairs of children in a classroom or the set of formal diplomatic ties maintained by pairs of nations in the world.

A relation refers to the collection of ties of a given kind measured on pairs of actors from a specified actor set.

Social Network: We are now in a position to define a social network.

A social network consists of a finite set or sets of actors and the relation or relations defined on them. The presence of relational information is a significant feature of a social network.

“significant” ← “critical&defining” not same.

[WAS1994, p.19]

Triad: Relationships among larger subsets of actors may also be studied. Many important social network methods and models focus on the triad; a subset of three actors and the (possible) tie (s) among them.

Balance theory has informed and motivated many triadic analyses. Of particular interest are whether the triad is transitive (if actor i “likes” actor j and actor j in turn “likes” actor k, then actor i will also “like” actor k), and whether the triad is balanced (if actors i and j like each other, then i and j should be similar in their evaluation of a third actor k, and if i and j dislike each other, then they should differ in their evaluation of a third actor, k).

Subgroup. Dyads are pairs of actors and associated ties, triads are triples of actors and associated ties. It follows that we can define a subgroup of actors as any subset of actors, and all ties among them. Locating and studying subgroups using specific criteria has been an important concern in social network analysis.

Group. Network analysis is not simply concerned with collections of dyads, or triads, or subgroups. To a large extent, the power of network analysis lies in the ability to model the relationships among systems of actors

For our purposes,

a group is the collection of all actors on which ties are to be measured.

[WAS1994, p.20]

Relation. The collection of ties of a specific kind among members of a group is called a relation. For example, the set of friendships among pairs of children in a classroom, or the set of formal diplomatic ties maintained by pairs of nations in the world, are ties that define relations. ... It is important to note that a relation refers to the collection of ties of a given kind measured on pairs of actors from a specified actor set...

Social Network. Having defined actor, group, and relation we can now give a more explicit definition of social network

A social network consists of a finite set or sets of actors and the relation or relations defined on them. The presence of relational information is a critical and defining feature of a social network ...

[SAI2008, p.2178, SHA2008, p.2]

none

Rezazad p.14 →

[WR, p.19]

Computational Facets of Social Network Analysis [Section]

The main goal of social network analysis is the detection and interpretation of patterns of social ties among actors.

Social network analysis may be viewed as a broadening or generalization of standard data analytic techniques and applied statistics that focus on observational units and their characteristics.

Complex network data sets may contain information about the characteristics of the actors (such as the gender of people in a group or the GNP of nations of the world) as well as structural variables.

Network problems naturally give rise to graphs.

The structural and compositional variables necessary for social network analysis often result in complicated data sets that must be modeled with sophisticated graph theoretic, algebraic and statistical methods.

The underlying mathematical frameworks used to build social network models are called graphs. A graph is a discrete structure consisting of vertices (nodes) and edges (links), where the vertices correspond to the objects, and the edges to the relations of the structure to be modeled.

[DEN2005, 1.3, p.31]

The main goal of social network analysis is detecting and interpreting patterns of social ties among actors.

{WAS1994, p. 21]

Social network analysis may be viewed as a broadening or generalization of standard data analytic techniques and applied statistics which usually focus on observational units and their characteristics ...

Complex network data sets may contain information about the characteristics of the actors (such as the gender of people in a group or the GNP of nations of the world) as well as structural variables.

[Antecedent not found]

[WAS1994, p. 22]

But the fact that one has not only structural, but also compositional variables, very complicated data sets that can be approached only with sophisticated graph theoretic, algebraic and/or statistical methods.

[Antecedent not found, standard definition]

[SAI2008, p.2178, SHA2008, p.2]

[SAI2008, p.2178, SHA2008, p.3]

Partitions divide the vertices of a network into a number of mutually exclusive subsets. That is, a partition splits a network into parts.

Partitions are also sometimes called blocks or block models. These are essentially a way to cluster actors together in groups that behave in a similar way.

At this point [SHA2008] diverges, no longer included.

[WR, p.20]

Computational Facets of Social Network Analysis [cont.]

A network consists of a graph and additional information on the vertices or lines of the graphs

Names of people or businesses or countries represent additional information on vertices.

Line values are numbers for arcs and edges that indicate the strength of relationships between actors.

This flexible definition allows a wide variety of empirical phenomena to be modeled as networks.

Properties of vertices are used to find and interpret patterns of ties in a network. Social networks are often complicated and may be large

Partitions are used to reduce a network so that different facets can be studied.

Partitions – A partition of a network is a classification or clustering of the vertices in the network so that each vertex is assigned to exactly one class or cluster.

Partitions may specify some property that depends on attributes of the vertices.

Partitions divide the vertices of a network into a number of mutually exclusive subsets. That is, a partition splits a network into parts.

We can produce a local view defined by a selected class of vertices that consists of all of the structural ties between nodes in the selected class of vertices.

Partitions are also sometimes called blocks or blockmodels. These are essentially a way to cluster actors together in groups that behave in a similar way.

[DEN2005, p.7]

A network consists of a graph and additional information on the vertices or lines of the graph. In the dining-table partners network, the names of the girls represent additional information on the vertices that turns the graph into a network. The numbers printed near the arcs and edges offer additional information on the links ... They are called line values, and they usually indicate the strength of a relation.

[DEN2005, p. 24]

This flexible definition permits a wide variety of empirical phenomena, ranging from the structure of molecules to be modeled as networks.

[DEN 2.7, p.51]

... we used properties of vertices to find and interpret patterns of ties in a network ... Social networks are often large and complicated. To understand network structure, it helps to study reductions of the network first.

Partitions can be used to reduce a network

[DEN2005, p. 31]

... A partition of a network is a classification or clustering of the vertices in the network such that each vertex is assigned to exactly one class or cluster.

Partitions may specify a structural property We call the latter attributes of vertices.

[DEN2005, p. 36]

Partitions divide the vertices of a network into a number of mutually exclusive subsets. In other words, a partition splits a network into parts.

Local View The easiest way to reduce a network is to select one class of vertices.

[SAI2008, p.2178 only]

Allegiance measures the support that an actor provides for the structure of his block. An actor supports his block by having internal block edges. A measure of this is the total number of edges that an actor has internal to his block. An actor supports his block by not having external edges from the block to other actors or blocks. A measure of this is the total number of possible external edges minus the total number of existing external edges. The allegiance for a block is a weighted sum of a measure of internal allegiance and a measure of external allegiance. The overall allegiance for a social network is the sum of the allegiances for the individual blocks. If the overall allegiance is positive then a good partition was made. The partitioning continues recursively until a new partition no longer contributes to a positive allegiance.

Rezazad p.15→

[WR, p.20]

Allegiance – Allegiance measures the support that an actor provides for the structure of his block. An actor supports his block by having internal block edges. A measure of this is the total number of edges that an actor has internal to his block. An actor supports his block by not having external edges from the block to other actors or blocks. A measure of this is the total number of possible external edges minus the total number of existing external edges. The allegiance for a block is a weighted sum of a measure of internal allegiance and a measure of external allegiance. The overall allegiance for a social network is the sum of the allegiances for the individual blocks. If the overall allegiance is positive then a good partition was made. The partitioning continues recursively until a new partition no longer contributes to a positive allegiance.

[RIG2004, p.2.] (should be [RIG2005])

Allegiance Allegiance measures the support that an actor provides for the structure of his block. An actor supports his block by having internal block edges. A measure of this is the total number of internal block edges that an actor has... An actor supports his block by not having external edges from the block. A measure of this is the total number of possible external edges minus the total number of existing external edges;

(The later [RIG2005] is likely the real source.)

If the overall allegiance, $O(A(k))$, is positive then a good partitioning was made. This process is iterated until additional partitioning no longer have positive effects.

[DEN2005, 2.4.2, p.39]

...[W]e may also zoom out to obtain a global view. Now, we are no longer interested in each individual vertex but we want to study relations between classes ...

[DEN2005, 2.4.2, p.59]

Cohesion

Solidarity, shared norms, identity, collective behavior, and social cohesion are considered to emerge from social relations. Therefore, the first concern of social network analysis is to investigate who is related and who is not. ... The general hypothesis here states that people who match on social characteristics will interact more often and people who interact regularly will foster a common attitude or identity.

[DEN2005, 3.1, p. 61]

Social networks usually contain dense pockets of people who “stick together.” We call them cohesive subgroups and we hypothesize that the people involved are joined by more than interaction. ... people who interact intensively are likely to consider themselves a social group.... This phenomenon is called homophily: birds of a feather flock together... we present a number of techniques to detect cohesive subgroups in social networks, all of which are based on the ways in

which vertices are interconnected. These techniques [have as] ultimate goal... to test whether structurally delineated subgroups differ with respect to other social characteristics, for instance, norms, behavior, or identity... May we conclude that a cohesive subgroup represents an emergent or established social group?

I would not call the red-bracketed “allegiance” section plagiarism, although it is uncited, but since the original author is involved, that happens.

The original text seems to be [RIG2004] which at least references [WAS1994], but it likely got changed somewhat for [RIG2005]. It seems plausible that the cyan+green text above came directly from that.

Rigsby is a coauthor of [SAI2008], so that is reasonable. Rigsby is Acknowledged in WR, so that use seems reasonable, although it might have been helpful to have actually cited and referenced the source.

The extent to which this terminology is used outside Wegman’s group is unclear, W5.2.

Rezazad p.16→

Global View – We may want a global view of a network

that allows us to study relationships among classes.

Cohesion –

Solidarity, shared norms, identity, collective behavior, and social cohesion are considered to emerge from social relations. The first concern of social analysis is to investigate who is related and who is not. The general hypothesis assumes that people who match on social characteristics will interact more often and people who interact regularly will foster a common attitude or identity.

Social networks usually contain dense pockets of people who stick together. They are called cohesive subgroups and usually

more than interaction joins the people involved. People who interact intensively are likely to consider themselves as a social group. This phenomenon is known as homophily: “birds of a feather flock together”. There are several techniques that detect cohesive subgroups in social networks. All of these techniques are based on the ways in

[WR, p.21]

which the vertices are interconnected. These techniques are used to investigate whether

a cohesive group represents an emergent or established social group.

[SAI2008]

None

[WR, p.20]

Social cohesion is used to describe structural concepts of density and connectedness. Density refers to the number of links between vertices. A network is strongly connected if it contains paths between all of its vertices and is weakly connected when semi-paths connect all of its vertices. Connected networks and networks with high average degree are thought to be more cohesive. There are several techniques to detect cohesive subgroups based on density and connectedness.

Affiliations – Membership in an organization or participation in an event is a source of social ties. An affiliation is a

relationship between people and an organization.

Affiliations are often institutional or structural and tend to be less personal as they result from private choices to a lesser degree than sentiments and friendship.

Brokerage – Social relations can be considered to be channels that transport information, services, or goods between people or organizations. From a bird’s eye view, social structure helps to explain how information, goods or even attitudes and behavior diffuses within a social system. Network analysis reveals social structure and helps to trace the routes that goods and information may follow. Some social structures permit rapid diffusion of information, whereas others contain sections that are difficult to reach.

We can also focus on the position of specific people or organizations within the network. In general, being well connected is advantageous. Contacts are necessary to have access to information and help. The number and intensity of a person’s ties are called his or her sociability or social capital. Social capital is known to correlate positively to age and education in Western societies. Some people occupy central or strategic positions within the system of channels and are crucial for the transmission process. Some positions may exert pressure on their occupants, but they also yield power and profit.

The direction of ties is not very important in social network structures that capture the exchange of information.

[DEN2005, p.77]

[S]ocial cohesion was linked to the structural concepts of density and connectedness. Density refers to the number of links between vertices. A network is strongly connected if it contains paths between all of its vertices and it is weakly connected when all of its vertices are connected by semipaths. Connected networks and networks with high average degree are thought to be more cohesive. ... There are several techniques to detect cohesive subgroups based on density and connectedness ...

[DEN2005, p. 101]

Membership of an organization or participation in an event is a source of social ties... Note that we studied relations among actors of one kind: relations between people or between organizations, but not between people and organizations. Now, we focus on the latter type, which is called an affiliation. Affiliations are often institutional or “structural.”... They are less personal and result from private choices to a lesser degree than sentiments and friendship.

[DEN2005, p. 121]

In quite a few theories, social relations are considered channels that transport information, services, or goods between people or organizations. In this perspective, social structure helps to explain how information, goods, or even attitudes and behavior diffuses within a social system. Network analysis reveals social structure and helps to trace the routes that goods and information may follow. Some social structures permit rapid diffusion of information, whereas others contain sections that are difficult to reach. This is a bird’s-eye view of an entire social network. However,

we can also focus on the position of specific people or organizations within the network. In general, being well connected is advantageous. Contacts are necessary to have access to information and help. The number and intensity of a person’s ties are called his or her sociability or social capital, which is known to correlate positively to age and education in Western societies. Some people occupy central or strategic positions within the system of channels and are crucial for the transmission process. Such positions may put pressure on their occupants, but they may also yield power and profit. ... [W]e focus on social networks as structures that allow for the exchange of information. In this approach, the direction of ties is not very important.

[SAI2008, p.2178 only]

Centrality is one

Rezazad p.17→

of the oldest concepts in network analysis. Most social networks contain people or organizations that are central. Because of their position, they have better access to information, and better opportunity to spread information. This is known as the ego-centered-approach to centrality. The network is centralized from socio-centered perspective. The notion of centrality refers to the positions of individual vertices within the network, while centralization is used to characterize an entire network. A network is highly centralized if there is a clear boundary between the center and the periphery. In a highly centralized network, information spreads easily, but the center is indispensable for the transmission of information. There are several ways to measure the centrality of vertices and the centralization of networks.

The concepts of vertex centrality and network centralization are best

understood by considering undirected communication networks. If social relations are channels that transmit information between people, central people are those people who have access to information circulating in the network or who may control the circulation of information. *i.e., they play a brokerage role.*

The accessibility of information is linked to the concept of distance. If you are closer to the other people in the network, the paths that information has to follow to reach you are shorter, so it is easier for you to acquire information. If we take into account direct neighbors only, the number of neighbors (the degree of a vertex in a simple undirected network) is a simple measure of centrality. If we also want to consider other indirect contacts, we use closeness centrality, which measures our distance to all other vertices in the network. The closeness centrality of a vertex is higher if the total distance to all other vertices is shorter.

Rezazad p.18→

[WR, p.21] (cont)

Centrality – This is one

of the oldest concepts in network analysis. Most social networks contain people or organizations that are central. Because of their position, they have better access to information, and better opportunity to spread information. This is known as the ego-centered-approach to centrality. The network is centralized from socio-centered perspective. The notion of centrality refers to the positions of individual vertices within the network, while centralization is used to characterize an entire network. A network is highly centralized if there is a clear boundary between the center and the periphery. In a highly centralized network, information spreads easily, but the center is indispensable for the transmission of information. There are several ways to measure the centrality of vertices and the centralization of networks.

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[WR, p. 22]

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The accessibility of information is linked to the concept of distance. If you are closer to the other people in the network, the paths that information has to follow to reach you are shorter, so it is easier for you to acquire information. If we take into account direct neighbors only, the number of neighbors (the degree of a vertex in a simple undirected network) is a simple measure of centrality. If we also want to consider other indirect contacts, we use closeness centrality, which measures our distance to all other vertices in the network. The closeness centrality of a vertex is higher if the total distance to all other vertices is shorter.

[DEN2005, p.123]

...we present the concepts of centrality and centralization, which are two

of the oldest concepts in network analysis. Most social networks contain people or organizations that are central. Because of their position, they have better access to information and better opportunities to spread information. This is known as the ego-centered approach to centrality. Viewed from a sociocentered perspective, the network as a whole is more or less centralized. Note that we use centrality to refer to positions of individual vertices within the network, whereas we use centralization to characterize an entire network. A network is highly centralized if there is a clear boundary between the center and the periphery. In a highly centralized network, information spreads easily but the center is indispensable for the transmission of information. In this chapter, we discuss several ways of measuring the centrality of vertices and the centralization of networks...

[DEN2005, p. 133]

The concepts of vertex centrality and network centralization are best

understood by considering undirected communication networks. If Social relations are channels that transmit information between people, central people are those who either have quick access to information circulating in the network or who may control the circulation of information.

The accessibility of information is linked to the concept of distance: if you are closer to the other people in the network, the paths that information has to follow to reach you are shorter, so it is easier for you to acquire information. If we take into account direct neighbors only, the number of neighbors (the degree of a vertex in a simple undirected network) is a simple measure of centrality. If we also want to consider indirect contacts, we use closeness centrality, which measures our distance to all other vertices in the network. The closeness centrality of a vertex is higher if the total distance to all other vertices is shorter.

[SAI2008, p.2178 only]

The importance of a vertex to the circulation of information is captured by the concept of betweenness centrality. From this perspective, a person is central if he or she is a link in more information chains between other people in the network. High betweenness centrality indicates that a person is an important intermediary in the communication network. Information chains are represented by geodesics and the betweenness centrality of a vertex is simply the proportion of geodesics between other pairs of vertices that include the vertex. The centralization of a network is higher if it contains very central vertices as well as very peripheral vertices.

[WR, p.22] (cont)

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[DEN2005, p.133] (cont)

The importance of a vertex to the circulation of information is captured by the concept of betweenness centrality. In this perspective, a person is more central if he or she is a link in more information chains between other people in the network. High betweenness centrality indicates that a person is an important intermediary in the communication network. Information chains are represented by geodesics and the betweenness centrality of a vertex is simply the proportion of geodesics between pairs of other vertices that include the vertex. The centralization of a network is higher if it contains very central vertices as well as very peripheral vertices.

Students often plagiarize to avoid work or to appear more knowledgeable, and this section seems of that sort, including trivial changes. Sometimes results become less clear than the antecedents, but no obvious bias is apparent in any of these, unlike **W.2.1, W.11**.

<ec> [DEE2010j] finds 8 minor issues, generally editing errors and oddities.

W.3 Literature Review

The following annotates WR pp.23-27. Text with “issues” is underlined.

WR, p.23

3. LITERATURE REVIEW OF GLOBAL CLIMATE CHANGE RESEARCH

“Michael Mann’s Dissertation and Related Work

In his 1998 dissertation, Michael Mann used instrumental data and multi-proxy datasets to observe climate variability over the past few centuries. He also used a simplified coupled ocean-atmosphere model to describe mechanisms that may contribute to the climate variability. In his dissertation, Dr. Mann described a 70 to 100 year oscillation in the climate signal formed by the proxy and instrumental data. He notes that this century scale variation in the climate involves a combination of meridional overturning (the circulation of cold water in the ocean) and gyre-scale circulation.”

As noted elsewhere, this seems mostly irrelevant. Ocean oscillations change geographic distributions of temperatures, and hence generate noise, but they are not generally considered radiative forcings, Theme-E⁴.

“After being awarded his doctorate, Dr. Mann, together with his colleagues Dr. Bradley and Dr. Hughes, continued multi-proxy reconstruction research with his 1998 paper, Global-Scale Temperature Patterns and Climate Forcing over the Past Six Centuries, [MBH98]. In this paper, he attempts to use PCA to find an eigenbasis (a new coordinate system where the axes are the eigenvectors, or principal components that represent the significant relationships in the data) for the multi-proxy data series for the period 1610- 1995. He also uses a multivariate regression method to observe possible forcing agents, or contributors to warming. Dr. Mann uses linear relationships between possible forcing agents (greenhouse gases, solar irradiance and volcanic aerosols) and climate in previous studies by R.S. Bradley and T.J. Crowley as a basis for regression analysis. He reports that the results are a large spike in greenhouse gas forcing in the 20th century. Additionally, he notes that 1995 and 1997 were likely the hottest years since 1400 AD within a 99.7% level of certainty.”

 Note personalization to Mann, “he ...” *The paper had 3 authors.*

Meme-d¹.

The large spike in greenhouse gases is well-measured, and the spike in forcing follows from basic physics, with lags and jiggles. [IPC2001] discusses this in detail, and these are not particularly MHB’s results, Theme-B⁴, Theme-C⁴.

In 1999, Dr. Mann and colleagues supplemented MBH98 by a new paper, Northern Hemisphere Temperatures during the Past Millennium: Inferences, Uncertainties, and Limitations MBH99. In this work they used similar methods to reconstruct temperatures further back to the beginning of the millennium. Although uncertainties are magnified with each previous year of reconstruction, their results suggested that 20th century warming counters a millennial-scale cooling trend and that the 1990s was likely the hottest decade in the millennium, with moderate certainty.

The title of MBH99 includes “Uncertainties” and its uncertainty ranges have certainly encompassed most points of most other professional reconstructions, W.4.4. Professionals understand uncertainty ranges and expect that different reconstructions would be plotted as “spaghetti graphs.” This is normal science trying to bound uncertainty, Theme-J⁴.

<EB> The comment “uncertainties are magnified with each previous year” is misleading. Uncertainties increase as proxies drop out, not just because of the calendar. This relates to the mis-use of Bradley (1999) mentioned as issue 3, W.2.1, Meme-e¹. The millennial cooling trend, with noise, is exactly what people expect from Milankovitch orbital cycles, as per Crowley (2002), Evans, et al (1976), others. From past cycles, without anthropogenic influence, Earth should be undergoing a slight, slow global cooling, with the usual jiggles.

“McIntyre and McKittrick

After MBH99, Stephen McIntyre and Ross McKittrick [MM03] published their critique of the 1998 paper, citing calculation errors, unjustified truncation or extrapolation of source data, obsolete data, geographical location errors and incorrect calculation of principal components. They also claimed that using the MBH98 methodology and the Northern Hemisphere average temperature index for the period 1400-1980 shows that temperatures in the 15th century exceeded those of the late 20th century.³ In particular, they claim that MBH98’s incorrect usage of PCA alone resulted in the well-known “hockey stick” shape.”

MBH had a few data problems, but they made little difference, W.8.4. MM03, p.8, Figure 8 managed to produce a reconstruction showing the highest temperature between 1400-1450AD, much higher than the 20th century, W.4.4, Meme-56¹.

<EB> None of the credible reconstructions do this. In fact, MBH98 has one of the higher curves for the 1400s. It seems strange to accept an extreme outlier as firm truth. [ESS20002] proposes one set of reasons to claim MBH98/99 wrong. MM03 offers a second set of reasons for it to be wrong, but decentered PCAs had not yet been noticed, after years of intense study. People paid little attention to MM03, given its publication in E&E and focus on a 4-year-old paper that had been confirmed (within uncertainty limits) and superseded by newer methods. Theme-A⁴,

“In a 2004 corrigendum, Mann et al. replied to these criticisms, contending that McIntyre and McKittrick’s finding resulted from an elimination of over 70% of the 15th century”

WR p.24

“proxy data used by MBH98. They also assert that MM03’s results fail independent crossvalidation tests. In subsequent response papers, MM05a and MM05b noted that the data eliminated in their earlier critique of MBH98 was restricted to two proxy series, the Gaspé cedar ring-width series, and the first principal component from the North American tree ring network in MBH98. In the case of the first principal component, McIntyre and McKittrick stated that the mean was not centered correctly over the period of analysis, 1400-1980. Instead of subtracting the mean of each data series between the years 1400 and 1980, they subtracted the mean of 1902-1980. McIntyre and McKittrick state that this decentering of the mean causes the explained variance of certain proxies to be inflated, namely the proxy series that causes the hockey stick shape. Subsequently, that particular proxy series is chosen as the principal component, indicating it is the most significant correlation in the data. With regard to the Gaspé cedar tree ring series, McIntyre and McKittrick state that Mann 1998 did not use archived data, but rather made an extrapolation in which they misrepresented the start date of the series. They also state that this extrapolation depresses the early 15th century results. Lastly, they note that the underlying dataset up to 1421 is based only on one tree, and only on two trees up to 1447.”

By MM05a, PCA decentering had become the main reason a then 7-year-old paper was wrong. In any case, using decentered PCA where it was actually used makes minimal difference, **W.8.4**.

“Mann and Rutherford’s 2005 paper in turn responded to these new criticisms, stating that McIntyre and McKittrick’s misunderstanding of their methodology and use of an incorrect version of the proxy dataset is the source of the

discrepancy in their results. They argue that the Mann et al. 1998 implementation calculates PC series of proxy networks over progressively longer intervals, which allows for the use of the maximum amount of data. For example, if there were 50 proxy series, and only 10 date back to AD 1400, then calculating one set of PC would eliminate 40 of the 50 series available back to AD 1600. By using two different intervals, 1400-1980 and 1600-1980 in this example, all proxy series can be utilized. Mann et al. contend that this misunderstanding is what led to the elimination of data prior to 1500 AD and is also what gives rise to the warmth in the 15th century of McIntyre and McKittrick’s version of MBH98.

To address the extrapolation critique, Mann et al. terminated the 1971 calibration period in which they filled in missing proxy values in the multi-proxy PC network between 1971 and 1980. **They also approached the reconstruction using a different method, regularized expectation maximization (REGEM), and yielded the same results.** They then conclude that their reconstruction is robust and reproducible, based on their use of an independent Climate Field Reconstruction method (the REGEM method) and their use of individual proxies instead of the multi-proxy PC representation used in Mann et al.1998.”

The WP knew all this, and in 2006 were still focused on 1998/1999 papers, Theme-A⁴. As shown in **A.1.3**:

[**BAR2006a**, p.38] Wegman contradicts this:

“MR. STUPAK. Okay. Let me ask you this question. **Have you reviewed any of Mr. Mann's later refinements of his 1999 report?**
DR. WEGMAN. I have reviewed some level of detail, not in intense level of detail, the continuing papers, detail, not in intense level of detail, the continuing papers, most of which are referenced--in fact, the ones that are referenced--
MR. STUPAK. Did he refine his data and his methodology?
DR. WEGMAN. My take on the situation is that rather than accept the criticism that was leveled, he rallied the wagons around and tried to defend this incorrect methodology.”

 Had Wegman studied Mann, et al (2005)? Had he even read the WR Summary of it? Did he read the words in the last quoted paragraph above? It is clear from all of these that Mann and others had moved beyond PCA. Actually, that was fairly clear earlier in Mann, Jones (2003). The testimony is very inconsistent, Meme-b¹, Theme-N⁴.

“Other Notable Works

While Mann et al. have focused much of their work on high frequency proxies, or those proxies that provide data on climate variability on a decadal or even yearly scale, Jan Esper and colleagues have investigated the effect of using low-frequency proxies that preserve data on climate variability on a centennial scale in their paper Low-Frequency Signals in Long Tree-Ring Chronologies for Reconstructing Past Temperature Variability. Esper et al. contend that preserving multi-centennial variability in tree-ring records is critical in comparing the temperatures of the Medieval Warming Period”

WR p.25

“(MWP) and those of the 20th century. By carefully selecting tree-ring chronologies from fourteen sites in the Northern Hemisphere (NH) extratropics, Esper et al. produced a reconstruction that preserved the multi-centennial variation, as well as supported the large-scale occurrence of the MWP over the NH extratropics. Using the regional curve standardization (RCS) method for their chronologies, Esper et al. found that there were significant multi-centennial differences between Mann et al. 1998 and their reconstruction. These differences may be explained by the fact that Mann et al.’s analysis includes data from the tropical and subtropical Northern Hemisphere whereas Esper’s analysis includes only the extra tropic region.

Low-frequency proxies may well be useful. The WR often confuses NH vs NH extra-tropics, and NH vs global, **W.4.4**. From the last sentence one might think that the WP would understand plausible reasons for different reconstructions, Theme-F. *The last sentence seems derived from Esper, et al (2002), p.2252, but the message did not seem to be noticed:*

“The MBH reconstruction includes temperature estimates from the tropical and subtropical NH (2), which is not represented in the RCS record. This may explain some of the observed differences.”

WR p.25

“In their 2005 paper *Highly Variable Northern Hemisphere temperatures reconstructed from low- and high-resolution proxy data*, Moberg et al. further studied the use of low resolution proxy data in order to preserve multi-centennial variability in climate reconstructions. Specifically, they focused on lake and ocean sediment cores, which tend to have a lower time resolution, but provide climate information at multi-centennial timescales that may not be captured by tree-ring data. Moberg et al. reconstructed Northern Hemisphere temperatures for the past 2,000 years by combining low-resolution proxies with tree-ring data. Their reconstruction shows a larger multi-centennial variability

than most previous multi-proxy reconstructions. Furthermore, their reconstruction depicted high temperatures that are comparable to the 20th century temperatures in the period AD 1000-1100.¹ Their results also suggest a natural trend in multi-centennial variability that is likely to continue.²”

<ecb> Meme-56.

Moberg et al (2005) p.617) says:

“Here we reconstruct Northern Hemisphere temperatures...
... high temperatures—similar to those observed in the twentieth century before 1990— occurred around AD 1000 to 1100 ...”

They also say (p.615):

“The model’s variability before the twentieth century is largely determined by the combined effects of the reconstructed solar and volcanic forcing (that is, natural forcing) that was used in the integration^{13,25}, whereas the notably strong warming in the model data after AD 1900 is largely due to rapidly increasing concentrations of anthropogenic greenhouse gases.”

Moberg, et al make normal scientific arguments that natural variability is higher than MBH said. However, the WR sentence claims a trend in variability. Statisticians know the difference between the **size of variability** and a **trend in size of variability**.

<ecb>*This may just be careless writing, but is surprising from professional statisticians.* If one wants to think that 20th-century warming might be mostly natural, an (upward) trend in variability might be useful.

“Following in this same trend, Hans von Storch et al., in their 2004 paper *Reconstructing Past Climate from Noisy Data*, used a coupled atmosphere-ocean model simulation of the past 1000 years to test the skill of past empirical reconstructions, specifically those of Mann et al. 1998, 1999. They found that while previous millennium based multi-proxy records find small amplitude variations followed by a clear warming trend in the past two centuries, the centennial variability of the Northern Hemisphere temperature is underestimated by these regression based methods. Their results also suggest that actual variability may have been at least twice as large as the variability predicted in these past studies. The authors surmise

WR p.26

“that this conclusion probably applies to most regression based methods of analysis and that other methods that estimate past temperatures with

physical (instead of statistical) methods or regression methods that address retention of low-frequency variability in proxies may be free from this critique.”

[IPC2007, pp.473-474] has a clear comment:

www.ipcc.ch/pdf/assessment-report/ar4/wg1/ar4-wg1-chapter6.pdf

“It is **very unlikely, however, that any bias would be as large as the factor of two suggested by von Storch et al. (2004) with regard to the reconstruction by Mann et al. (1998), as discussed by Burger and Cubash (2005) and Wahl et al. (2006).** ... **On the evidence of the previous and four new reconstructions that reach back more than 1 kyr, it is likely that the 20th century was the warmest in at least the past 1.3 kyr.** Considering the recent instrumental and longer proxy evidence together, it is very likely that average NH temperatures during the second half of the 20th century were higher than for any other 50-year period in the last 500 years.”

[IPC2006, p.6-32], offers an almost-identical passage, but 2007 had strengthened “not likely” to “very unlikely”:

pds.lib.harvard.edu/pds/view/7768990?n=570

 The WR emphasizes uncertainties in legitimate papers, but experts simply did not accept the more extreme uncertainties.

WR p.26

“Another 2005 paper *Are Multiproxy Climate Reconstructions Robust?* by Gerd Burger and Ulrich Cubasch questions whether these methods are statistically significant enough to be able to make robust conclusions. Burger and Ulrich describe sixty-four climate reconstructions, based on regression of temperature fields on multi-proxy networks, which are each distinguished by at least one of six standard criteria of this method. By combining these criteria Burger and Ulrich define numerous variants on millennial histories. No one criterion can account for the number of variations and no particular variant is more valid than another. Even the variant with the best reduction of error statistic is the furthest variation from the climate history of Mann et al. 1998. Burger and Cubasch conclude that the regression model is not valid when applied in an extrapolative manner, as in climate reconstruction.”

“In a 2006 paper, *The Spatial Extent of 20th Century Warmth in the Context of the Past 1200 Years*, Timothy Osborn and Keith Briffa examine the most prominent anomalies in proxy records since AD 1200. They state that the most significant anomaly is the geographical extent of warmth in the middle to late 20th century. They also detail anomalies during AD 890 to 1170 and AD 1580

to 1850 as being consistent with the concepts of a Medieval Warming Period (MWP) and Little Ice Age (LIA), respectively. However, they found that when comparing these anomalies with instrumental temperatures of the 20th century, the spatial or geographical extent of this warm anomaly is far greater than that of the MWP or LIA. Their study consisted of fourteen regional temperature-related proxy records. Since it is not possible to conduct a direct comparison between proxy records and instrumental temperatures, the proxy data analysis was conducted with each series normalized over the 1856 to 1995 period, or the period where proxy and instrumental data overlap. Relative to a decadal time scale, Osborn and Briffa found supporting evidence for the MWP and LIA, but their geographical reach appeared restricted since these anomalies were sensitive to specific proxy records.”

Most credible papers and [IPC1990] (which the WP did not read) have said the MWP varied temporarily or spatially Theme-G.

“Analysis

While the work of Michael Mann and colleagues presents what appears to be compelling evidence of global temperature change, the criticisms of McIntyre and McKittrick, as well as those of other authors mentioned are indeed valid.

 This is so easily misinterpreted. The vagueness of the statement lets it be a surrogate for Meme-56 and Meme-18. Anyone who can quote this to say “MBH claimed 20th-century warming was unusual, but the WR said they were wrong.”

Because the error and uncertainty involved in climate reconstructions is magnified with each preceding year, the ability to make certain conclusions about the climate at the beginning of the millennium is not very robust. This is even less robust considering the inability to actually calculate an accurate uncertainty for these reconstructions.

 This is also misleading. MBH made claims with uncertainty limits, Theme-J. Climate papers often argued about uncertainties, but the WP seems to pay little attention to those discussions. Regardless of how they were computed, the MBH99 limits included most of the points of the other reconstructions. *Do statisticians make no claims unless “certain?”*

Additionally, the work of Esper, Von Storch and Moberg make valid arguments for the inclusion of low-frequency proxies as well as the inability of PCA to effectively measure variations on a multi-centennial scale.

Including low-frequency proxies likely has merit, but those papers yield results generally within MBH’s error bars.

This pitfall of PCA is further complicated by its tendency for misuse during the calibration process, specifically the decentering of the mean that McIntyre and McKittrick mention.

<EB> Wahl; and Ammann (2006) showed that decentering made very little difference, **W.8.5**. One would expect statisticians to do sensitivity analyses or at least not dismiss a paper with a 1-line footnote. *The WP focuses on 1998/1999 papers, despite citing later work*, Theme-A₄.

“The papers of Mann et al. in themselves are written in a confusing manner, making it difficult for the reader to discern the actual methodology and what uncertainty is actually associated with these reconstructions.”⁵ Vague terms such as “moderate certainty” (Mann et al. 1999) give no guidance to the reader as to how such conclusions should be weighed. While the works do have supplementary websites, they rely heavily on the reader’s ability to piece together the work and methodology from raw data. This is especially unsettling when the findings of these works are said to have global impact, yet only a small population could truly understand them. Thus, it is no surprise that Mann et al. claim a misunderstanding of their work by McIntyre and McKittrick.

 Competent researchers were able to do it, and early papers often have such issues. *Are all statistics papers written for a vast audience of non-specialists?* The WP talked to McIntyre when they needed help. Perhaps, had they talked to Mann, this would have been less of a problem. The MBH99 Summary had one of the highest percentages of plagiarism, showing nobody really cared much what it said, **W.8.2, W.11.8**.

In their works, Mann et al. describe the possible causes of global climate change in terms of atmospheric forcings, such as anthropogenic, volcanic, or solar forcings. Another questionable aspect of these works is that linear relationships are assumed in all forcing-

 Has the WP demonstrated any science credibility to argue this?

WR p.27

climate relationships. This is a significantly simplified model for something as complex as the earth’s climate, which most likely has complicated nonlinear cyclical processes on a multi-centennial scale that we do not yet understand.

 It is complicated, but that hardly means people understand nothing. This is usually called “an argument from ignorance.” Meme-e₁.

Mann et al. also infer that since there is a partial positive correlation between global mean temperatures in the 20th century and CO2 concentration,

greenhouse-gas forcing is the dominant external forcing of the climate system. Osborn and Briffa make a similar statement, where they casually note that evidence for warming also occurs at a period where CO2 concentrations are high. A common phrase among statisticians is *correlation does not imply causation.*

<EB> This sequence totally writes off basic physics via “correlation does not imply causation,”

It acts as though MBH99 was the basis for thinking CO2 was related to global temperature. Theme-B₄, Theme-C₄. Theme-H₄.

The variables affecting earth’s climate and atmosphere are most likely to be numerous and confounding. Making conclusive statements without specific findings with regard to atmospheric forcings suggests a lack of scientific rigor and possibly an agenda.

 Once again, “confounding factors” are invoked, just as the WP did in its handling of Bradley (1999), Meme-e₁. It consistently and wholeheartedly accepts and amplifies anything by MM, and denigrates the professionals in the field as a group. *In the light of this report, “possibly an agenda” seems rather inappropriate.*

It is also interesting to note that Mann’s dissertation focused on 70 to 100 year climate signal variability, yet his future work does not have a similar component. His subsequent papers focus heavily on tree ring measurements, which provide data on a decadal or yearly scale. In later work, he also makes no mention of the ocean circulation variables, which he describes in his thesis as being integral to the variation in climate. If this type of forcing is a natural variable, it makes the conclusions about atmospheric forcings seem incomplete.

<EB> Theme-E₄.

The work initiated by Mann and his colleagues is still in its infancy, and as such further study, the use of wider proxy networks and the development of more sophisticated climate models will all be necessary future steps in propagating this research. It is not expected or likely that after preliminary research, definitive conclusions can be made about the earth’s climate over the past millennium.

<EB> “Definitive conclusions” is another strawman. *The WP just did not seem to understand the science, starting with college-sophomore physics.* The work was hardly in its infancy in 2006.

W.4 Reconstructions & Exploration of PC Methodologies

W.4.1 Where is real statistical analysis?

This whole statistics discussion is tailor-made for causing confusion. It is the core of real mission #1.

Decentered Principal Components and other statistical issues have been covered elsewhere enough, as in recent discussion by a good statistician [TAM2010]. Even experts must do specific work to replicate results and know whether legitimate general complaints (data errors, decentering) actually matter in the specific case.⁵⁷ To do it “right” requires both centering and choosing the correct number of PCs, which MM failed to do, **W.8.4**. Given all this, I had resolved to avoid the real statistics analysis in the WR, *but eventually realized there was none*.

This section briefly excerpts WR 28-37, discussing graphs later.

WR p.28, Paragraph 1

“Mann et al. (2005) identify two major methods of climate reconstruction, which they describe respectively as climate field reconstruction (CFR) methods and what they describe as *simple climate-plus-scale* (CPS) methods. CFR methods are claimed to “assimilate proxy records into a reconstruction of underlying patterns of past climate change” and among papers identified as using these methods are MBH 98, **Evans et al. (2002)**, **Luterbacher et al. (2002)**, Rutherford et al. (2005) and **Zhang et al. (2004)**. In contrast CPS methods are said to “composite a number of proxy series and scales the resulting composite against a target (e.g. Northern Hemisphere temperature) instrumental series.” Examples of papers using the CPS methods include **Jones et al. (1998)**, **Crowley and Lowery (2000)**, **Briffa et al. (2001)**, **Esper et al. (2002)**, **Mann and Jones (2003)** and **Crowley et al. (2003)**. Although the language describing both of these methods seems somewhat obscure, it would appear that CFR methods are just principal component methods as describe earlier and in the appendix and that CPS methods are just simple averaging of climate proxies and then scaling them to actual temperature records.”

 Hence, an “obscure” verdict must be assessed carefully.

W.11.8, #43, Mann, et al (2005) shows the side-by-side, rated at 47% **ID**, 85% **SS**, which displays little understanding, just cut-paste-and-edit.

⁵⁷ For example, Cressie suggested in #2 redoing the analysis with the right centering, which Wahl, Ammann (2007) did, but the WP did not. deepclimate.files.wordpress.com/2010/08/cressie-email-2006-07-18-with-attachment.pdf

The following started with Mann et al (2005), p.4097, was adapted to WR p.84 and further adapted here, but with references added.

<e> “simple climate-plus-scale” is wrong, Mann, et al (2005):

“MBH98; **Evans et al. 2002**; **Luterbacher et al. 2002**; Rutherford et al. 2005; **Zhang et al. (2004)**. The other group, simple so-called composite- plus-scale (CPS) methods (Bradley and Jones 1993; **Jones et al. 1998**; **Crowley and Lowery 2000**; **Briffa et al. 2001**; Esper et al. 2002; Mann and Jones 2003, henceforth MJ03; **Crowley et al. 2003**), ...”

Bold references above appear in WR Bibliography, but are cited nowhere else or only indirectly, **WR.5.8**, *whose origin likely was McIntyre*.

WR p.28, Paragraph 2

“The key issue in dispute is the CFR methodology as used in MBH98 and MBH99. The description of the work in MBH98 is both somewhat obscure and as others have noted incomplete.” (*discussion of decentering*)

 “Obscure” appears again, and the other noters are MM.

The decentering topic seems reasonable, as far as it goes, not far enough. www.realclimate.org/index.php/archives/2006/07/the-missing-piece-at-the-wegman-hearing

WR p.29, Paragraph 1

“While this error would have been less critical had the paper been overlooked like many academic papers are, the fact that their paper fit some policy agendas has greatly enhanced their paper’s visibility. Specifically, global warming and its potentially negative consequences have been central concerns of both governments and individuals. The ‘hockey stick’ reconstruction of temperature graphic dramatically illustrated the global warming issue and was adopted by the IPCC and many governments as the poster graphic. The graphics’ prominence together with the fact that it is based on incorrect use of PCA puts Dr. Mann and his co-authors in a difficult face-saving position. We have been to Michael Mann’s University of Virginia website and downloaded the materials there. Unfortunately, we did not find adequate material to reproduce the MBH98 materials.”

 This clear point of view is not actually statistical analysis.

“We have been able to reproduce the results of McIntyre and McKittrick (2005b). While at first the McIntyre code was specific to the file structure of his computer, with his assistance we were able to run the code on our own machines and reproduce and extend some of his results.”

 They had trouble with both sets of software. They worked with McIntyre, never contacted Mann.

WR pp.30-32

Using McIntyre's code and help, they reproduced MM05b-like charts. Re-running code does not prove that its assumptions are correct.

WR p.33

"Figure 4.4: One of the most compelling illustrations that McIntyre and McKittrick have produced..."

Unlike Figures 4.1-4.3, this is uncited. It is not in MM05b, the only peer-reviewed paper, nor MM05a. The closest seems MM05x, Figure 7, p.9, reused in MM06, Figure 4, p.9. *Figure 4.4 may have been obtained from some unreferenced MM source, generated by WP from scratch or created by McIntyre specifically for the WR.*

Stanford's David Ritson⁵⁸ explained serious errors, repeatedly asked Wegman, et al for code and explanation, got nothing.

www.meteo.psu.edu/~mann/house06/RitsonWegmanRequests.pdf

"You will notice that the M&M inputs purport to show
 >>strong persistence out to lag-times of 350 years or beyond.
 >>Your report makes no mention of this quite improper M&M procedure
 >>used to obtain their ACFs. Neither do you provide any specification data for
 >>your own results that you contend confirm the M&M results. Relative to your
 >>**Figure 4.4** you state
 >>"One of the most compelling illustrations that M&M have produced
 >>is created by feeding red noise (AR (1) with parameter = .2 into the MBH
 >>algorithm".
 >>**In fact they used and needed the extraordinarily high persistences contained in**
 >>the attached figure to obtain their 'compelling' results."

A general problem may or may not apply to a specific case, depending on parameters, Meme-g⁶.

WR p.34

 This is a long-obsolete, irrelevant and even distorted sketch, **W.4.2**. It is likely from MM05x, p.5.

WR p.35-36

The WR offers two pages of noise superimposed on an irrelevant graph.

WR p37

"The point being made..."

Thus even discussions of 'independent replications' of the hockey stick results by different methods may not be what they superficially appear to be."

This is Meme-c¹. Not only do they dismiss MBH99 without trying the decenter fix and without proper PC selection, but they cast doubt on any other methods as well, without trying them. As [TAM2010] says:

"The truth is that whichever version of PCA you use, the hockey-stick shaped PC is one of the statistically significant patterns. There's a reason for that: the hockey-stick shaped pattern is in the data, and it's not just noise it's signal. Montford's book makes it obvious that MM actually do have a selection rule of their own devising: if it looks like a hockey stick, get rid of it."

Summary of this section

Despite all the graphics and technical discussion, the WR fails to do the one thing to be expected of statisticians criticizing a method's results: reanalyze with the "right" method (centered PCA **and** correct number of PCs) and see if it makes any difference. *The WR contains no actual new statistical analysis of MBH itself, just reworks of MM material.*

This rest of this section collects graphs used directly or indirectly by the WR, with some curves superimposed on others.

This may help the reader see the WR's internal contradictions.⁵⁸

W.4.2 discusses the problems with the sketch on WR p.34.

W.4.3 shows graphs of borehole reconstructions mentioned in the WR.

W.4.4 shows the MM03 temperature reconstruction, which actually contradicts the sketch, even the distorted version in the WR.

None of the WR's favored charts bears much resemblance to the host of different reconstructions done by credible researchers.

⁵⁸ Alice in Wonderland seems relevant once again. This is perhaps akin to the White Queen, who could believe "six impossible things before breakfast." In this case it seems more like simultaneously believing at least 3 incompatible charts, (1990 FAR, MM03/MM05x, Huang, et al (19997), none of which fit very well with more credible research.

W.4.2 Obsolete graph, distorted, well-used

The WR continually supports the idea of a globally synchronous MWP, noticeably hotter than today's climate, Meme-56¹. They use an outdated source, repeatedly ignoring the Summarized papers and many references saying it simply was not true, Theme-A⁴, Theme-G⁴.

WR pp.34-36 employs a long obsolete sketch purportedly digitized from [IPC1990] used often by MM in non-peer-reviewed talks referenced, but never cited by the WR:

- McK05 p. 5, Figure 3.
- MM05x, p. 5, Figure 4, *the likeliest source*.

In any case, somewhere the sketch got distorted further. As shown in [SAI2007, p.8], Said had a correct copy of the sketch.

The WP did **not** get this directly from the 1990 IPCC, as per replies to questions from Rep Stupak :

[**BAR2006a**, p.37]:

“DR. WEGMAN. No, I have not been able to obtain a copy of the 1990 report. ...DR. WEGMAN. The temperature profile that was published in 1990 I believe was related to the European temperatures and was a cartoon-- essentially a cartoon.”

[**BAR2006a**, p.38], the story is slightly different:

“MR. STUPAK. I see. Or a cartoon.

DR. WEGMAN. The cartoon is IPCC's cartoon, not mine.

MR. STUPAK. You relied upon it though in your executive summary. So I am looking at the cartoon. There is no data, is there, to say that around 1300 it warmer than it is in the latter half of--

DR. WEGMAN. I think that is an inaccurate statement. I think there is data. I think the data--

MR. STUPAK. Do you have any of it? Can you show us where any of that is?

DR. WEGMAN. No, I don't have it. I take no responsibility for what IPCC did in 1990. There is no way I could do that. Their data is not available to me. In fact, **the reason it was digitized was that I had to go back and construct it from their picture. That doesn't mean no data exist. And in fact, as far as I know, it was based on European and Asian temperature profiles that were available in the 1990s.**”

<**B**> This is an odd approach for a senior statistician, to insist the data must exist because he has a cartoon from 1990, claim it included European and Asian temperature profiles, when it was a rough idea only of Central England, Theme-G⁴.

Bradley, et al (2003) explains the history, especially noting evidence against global synchronicity of the MWP, also in Cronin (1999), pp.300-302, Crowley and Lowery (2000) [IPC2001, p.13].

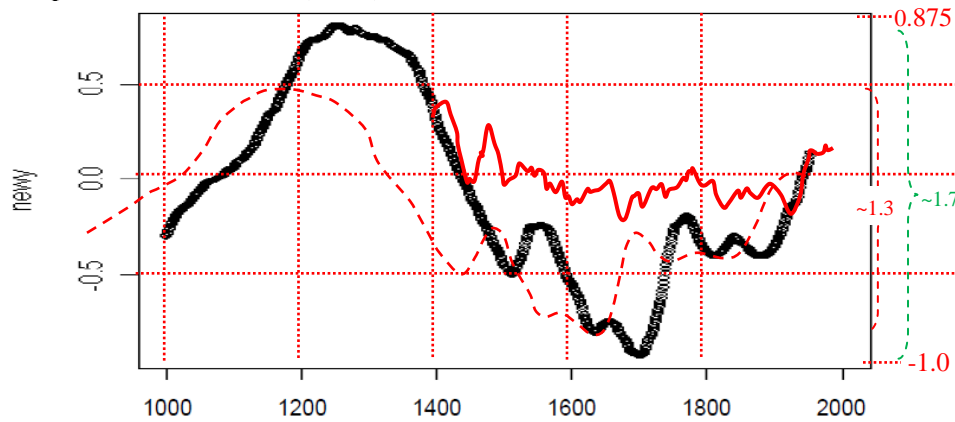
A more detailed explanation appears in Appendix A, p.36 in pubs.giss.nasa.gov/docs/2009/2009_Jones_et_al_2.pdf

“In summary, we show that the curve used by IPCC (1990) was locally representative (nominally of Central England) and not global, and was referred to at the time with the word ‘schematic’.

The WP did not read the 1990 IPCC, so never read the caveats.

In any case, a large, synchronous MWP had disappeared well before MBH98, §1.4. In fact, it was not in the IPCC 1992 Supplement: en.wikipedia.org/wiki/Description_of_the_Medieval_Warm_Period_and_Little_Ice_Age_in_IPCC_reports#1992_supplement

WR p.34 is shown below (black), with colored elements added here:



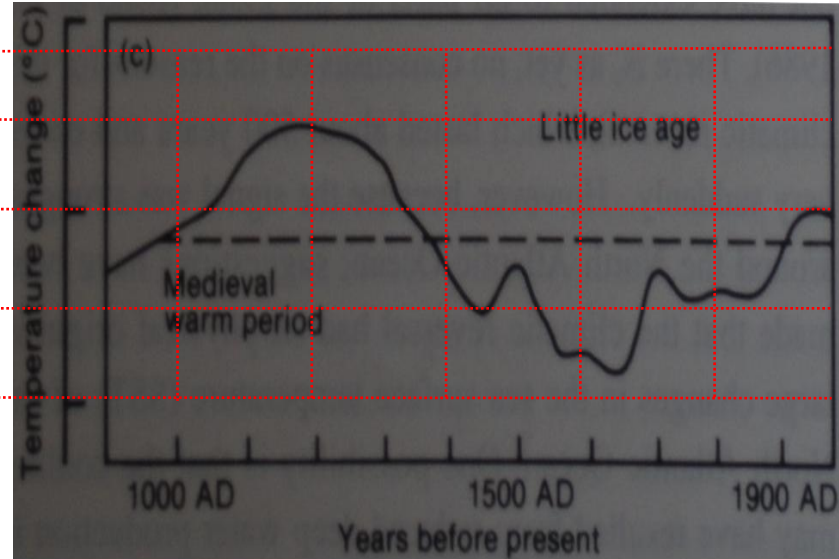
“Figure 4.5: Here we have digitized the temperature profile as presented in the IPCC Assessment Report 1990. The early period between 1100 to about 1400 of above average temperatures is known as the Medieval Warm Period and the period from about 1500 to 1900 is known as the Little Ice Age.

Discussion: In ... (repeats words above)... The 1990 report was not predicated on a global warming scenario. It is clear that at least in 1990, the Medieval Warm Period was thought to have temperatures considerably warmer than the present era.”¹

<ECB> The actual scanned image is at right, curve is overlaid in dashed red above. The solid red is from MM05x or MM03, W.4.4, Theme-A, Theme-G.
scienceblogs.com/stoat/2010/06/ipcc_1990_fig_71c_again.php

The IPCC curve is copied as the dashed red at left. To match the WR black line, the red must be shifted ~50 years later, shrunk slightly horizontally and expanded vertically ~30%. That is closer to MM03’s 1400AD, but still different in shape.

Even if one assumes that 0C change was indicated by the dashed line at right, the red dashed line above moves upward a bit, but it is still mislocated horizontally and distorted.



Ipcc-90-fig-7.1c

Caveat: this illustrates a problem with comparing anomaly charts, which show the change relative to the average for some set of years (baseline), set at 0C. Sometimes charts use different baselines. I have tried to be careful of that when superimposing curves, but this is all done by simple graphic editing, so errors may be found. Regardless of vertical displacements, shape differences are clear.

The WR gives no evidence that IPCC would have agreed with the underlined WR statement, but evidence exists otherwise:
en.wikipedia.org/wiki/Description_of_the_Medieval_Warm_Period_and_Little_Ice_Age_in_IPCC_reports
scienceblogs.com/stoat/2007/05/tggws_again_again.php

The WR does not actually contain the next chart, but supports it indirectly. Wegman mentions boreholes in [WEG2006c]. As boreholes are rather irrelevant to tree-ring studies and most reconstructions, this is another hint on McK05/MM905 sourcing. Wegman did not seem to understand the basic Greenhouse Effect, but found time to mention boreholes and the WR includes 3 Huang references, although no actual citations.

W.4.3 Boreholes and odd MM references

McK05 is a talk for an economics group in Australia, MM05x a talk for GMI. As had [ESS2002], both use the same chart shown below, on which

W.4.4 MM03/MM05x chart is overlaid in red. In the first, p.5-6 says: “Those wanting to “get rid of” the MWP run into the problem that it shows up strongly in the data. Shortly after Deming’s article appeared, a group led by Shaopeng Huang of the University of Michigan completed a major analysis of over 6,000 borehole records from every continent around the world. Their study went back 20,000 years. The portion covering the last millennium is shown in Figure 4. The similarity to the IPCC’s 1990 graph is obvious. The world experienced a “warm” interval in the medieval era that dwarfs 20th century changes. The present-day climate appears to be simply a recovery from the cold years of the “Little Ice Age.” Meme-32

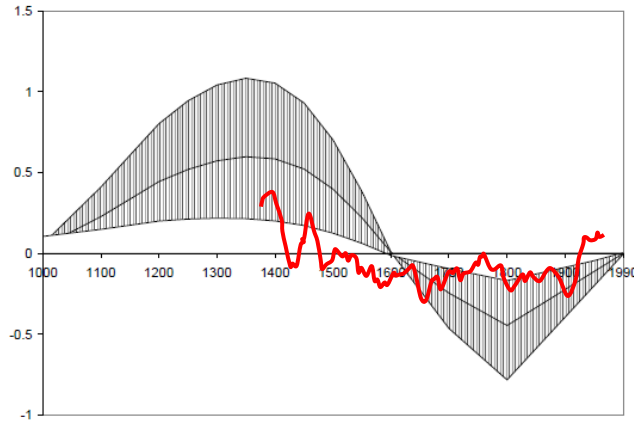


Figure 4. World Climate History after AD1,000 according to ground borehole evidence. Vertical axis: average anomalies in °C, with range indicating Bayesian probability boundaries. Source: Huang et al. (1998); data supplied by Huang.

“Huang and coauthors published their findings in Geophysical Research Letters in 1997. The next year, Nature published the first Mann hockey stick paper, commonly called “MBH98.”⁷ Mann et al. followed up in 1999 with a paper in GRL (“MBH99”) extending their results from AD1400 back to AD1000.⁸ In early 2000 the IPCC released the first draft of the TAR. The hockey stick was the only paleoclimate reconstruction shown in the Summary, and was the only one in the whole report to be singled out for repeated presentation. The borehole data received a brief mention in Chapter 2 but the Huang et al. graph was not shown. A small graph of borehole data was, taken

from another study and based on a smaller sample, was shown in a post-1500 segment, which, conveniently, trended upwards.”

Graphs are powerful. Would an economics audience recognize serious errors, Theme-A?

Researchers doubt the precise reconstruction usefulness of borehole data for much more than about 500 years. This was covered in [IPC2001, 2.3.2.1, p.132], the Chapter cited by the WR, and which McKittrick noted: www.grida.no/publications/other/ipcc_tar/?src=/climate/ipcc_tar/wg1/index.htm :

“Although large-scale temperature reconstructions have been made to more than a millennium ago (Huang et al., 1997), they show substantial sensitivity to assumptions that are needed to convert the temperature profiles to ground surface temperature changes. Borehole data are probably most useful for climate reconstructions over the last five centuries (Pollack et al., 1998).”

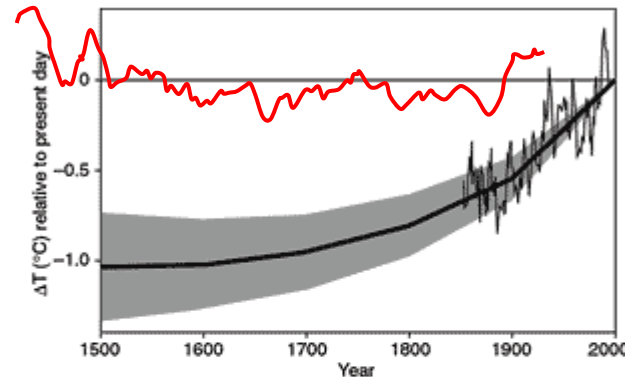


Figure 2.19: Reconstructed global ground temperature estimate from borehole data over the past five centuries, relative to present day. Shaded areas represent ± two standard errors about the mean history (Pollack et al., 1998). Superimposed is a smoothed (five-year running average) of the global surface air temperature instrumental record since 1860 (Jones and Briffa, 1992).

[IPC2001, Figure 2.19] above is overlaid with **W.4.4’s** MM03/MM05x chart in red, showing an even poorer match than the chart at left. I am not certain of identical reference lines, but that is irrelevant. By 1998: Henry N. Pollack, Shaopeng Huang, and Po-Yu Shen, “Climate Change Record in Subsurface Temperatures: A Global Perspective,” Science 9 October 1998 282: 279-281.

www.sciencemag.org/cgi/reprint/282/5387/279.pdf (paywall)

“The combination of the predominant depth range of observations and the characteristic magnitude of noise has led us to choose five centuries as the practical interval over which to develop climate reconstructions. ...

The geothermal reconstruction ... show that the 20th century is the warmest recent century and that the mean rate of temperature increase in the 20th century is well in excess of temperature trends of earlier centuries.”

McKittrick cites Huang, et al (1997) on the page following the 1990 IPCC chart in **W.4.2**. The same research group clarified the issue in Pollack, et al (1998), which McKittrick alluded to, but the WR did not cite. The WR referenced, but never cited the later Huang, et al (2000).

If there had been any doubt, this is explained in Huang, et al (2008), explaining why Deming was wrong:

www.geo.lsa.umich.edu/~shaopeng/2008GL034187.pdf

“In the ensuing debate **one of our publications** [Huang et al., 1997] (hereafter called HPS97) **was occasionally offered as evidence that the MWP was in fact warmer than late 20th century** [e.g., Deming, 2004]. ...

The fundamental difference between HPS97 and HPS00 is that they do not analyze the same data. Below we describe their respective datasets, and **show why the results of HPS97 cannot be used for comparing MWP warmth to the 20th century.**”

McK05, p.4:

“In the mid-1990s the use of ground boreholes as a clue to paleoclimate history was becoming well-established. In 1995 David Deming, a geoscientist at the University of Oklahoma, published a study in *Science*⁴ that demonstrated the technique by generating a 150-year climate history for North America. Here, in his own words, is what happened next.

‘With the publication of the article in *Science*, I gained significant credibility in the community of scientists working on climate change. They thought I was one of them, someone who would pervert science in the service of social and political causes. So one of them let his guard down. A major person working in the area of climate change and global warming sent me an astonishing email that said “We have to get rid of the Medieval Warm Period.”⁵

⁴ Deming, D. (1995). “Climatic Warming in North America: Analysis of Borehole Temperatures.” *Science* 268, 1576-1577.

⁵ David Deming (2005) “**Global Warming, the Politicization of Science, and Michael Crichton’s State of Fear.**” Forthcoming, *Journal of Scientific Exploration*, v.19, no.2.’

MM05x, p.4 lost the footnotes and changed the story:

‘Not too long ago, another borehole researcher published an essay describing some things that happened to him after he published a paper on this in 1995. He published a paper in *Science* reconstructing climatic conditions in North America based on borehole record and concluded in the paper that present conditions still appeared to be within the range of natural variability. In his essay he comments,

“With the publication of the article in *Science* [in 1995], I gained significant credibility in the community of scientists working on climate change. They thought I was one of them, someone who would pervert science in the service of social and political causes. So one of them let his guard down. A major person working in the area of climate change and global warming sent me an astonishing email that said, “We have to get rid of the Medieval Warm Period.” – D. Denning (sic), *Science* 1995.’

A casual reader might easily be misled by the reference to *Science*.

JSE disappeared, but the Meme remained, to this day.⁵⁹

For example, [MON2010, pp.17-30] relies heavily on Deming and his JSE quotes, likely via McK05.

The JSE, Deming’s article and some climate anti-science background are

www.scientificexploration.org/journal/articles.html⁶⁰

www.scientificexploration.org/journal/jse_19_2_deming.pdf
en.wikipedia.org/wiki/David_Deming

[**WEG2006c**, p13] Wegman writes:

“results of the MBH methodology does not coincide with the results of other methods such as borehole methods...”

How strong was Wegman’s borehole expertise?

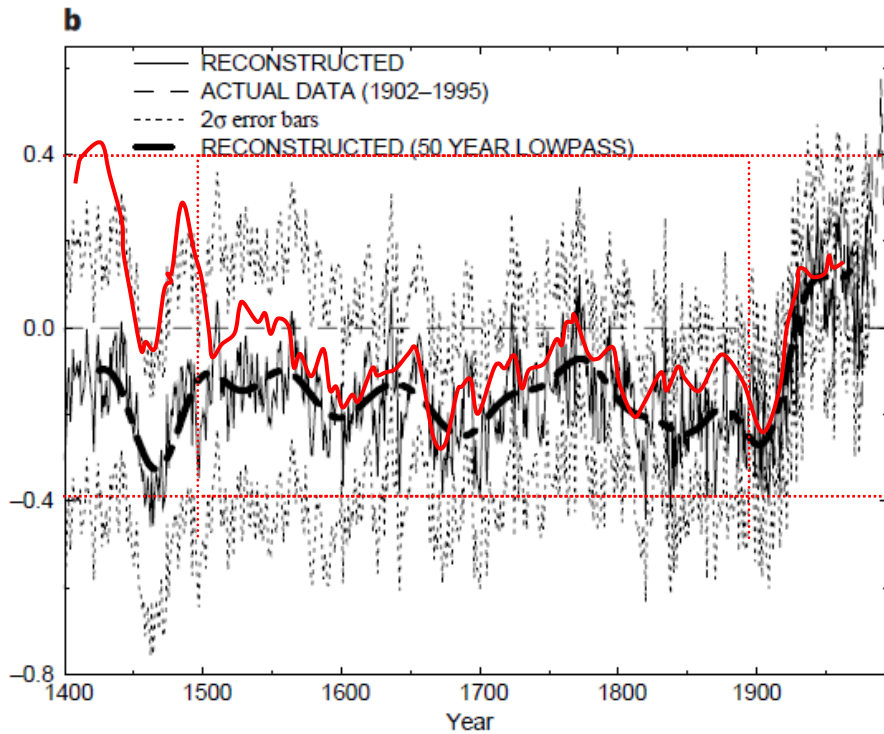
⁵⁹ Like the Cheshire cat in *Alice in Wonderland*, the cat is gone, but the grin stays, floating in air with no support. “Off with its head” didn’t work, since it had no body. [MON2010, p.421] shows that Jonathan Overpeck, supposed writer of the email to Deming strongly disavowed it. If it ever existed, Deming had not kept a copy, and even if it existed, context-less email is prone to misinterpretation.

⁶⁰ A quick perusal of the first calibrates JSE, with papers like “An Empirical Study of Some Astrological Factors in Relation to Dog Behaviour Differences by Statistical Analysis and Compared with Human Characteristics.” www.scientificexploration.org/journal/jse_21_2_braesch.pdf

W.4.4 Odd graphs in MM03, MM05x

The WR places great emphasis on various MM papers. Without going into the endless statistical arguments, it is worth comparing graphs,

Figure 5.b [MBH98, p.783], with MM03/MM05x line added in red.



MBH98 shows error bars, as usually done in real science. MBH98 and MBH99 are well-caveated papers with serious discussions of uncertainty. Scientists argue about the sizes of error bars and the central lines of different reconstructions. Progress occurs by narrowing the error bars and resolving real differences among reconstructions, not ones caused merely by use of different data sets in different geographies. Consistency is not a binary decision, but generally, a reconstruction is consistent with another if it falls within the latter’s error bars. The red line is well within the black error bars, except pre-1500AD, and especially pre-1450AD, where it is at the edge or beyond. There they disagree. New data might help choose between them, and much more data is actually available.

The graph was copied by tracing to show general placement.

Following is MM05x, Figure 6, p.8, colored version of Figure 8 MM03, p.766, scaled to match MBH98 graph at left, overlaid with red grid at +/-0.4C, AD 1500, AD 1900 for placement.

McKittrick (2005), Fig. 8, p.14 is ~similar, although with an odd scale.

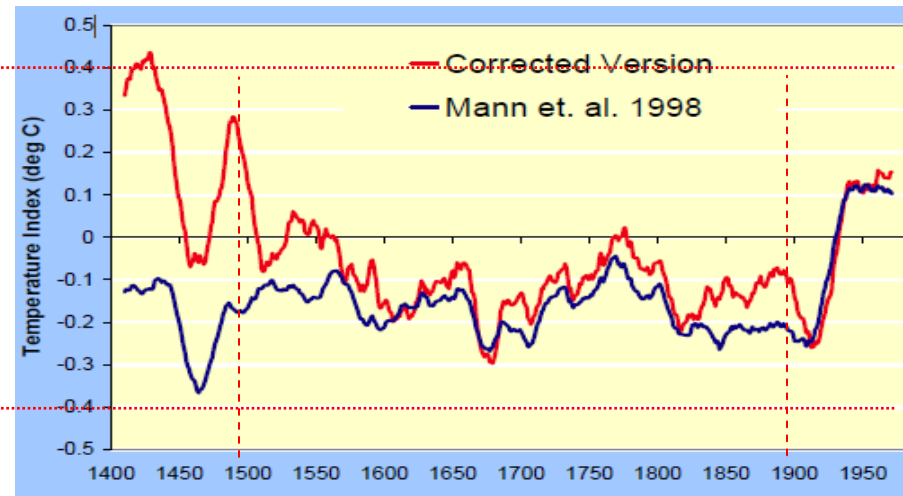


Figure 6

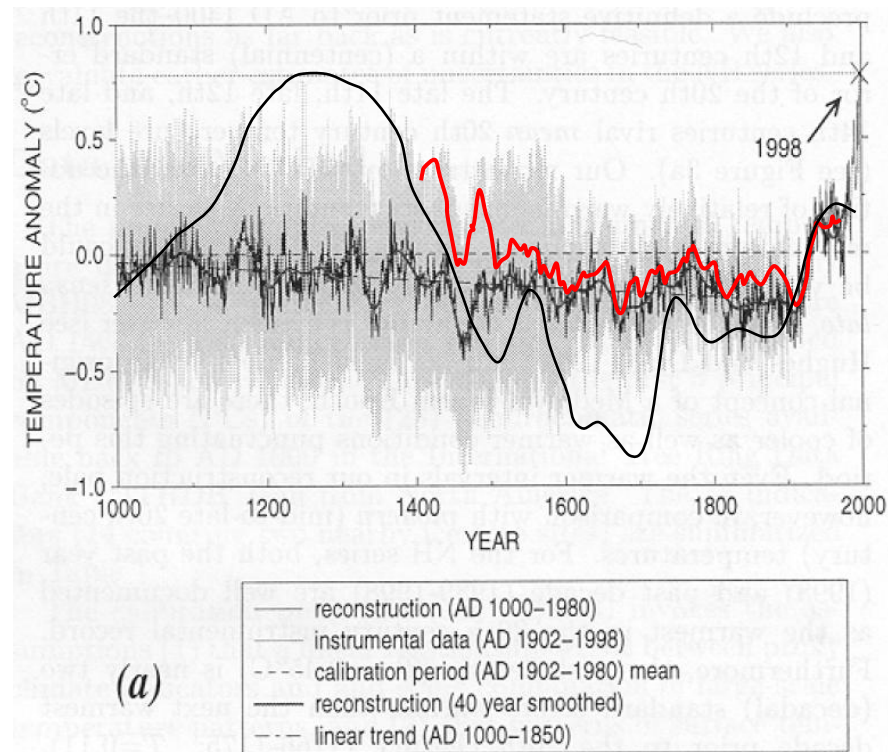
The WR, pp. 75-76 says:

“Having accounted for the major errors, MM03 reconstructed the temperature history. Using the MBH98 methodology, they were able to accurately reproduce the “hockey stick” shaped graph in the MBH98 findings. Still using the same basic methodology, MM03 prepared the data with improved quality control, including using the most recent data and collating it correctly. **The result was a northern hemisphere temperature reconstruction that takes on a different shape in which the temperature index peaks at around 1450 AD, near the earliest measured point on the graph.** MM03 concluded that the errors in MBH98 make the data unreliable and obsolete such that it does not support their end conclusions.”

The WR strongly supports (but does not actually show) this graph. It noticeably contradicts every other credible source, including many cited in the WR. It supports Meme-56. This graph is overlaid on others following to make this clear.

Following is MBH99, Fig. 3, p.761, MM03 overlaid in red. MM03 focused so heavily on MBH98, already 4+ years old and already superceded by MBH99, in which 1400AD was no longer the edge.

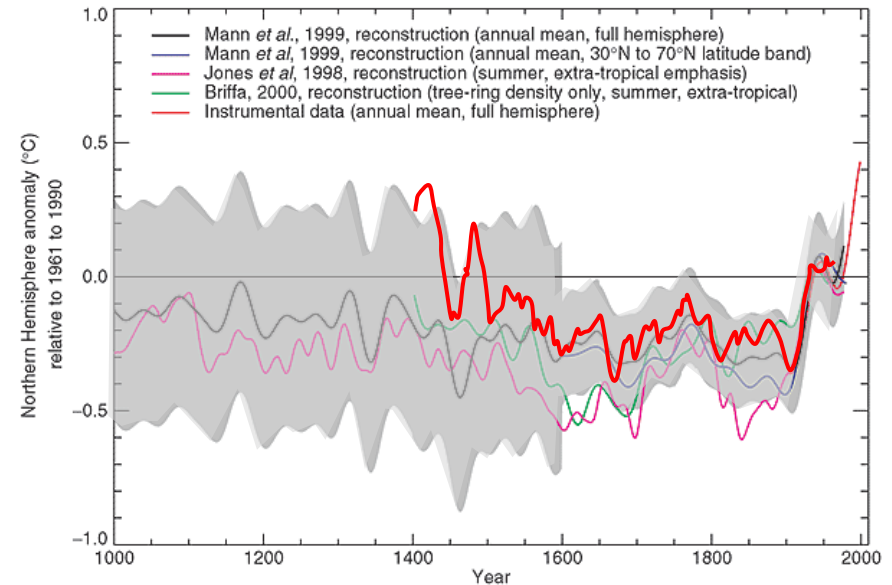
The black line overlays the WR graph shown in W.4.2. The reader can see why that sketch was long ago discarded by professionals. It is also clear that the red and black lines agree poorly although the changes described in W.4.2 moved the black line closer to the red in 1400AD-1500AD. The black line is shown for simplicity only here. (Be careful of baseline changes, the one at right is higher than the one below.)



Following is Figure 2.21 from [IPCC TAR (2001), p.134], MM03 overlaid in red:

www.grida.no/publications/other/ipcc_tar/?src=/climate/ipcc_tar/wg1/index.htm

The WR cited this section of the IPCC TAR. The MM03 chart is an outlier compared to the other reconstructions, especially before 1500AD.



Of course, the WP might argue that MM03 was right, and the rest flawed, especially as several were MBH reconstructions. However, additional reconstructions had already been done by various other teams, well before the WR, as shown on next page.

This had been covered at RealClimate January 2005:
www.realclimate.org/index.php/archives/2005/01/peer-review-ii
 The WR listed RealClimate in its Bibliography.

Finally, Wahl and Ammann (2006) showed clear problems (W.8.4) with the MM03 analysis, but the WP dismissed it in a footnote. More discussion was provided in questioning [WEG2006c].

This is Figure 6.10, [AR4 WG I (2007), p.467] with MM03/MM05x overlaid (red), and uncertainty limits (grey) from previous chart. Some people call this the “spaghetti graph.”

www.ipcc.ch/pdf/assessment-report/ar4/wg1/ar4-wg1-chapter6.pdf

Of course, this appeared after the WR, but most of the papers were published before, as was the AR4 Second Order Draft [IPC12006]. The WR cited 7 of the 12 reconstructions, with “Important papers” in **Bold:**

MBH1999 = Mann, Bradley, Hughes, MBH99

BOS..2001 = Briffa et al (2001)

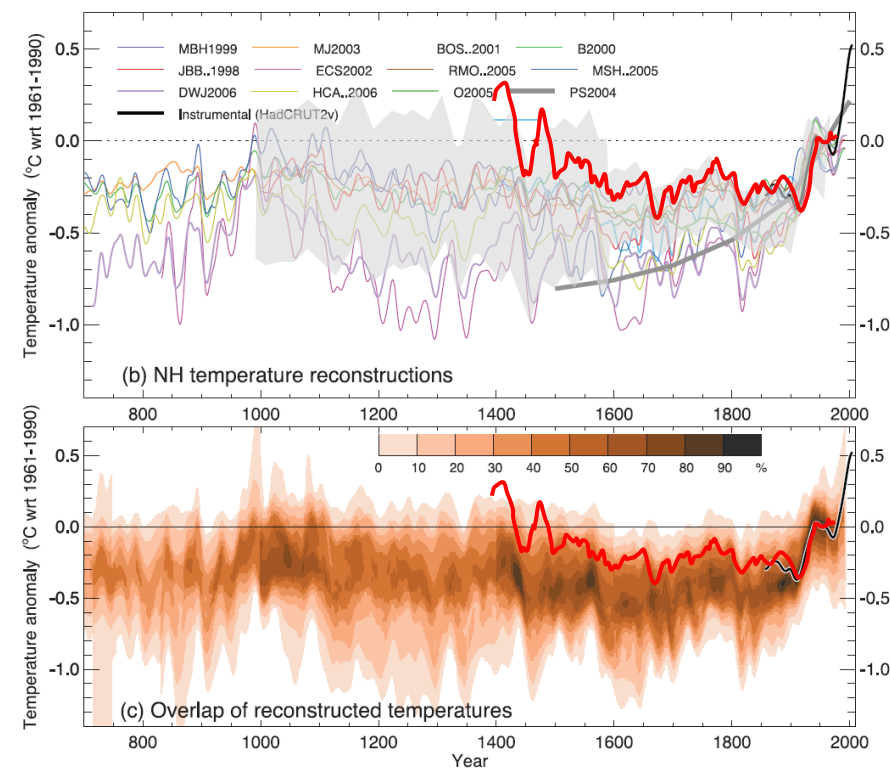
ECS2002 = Esper, et al (2002)

MJ2003 = Mann, Jones (2003)

RMO..2005 = Rutherford, et al (2005)

MSH..2005 = Moberg, et al (2005)

DWJ2006 = D’Arrigo, et al (2006)



In the top chart, MM03/MM05x is at outlier, especially pre-1500AD. The top chart can be misleading to the casual reader:

- Different reconstructions cover different geographies, and in particular, those focused on (land-dominated) NH extratropics are expected to vary more than the entire NH, which in turn varies more than global.
- Human eyes tend to notice the outer edges of the spaghetti graph more than the density of lines between.

The lower chart is in some ways better, but requires more sophisticated graphics software. It illustrates the overlap and consistency amongst the various reconstructions - darker color shows more agreement. This avoids confusion caused by seeing two discrete lines and thinking they really disagree, when both are well within each other’s error-bars. It is nontrivial to understand a graph where many lines have their own error bars. The darkest areas still look like a hockey stick, and they still mostly fit within the MBH uncertainty limits. If anything, MBH allowed for a higher MWP than many studies.

The WR simply accepts MM03, despite its disagreement with the other studies available at the time. Consistent with numerous errors, they never contacted Mann or anyone else in paleoclimate [WEG2006c, p.7]:

“Ans: I spoke with no one in paleoclimate studies. To the best of my knowledge neither have my colleagues.”

Nevertheless, the WR strongly preferred MM results to those not just of MBH, but of an entire field.

The WP devotes many pages to human (not computer) social network analysis, a field in which they had little or no prior experience, to discredit the field via coauthorship overlap, Meme-b. They spend much effort to show overlap of proxies, Meme-c. They are strongly critical of statistical expertise in paleoclimate. However, in basic data analysis, people rarely use an extreme outlier in preference to the bulk of the data, unless they have strong expertise and articulate clear reasons for doing so. The WR devotes pages to the graph in W.4.2, but also strongly supports the graph from W.4.4, which contradicts that. Both contradict the reconstructions shown in W.4.4 and W.4.3.

Conclusion: <EB>. Meme-05, Meme-18, Meme-a, Theme-J.

W.5 Social Networks in WR and successors

Theme-M₄ emerged from continual hints that Wegman's group appeared unfamiliar with social network analysis (SNA), including coauthorship studies. The whole SNA effort in the WR seems *strange*, a topic far beyond the claimed focus, but it supports the real mission #2.

W.5.1 Introduction to relevant fields

Graph theory, network theory, social networks

Graph theory, network theory and cluster analysis have long histories and wide applicability.⁶¹

en.wikipedia.org/wiki/Graph_theory en.wikipedia.org/wiki/Network_theory

en.wikipedia.org/wiki/Cluster_analysis

Social network analysis (SNA) uses some of the same underlying mathematics. For instance, [DEN2005] references graph theorist Frank Harary. [WAS1994] references him and Claude Berge, often.

en.wikipedia.org/wiki/Social_network

SNA normally applies to human issues, sometimes with different terminology than when the same mathematics is used in other disciplines. Human social networks include multiple kinds of relationships, as clearly shown in these books, sometimes paraphrased. Many people employ informal SNA, especially when dealing with large organizations, politics or academic research networks. At Bell Labs, when trying to accelerate technology diffusion, we looked for networks of “technology gatekeepers.”

⁶¹ I first heard about graph theory applications in telephone networks around 1963, while in high school, from Henry Pollak, long-time Director of the Mathematics and Statistics Center at Bell Laboratories, where I worked 1973-1983. In college, my 2 graph theory courses used (famous) textbooks I still own: Frank Harary*, *Graph Theory* (1969) and Claude Berge*, *The Theory of Graphs* (1962). Berge was a Visiting Professor and taught the 2nd course.

Pollak's Center included Ronald Graham*, Edward G. Coffman (who I'd known in college) and many strong statisticians. The Associate Executive Director was John Tukey. The *'d names can be found in the Wikipedia graph theory page. I also knew Al Aho and Ravi Sethi, who often did graph-theory applications in computing. Of course many network analysis techniques were developed at Bell Labs. I claim no particular expertise, just note these topics are long developed and widely used to describe computers and communication systems, among others.

We sometimes designed organizations and building layouts to take advantage of or encourage unofficial relationships. Many others do these also. Those who sell expensive products (such as supercomputers) to large organizations are often superb practical analysts adept at finding crucial informal relationships.⁶² Of course, the rise of software like LinkedIn and Facebook has added a specific idea of “social networks” to the lexicon of the general public, whether or not that fits the formal term very well.

Co-authorship network, one relationship among many

A co-authorship relationship is only one among many, including some that may be much stronger, and sometimes asymmetric:

Thesis director : PhD student (often strong and long-lasting)

Editor: reviewer

Book editor : chapter author (not same as previous)

Conference session organizer : invited speaker

Joint committee membership

Joint department membership

And many others, including spousal or geographic neighbor relationships

A strong, long-lasting set of relationships is shown in **A.6**.

The Mathematics Genealogy Project tracks professor/student connections. Wegman is shown, for example.

genealogy.math.ndsu.nodak.edu/index.php

genealogy.math.ndsu.nodak.edu/id.php?id=41964

Mathematicians famously compute their “Erdős Number” or distance from mathematician Paul Erdős via coauthorship chains.⁶³

www.oakland.edu/enp

Finally, sociologists and other scientists have studied science coauthorship networks and citation analysis for decades, certainly back into the 1960s. Relationships are clear, well-recorded and public, requiring no time-consuming fieldwork or interviews. For example, each of the following obvious searches currently yields nearly 7,000 hits:

⁶² In one sales situation, it certainly mattered that the CEO of our prospect had bought his current home from the CEO of our competitor.

⁶³ Erdős (0) coauthored with Ron Graham (1), who coauthored with Shen Lin (2), who coauthored with Brian Kernighan (3). I've coauthored papers with Brian, which would rate me (4), but sadly, do not count as they were not math papers.

Google Scholar: social network co-authorship
scholar.google.com/scholar?q=social+network+co-authorship&hl=en&btnG=Search&as_sdt=2001&as_sdt=on

Google Scholar: co-authorship network
scholar.google.com/scholar?hl=en&q=co-authorship+network&btnG=Search&as_sdt=2000&as_ylo=&as_vis=0

At or near the top of each list is:

Barabasi, Jeong, Neda, Ravasz, Schubert and Vicsek, “Evolution of the social network of scientific collaborations,”

[Physica A: Statistical Mechanics and its Applications, Volume 311, Issues 3-4, 15 August 2002, Pages 590-614.](#)⁶⁴

arxiv.org/PS_cache/cond-mat/pdf/0104/0104162v1.pdf

Google Scholar shows 771 citations, not always accurate, but a hint. This is a sample of serious SNA. Coauthorship is a very well-studied topic.

This seemed not well understood by Wegman’s group in 2005-2006.

W.5.2 WP unfamiliarity with SNA

It is difficult to find evidence that Wegman and his team had much, if any pre-WR experience with SNA and especially coauthorship studies.

Generalists may know some widely used mathematical and computing techniques without knowing the literature and terminology of a specific field that uses such techniques. Occasionally, they may jump into an application field, but study the literature insufficiently to be able to produce credible, impactful results. They may reinvent techniques already widely used there, whereas specialists may tend to the inverse, reinventing mathematical techniques well-known in other fields.⁶⁵ The old adage

⁶⁴ Thanks to Garry Robins for pointing me at this specific reference.

⁶⁵ Clustering has a long history of unconnected research groups developing equivalent algorithms, but with different terminology. See W.5.5. Roger K. Blashfield, Mark S. Allenderfer, “The Literature on Cluster Analysis,” 1978, DOI: 10.1207/s15327906mbr1303_2 .
www.informaworld.com/smpp/content~db=all~content=a785042873
en.wikipedia.org/wiki/Cluster_analysis

“absence of evidence is not evidence of absence” applies. However, the combination of plagiarism of basic texts, of errors and plausible searches that find no positive evidence of knowledge all consistently argue for lack of prior experience.

Absence of evidence

Rigsby, Solka (2004) [RIG2004] – Not really SNA Paper

Although this references a related 2003 paper by the same authors, this was the earliest SNA-terminology I could easily obtain by anyone involved with the WR. But it is not really an SNA paper. It uses human social networking terminology (like “actor”) to analyze computer networks, where most people might use “node.” This is not apriori incorrect, but it seems that [RIG2004] picked up tools and terminology from SNA and applied them to computer networking analysis, a field with a long history and terminology of its own in using similar techniques. It does not increase confidence in a scholar’s familiarity with the literature in either discipline. The only SNA reference in that paper is [WAS1994], rather than papers comparing the effectiveness of different algorithms.

I kept looking for human networks, given all the mention of actors. Rigsby invents the idea of “allegiance,” used repeatedly in [WR, SAI2008, SHA2006], but not generally referenced by others. Is this a new idea or a reinvention repeatedly referenced mainly by associates? I do not know, but applicable clustering and factor analysis algorithms have a long history.⁶⁶

Their book, *Cluster Analysis: Quantitative Applications in the Social Sciences* (1984) is referenced in [DEN200, WAS1994]. Blashfield also provided the pointer to Hartigan’s book.

⁶⁶John Hartigan, *Clustering Algorithms* (1975) is a hugely-cited book. Among other things, it evaluates different clustering techniques, including blockmodels, of which coauthorship examples are relatively simple (binary symmetric matrices.) That is out of print, but I easily found Brieger, Boorman, Arabie, “An Algorithm for Blocking Relational Data, with Applications to Social Network Analysis and Comparison with Multidimensional Scaling, December 13, 1974, Technical Report 244, Stanford University. It discusses blockmodels and SNA algorithms.
suppes-corpus.stanford.edu/techreports/IMSSS_244.pdf
 Many algorithms for evaluating goodness of partitioning blockmodels date from the 1970s or earlier.

Nobody expects MS students to “know everything.” So, Rigsby’s work may be perfectly fine, but his “allegiance” idea might also just be a reinvention of a 50-year-old idea⁶⁷ Professors normally expect students to include relevant references, in part to assure readers of authors’ familiarity with the field. PhD dissertation committees are hopefully constructed to assure this as well. But [SAI2008, SHA2006, SHA2008] all display poor citation practices that sometimes list textbooks and methods books, but seem less strong on current refereed field papers.

Said dissertation (2005) [SAI2005]

“Social network” appears 6 times, used in a general sense. It references the paper Snijders (2001)⁶⁸, but has no other SNA references or obvious discussions.

Wegman C.V.

www.galaxy.gmu.edu/stats/faculty/wegman.resume2.pdf

“Social network (s)” appears only once (#143, April 2008). Of course, SNA material was included in [WR, Weg2006c, SHA2006, SAI2008], and he supervised Rigsby’s 2005 MS thesis [RIG2005]. If that expanded on [RIG2004], it seems likely to have been analysis of computers, not (human) SNA.

⁶⁷ “Allegiance” appears to be a metric for “subgroup cohesion,” as discussed in [WAS1994, pp.270-271], which measures relative strength of ties within a subgroup and between subgroups. It references Bock and Husain (1950), who proposed an iterative mechanism for constructing subgroups.

Google Scholar: cohesive subgroups yields ~26,000 hits.

Google Scholar: cohesive subgroups iterative algorithm yields ~1,100 hits.

Perhaps the Rigsby MS includes such references, and comparisons to explain why this is interesting, but it is disconcerting to find so many successive references to “allegiance” when there may well exist similar metrics known in the field.

[SHA2008] actually uses the standard “cohesive subgroups” terminology.

⁶⁸ T. Snijders is the Co-Editor of *Social Networks*, the Elsevier journal more appropriate for [SAI2008], **A5.6.1**.

Sharabati publications

web.ics.purdue.edu/~wsharaba/publications.htm

No SNA-relevant publications appear prior to [SHA2006].

Evidence of absence

The remaining issues are strong evidence of unfamiliarity. In light of the earlier discussion, the following is an astonishing claim:

[SHA2006, p.2], part of Wegman’s reply to Rep. Stupak:

“Of all the work that has been done on social networks, very few investigators have considered coauthorship network. Therefore, what we are about to observe in this paper is a brand new approach in this field.”

This looks to be substantial work under way for some time, not just created in a week or two. It cites [DEN2005, WAS1994], but no other papers from the field. It thanks Wegman, Tsetovat and Said for advice.

Did nobody question this statement? It is absurd.

[SHA2008, pp.9-10] repeats the first sentence, not the second. It actually references another Barabasi paper, but not the relevant, strongly cited paper mentioned earlier.

The plagiarism chain

Of course, the entire plagiarism chain shown in **W.2.3** is very strong evidence of field unfamiliarity. Experts need not do this.

Of course, we still do not know who did it, just that it happened.

W.5.3 Social networks analysis in WR

WR p.38 starts:

“One of the interesting questions associated with the ‘hockey stick controversy’ are the relationships among the authors and consequently how confident one can be in the peer review process. In particular, if there is a tight relationship among the authors and there are not a large number of individuals engaged in a particular topic area, then one may suspect that the peer review process does not fully vet papers before they are published. Indeed, a common practice among associate editors for scholarly journals is to look in the list of references for a submitted paper to see who else is writing in a given area and thus who might legitimately be called on to provide knowledgeable peer review. Of course, if a given discipline area is small and the authors in the area are tightly coupled, then this process is likely to turn up very sympathetic referees. These referees may have coauthored other papers with a given author. They may believe they know that author’s other writings well enough that errors can continue to propagate and indeed be reinforced.”

This is Meme-b¹, with help from Meme-g¹, §1.7, clearly an outgrowth of the [GMI2003], MM05x sequence. Any of this might be easily be true, but it is presented with zero evidence that paleoclimate actually suffers from the problem. In fact, from the Climategate emails, quite vigorous arguments often occurred [RUS2010]. The toughest reviewers are quite often colleagues.⁶⁹

 This discussion in effect attacks paleoclimate by innuendo, especially ironic given the obvious abuses of relationships and review processes in which Wegman has been involved, **A.1**, **W.5.6**.

WR pp.39-45

This simply offers graphs, showing that Mann unsurprisingly writes papers with his coauthors. Attention is focused especially on Mann, despite the fact that all this is embedded in a much bigger network, . Meme-d¹. They do some analysis of larger networks, but it is of course irrelevant anyway, as this has nothing to do with Mann’s network when doing MBH98/99.

⁶⁹ As related in [MAS2010, A.10.4], Bell Labs internal peer review was often considered stronger than external peer review. None of us wanted bad papers to appear with a Bell Labs affiliation on them. Comments could be fierce, and since they were transmitted through one’s management chain, they mattered. It is less clear that one’s graduate students are tough reviewers.

WR pp.46-47

These have nothing to do with SNA, **W.5.8**, **W.5.9**.

W.5.4 Testimony

[BAR2006a, p.40]

“DR. WEGMAN ... As mentioned earlier, I think for one person to have 43 coauthors is an unusually large number of coauthors. I personally believe that I probably have maybe 15 people that I have worked with over the years.”

Wegman had criticized Mann for the number of coauthors. Wegman’s statement is easily seen to be wrong from his C.V. [WEG2010], or clearly here in **A.6.1**, Theme-N¹:

Within a few weeks, Wegman stated that he had 101 coauthors, as shown in [SHA2006], work that seems likely to have been in progress well before his statement about 15 authors.

W.5.5 Wegman Reply to Stupak

[WEG2006c, p.2]

“Social network analysis is a powerful tool with a more than 50-year history of making obvious potentially hidden social relationships.”

That is true, *but one must ask why the WP jumped into SNA without knowing much about it, thinking coauthorship analysis was new.*

[WEG2006c, p.6]

“Statisticians, like computer scientists and mathematicians, have the experience of applicability to many fields and, hence, can bring to the table interdisciplinary experiences that many disciplines cannot.”

Statistics is one the most widely applicable disciplines, but from the discussion in **W.5.1**, *overconfident* generalists can sometimes waste enormous effort when they think they understand some application field, but have merely gotten a superficial introduction. Good statisticians do not do this (**A.8**), but spend the necessary time to learn enough to contribute. The reader might consult the Index for the cases in which the WP exhibited the many Themes, showing frequent lack of knowledge of physics, climate science, sociology or even basic research practice.

This error seems repeated by McShane, Wyner, **A.12**.

[WEG2006c, p.10]

J “Does your report include a recalculation of the MBH98 and MBH99 results using the CFR methodology and all the proxies used in MBH98 and MBH99, but properly centering the data?”

Ans: Our report does not include the recalculation of MBH98 and MBH99. We were not asked nor were we funded to do this. We did not need to do a recalculation to observe that the basic CFR methodology was flawed. We demonstrated this mathematically in Appendix A of the Wegman et al. Report. The duplication of several years of funded research of several paleoclimate scientists by several statisticians doing pro bono work for Congress is not a reasonable task to ask of us. We all have additional responsibilities to the people and agencies that pay our salaries.”

This sounds good, but it is Meme-h, Theme-K.

What is the point of doing all this work if one does not know whether or not the errors matter? This is akin to saying that computation of a mean is incorrect because 1 of 100 data points was omitted. It is incorrect, but does it really matter? Possibly, but usually not, unless the missing point is a real outlier. In any case, the WP did much work that was completely irrelevant to the claimed mission #1. This often seems used to evade awkward questions. Of course, it seems unlikely that Barton and Whitfield would have wished even the slightest chance that statisticians might actually evaluate MBH, rather than ratifying MM.

[SHA2006, starts PDF p.17, numbering restarts at 1.]

“2. Literature review...

Of all the work that has been done on social networks, very few investigators have considered coauthorship network. Therefore, what we are about to observe in this paper is a brand new approach in the social networks field.”

Usually a literature review reviews the literature. This did not.

As noted in **W.5.1**, this claim is simply ludicrous, given the wealth of co-authorship studies in the literature.

“We can also argue on the quality versus quantity of the publications. Wegman favored quality of the work rather than quantity.”

As for quality, one might review [SAI2008] in **W.2.3** and the next section.

W.5.6 Said, Wegman, Sharabati, Rigsby

This section is mostly from [DEE2010f], but adds some more information and consolidates comments on this rare “self-refuting” paper.

W.5.6.1 Background

[SAI2008] Yasmin H. Said, Edward J. Wegman, Walid K. Sharabati, John T. Rigsby, “Social networks of author–coauthor relationships,” *Computational Statistics & Data Analysis* 52 (2008) 2177 – 2184.

Received 8 July 2007; accepted 14 July 2007.

web.ics.purdue.edu/~wsharaba/SNA/Author-Coauthor%20Relationships.pdf

CSDA – Computational Statistics and Data Analysis (Elsevier)

www.elsevier.com/wps/find/journaldescription.cws_home/505539/description#description, Impact Factor 1.228.

The Editorial Board on 01/04/07 was:

web.archive.org/web/20070104023234/www.elsevier.com/wps/find/journaldescription.cws_home/505539/editorialboard

Through at least mid-2008 entries included:

“Advisory Board:

E.J. Wegman George Mason University, Fairfax, VA, USA
Nonparametric Function Estimation, Time Series Methods, High Interaction Statistical Graphics and Methods, Methods for Parallel Computation...

Associate Editors:...

C.E. Priebe John Hopkins University, Baltimore, MD, USA
Kernel and Mixture Estimates, Statistical Pattern Recognition, Statistical Image Analysis...

Y.H. Said 206 Crabb Avenue, Rockville, MD, USA
Biostatistics, Epidemiology, Public Health, Statistical Modeling and Graphics, Adaptive Design, Social Network Theory, Data Mining, Time Series Analysis, Computer Intrusion Detection, Climatology, Metadata”

According to [WEG2005], Wegman had been involved with CSDA since 1986 in various roles. Carey Priebe earned his PhD under Wegman in 1993, coauthored papers with him in 1997, 1998, 2004 (2), and wrote a book chapter for a book edited by Wegman and Jeffrey Solka, coauthor of [RIG2004], a 1995 PhD of Wegman’s, and a member of Said’s PhD Committee. Said earned her PhD Spring 2005, taught 2005-2006 in same JHU department as Priebe, then returned to GMU. Ted Kirkpatrick writes: deepclimate.org/2010/04/22/wegman-and-saids-social-network-sources-more-dubious-scholarship/#comment-3389

“Said et al. (2008) article is fishy in a number of ways. It purports to be about an “emerging tool”, yet the only references it gives are two unrefereed articles by the authors of this paper, a 34-year old article in a sociology journal, and a 13-year old textbook. Not a single citation from a statistical forum of a recent *refereed* article on this “key technique”...

Yet when this lightly-referenced, weakly-written article was submitted, it was accepted without revision in just six days. Contrast this with other articles in the same issue of that journal. Selecting five at random, all had considerably more references, all had recent references from the refereed statistical literature, and all required revision before acceptance. What was so special about this article?.. Said’s list, however, is an absolute grab-bag:... **More importantly, this is far too diverse a range of topics for a new scholar (two years out from her Ph.D.).** Bear in mind that topic lists for Associate Editors are given to indicate areas where they have sufficient expertise to select referees, evaluate their reports, and make a final publication decision. This is a higher level of expertise than required for listing as an interest on one’s professional Web page. **It defies likelihood that any young scholar could already have sufficient expertise to act as Associate Editor for such a diverse range of topics. Said is the only Associate Editor at the time who listed social networks—or “social” anything—in her interest list.** Presumably, her article was given to some other editor to handle for review, but it’s not clear that any of the possible editors knew the topic area well.”

Said was not on this board very long. Ted adds more useful commentary: deepclimate.org/2010/04/22/wegman-and-saids-social-network-sources-more-dubious-scholarship/#comment-3398

CSDA seems to publish little, if any, SNA, normally the purview of another Elsevier journal, *Social Networks*.

www.elsevier.com/wps/find/journaldescription.cws_home/505596/description#description Impact Factor 2.439 (higher than CSDA).

Social Networks’ Editorial Board at the time was:

web.archive.org/web/20070705113453/www.elsevier.com/wps/find/journaldescription.cws_home/505596/editorialboard

One Editor was P. Doreian of University of Pittsburgh, who has coauthored with Mrvar and Batelgij, as has Associate Editor F. Ferligoj, a colleague of theirs in Slovenia. Another Associate Editor was K. Faust. Such people might not have accepted this paper so easily, and might well have recognized some familiar text.

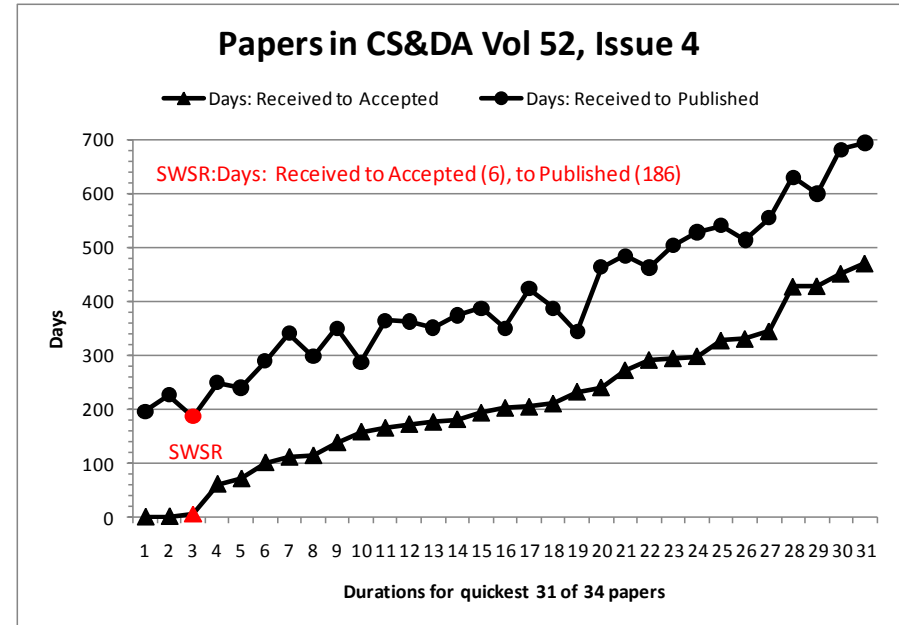
CSDA Back issues can be found at

www.sciencedirect.com/science/journal/01679473

I checked the article abstracts for dates and assembled the following, sorted by delay from Received to Accepted and graphed at right.

Computational Statistics and Data Analysis
Vol 52, Issue 4, Pages 1765-2272 (10 January 2008)

#	Author	Recvd	Revised	Accepted	Online	Published	Acc-Recvd Days	Publ-Recvd Days	
20	Gomes	06/28/07		06/28/07	07/05/07	01/10/08	0	196	
7	Timmerma	05/29/07		05/30/07	06/04/07	01/10/08	1	226	
29	Said	07/08/07		07/14/07	08/09/07	01/10/08	6	186	
27	Mandel	05/06/07	06/14/07	07/06/07	07/20/07	01/10/08	61	249	
34	Miao	05/15/07	07/25/07	07/25/07	08/07/07	01/10/08	71	240	
24	Mulekar	03/27/07	07/05/07	07/06/07	07/25/07	01/10/08	101	289	
6	Sarhan	02/04/07	05/14/07	05/26/07	06/04/07	01/10/08	111	340	
25	Krnjajic	03/18/07	07/09/07	07/10/07	07/27/07	01/10/08	114	298	
12	Orelien	01/25/07	06/06/07	06/12/07	06/14/07	01/10/08	138	350	
3	Jasra	03/29/07	09/03/07	09/03/07	09/21/07	01/10/08	158	287	
17	Karvanen	01/11/07	06/25/07	06/25/07	07/03/07	01/10/08	165	364	
23	Leiva	01/13/07	07/04/07	07/04/07	07/16/07	01/10/08	172	362	
31	San Martin	01/24/07	07/16/07	07/19/07	07/31/07	01/10/08	176	351	
22	Al-Saleh	01/01/07	06/30/07	07/01/07	07/10/07	01/10/08	181	374	
21	Cheng	12/19/06	04/09/07	06/30/07	07/19/07	01/10/08	193	387	
35	Archer	01/25/07	08/15/07	08/15/07	08/30/07	01/10/08	202	350	
10	Kundu	11/13/06	06/01/07	06/05/07	06/13/07	01/10/08	204	423	
30	Coin	12/19/06	07/17/07	07/17/07	07/31/07	01/10/08	210	387	
36	Demirtas	02/01/07	07/14/07	09/21/07	09/26/07	01/10/08	232	343	
8	Danneman	10/04/06	05/07/07	06/01/07	06/07/07	01/10/08	240	463	
13	Sheather	09/13/06	06/04/07	06/12/07	06/16/07	01/10/08	272	484	
33	Dray	10/05/06	07/23/07	07/23/07	08/06/07	01/10/08	291	462	
15	Musonda	08/25/06	06/16/07	06/15/07	06/29/07	01/10/08	294	503	
5	Tomita	07/31/06	12/27/06	05/25/07	06/15/07	01/10/08	298	528	
11	Meintanis	07/19/06	06/04/07	06/11/07	06/14/07	01/10/08	327	540	
26	Castillo	08/14/06	07/06/07	07/10/07	07/19/07	01/10/08	330	514	
14	Zhang	07/04/06	05/09/07	06/13/07	06/29/07	01/10/08	344	555	
16	Roca-Pardii	04/20/06	06/21/07	06/21/07	07/06/07	01/10/08	427	630	
32	deLeon	05/21/06	07/21/06	07/23/07	08/07/07	01/10/08	428	599	
4	Alonso	02/28/06	05/18/07	05/25/07	06/04/07	01/10/08	451	681	
9	Corduas	02/16/06	05/31/07	06/01/07	06/03/07	01/10/08	470	693	
28	McLeod	08/01/05	07/05/07	07/13/07	08/07/07	01/10/08	711	892	
18	Rocci	09/11/04	06/25/07	06/26/07	07/10/07	01/10/08	1018	1216	
	Grey not plotted at right						Means:	254	447
							Medians:	204	387
19	Shang	Unk	Unk	Unk	07/01/07	01/10/08			



This seems fairly clear. The median turnaround time is 204 days, but a few papers seemingly bypass peer review.

W.5.6.2 Comments

p. 2177 “Abstract ...

Based on the clustering within the co-author social network, we distinguish several styles of co-authorship including solo models (no co-authors), mentor models, entrepreneurial models, and team models. We conjecture that certain styles of co-authorship lead to the possibility of group-think, reduced creativity, and the possibility of less rigorous reviewing processes.”

At least in one case, the mentor model bypassed any credible review, publishing a poor paper in the wrong journal.

pp. 2177-2178 “1. Introduction”

This entire section uses text from unattributed authors who are not coauthors of this article.

pp. 2178-2179 “2. Clustering and allegiance”

[RIG2005] is referenced, but most text can be found in [RIG2004]:

2. Blockmodel (p.1)

4. Allegiance (p.3)

5. Determination of the number of groups via allegiance (pp.

pp. 2180-2183 “3. Co-authorship networks”

“Wegman et al (2006) undertook a social network analysis of a segment of the paleoclimate research community. This analysis met with considerable criticism in some circles, but it did clearly point out a style of co-authorship that led to intriguing speculation about implications of peer review. Based on this analysis and the concomitant criticism, we undertook to examine a number of author-coauthor networks in order to see if there are other styles of authorship. Based on our analysis we identify four basic styles of co-authorship, which we label, respectively, solo, entrepreneurial, mentor, and laboratory. The individuals we have chosen to represent the styles of co-authorship all have outstanding reputations as publishing scholars. Because of potential for awkwardness in social relationships, we do not identify any of the individuals or their co-authors.”

Lacking obvious relevant expertise, they did a study on coauthorship networks, a topic with a long history of which they seemed unaware. The WR lacks even a single relevant citation. If they ever consulted sociologists who do such work, it is not obvious. Social network analysis seems outside their charter, *and the whole idea likely came from MM05x*. The last sentence above is strange, as all authors are easily identifiable.

“Fig. 1 is the example of a block model type we call entrepreneurial style. We have removed co-author names from this network.”

That is identical to WR, Figure 5.2, p.40, which certainly shows the names. The principal author is obviously Mann and any reader could easily find that, given the reference to the WR. Although not so labeled in the WR, that work was done by Rigsby, as Wegman says later in [WEG2006c, p.3], on which the same chart appears.

p. 2181

“Fig. 2 is a example of a block model type we call the mentor style. Here the principal author has 101 co-authors. ... Thus the reason we call such a block model co-author social network the mentor style is fairly clear.”

This is obviously Wegman [WEG2006c, p.2, SHA2006, pp.1-28]. The analysis is Sharabati’s work, although the form of the Figure is not identical. Neither of these sources is mentioned. *Perhaps explicit*

identification of Wegman would have been awkward, given the argument that this working style was superior to the “entrepreneurial” style of Mann, who is also easily identifiable from the reference.

pp. 2181-2182

“Fig. 3 represents a block model type we call the laboratory style...”

This hybrid model represents something of a combination of the laboratory and mentor networks.”

This seems new to this paper, and presumably is the work of Rigsby or Sharabati, who have been doing these analyses.

p. 2183 “4. Implications for peer review”

“Wegman et al. (2006) suggested that the entrepreneurial style could potentially lead to peer review abuse. Many took umbrage at this suggestion. Nonetheless, there is some merit to this idea.”

This gets a publication in a” peer-reviewed” journal that casts doubt on peer review in paleoclimate. Although the discussion seems plausible, **no evidence of peer review abuse is actually presented.**

“Indeed, the paleoclimate discussion in Wegman et al. (2006), while showing no hard evidence, does suggest that the papers were refereed with a positive, less-than-critical bias.”

As shown in this Report, the WP’s competence in paleoclimate issues was minimal. This whole strange excursion into social network analysis seems merely a means to support MM’s earlier ideas of Meme-b, almost impossible to get published in peer-reviewed journals credible on this topic. Credible science journals tend to frown on totally-unsupported claims.

“Finally, the mentor style of co-authorship, while not entirely free of the possibility of bias, does suggest that younger co-authors are generally not editors or associate editors. And often they are not in a position to become referees, so that the possibility of bias is much reduced. Nonetheless, even here, a widely respected principal author has the possibility of smoothing the path for his or her junior collaborators, while the papers of a high reputation principal author may not be as critically reviewed as might be desirable.”

This paper is “self-refuting.” Among other things, Said managed to become an Associate Editor of C&DA less than 2 years after PhD. A poor paper was accepted in 6 days, despite plagiarized text from famous textbooks, weak references and conclusions unsupported by evidence.

Anyone familiar with academe knows that authors are often strongly influenced by their PhD supervisors, who help them find positions, help them get papers published, call on older students to help the younger ones, help past students' new PhDs get post-docs, etc. None of this is wrong or limited to academe, although the nature of academic publishing tends to record influences more visibly. However, without clear evidence, it seems strange to claim that mentor-style networks are less prone to abuse than others. As is well-recorded, the various climate authors argue with each constantly, behavior possibly not expected from junior academics with regard to a powerful senior patron, **A.6**.

CSDA has clear guides:

www.elsevier.com/wps/find/journaldescription.cws_home/505539/authorinstructions

www.elsevier.com/wps/find/editorshome.editors/2_Plagiarism_complaints

www.elsevier.com/wps/find/editorshome.editors/PERK_2_identify

“Literal copying is reproducing a work word for word, in whole or in part, without permission and acknowledgment of the original source. Literal copying is obvious plagiarism and is easy to detect by comparing the papers in question.” *That work is already done.*

www.elsevier.com/framework_editors/PERK/PDFs/Tree_2_-_Susp_plag_in_pub_article.pdf

Hopefully, CSDA and the International Association of the International Association of Statistical Computing (IASC) will deal appropriately with this. *At the least, one might expect the article to be retracted or removed.*

p. 2184 Acknowledgments

“The work of Dr. Yasmin Said was supported in part by the National Institutes on Alcohol Abuse and Alcoholism under grant 1 F32 AA015876-01A1. The work of Dr. Edward Wegman was supported in part by the Army Research Office under contract W911NF-04-1-0447. The work of Dr. Said and Dr. Wegman was also supported in part by the Army Research Laboratory under contract W911NF-07-1-0059.”

This set of acknowledgements might possibly lead to trouble, A.7.

W.5.6.3 Comments on [SAI2008] from an expert

I thought it helpful to find an SNA expert, and luckily got connected with Garry Robins, Associate Professor and Reader at the University of Melbourne. He has published much SNA research. He has coauthored with S. Wasserman. He has coauthored with T. Snijders (cited by Said in [SAI2005], and Co-editor of *Social Networks*). He thus collaborates with senior people in the field and publishes in appropriate journals. [SHA2008] even cites him.

www.psych.unimelb.edu.au/people/staff/RobinsG.html

He kindly sent me some comments on [SAI2008]:

“The implications for peer review in section 4 of the article present inferences that go too far beyond the data and the analysis. The argument is that because there are many tightly coupled groups working closely together they will have a common perspective, so unbiased reviewing is compromised. Of course, in regard to a given article, a set of co-authors (i.e. within the one group) will have a common perspective at least in regard to that article. That does not mean that the other groups necessarily share that same perspective (even if they share one co-author). The literature on network entrepreneurship, including work by Granovetter and Burt, gives plenty of theoretical and empirical reasons to suggest that such groups, linked by one entrepreneur (in this case the central author), may indeed have different opinions or approaches. In other words, a common perspective within groups does not imply a common perspective across groups. Because it is not possible a priori to infer perspectives from network structure, the issue is an empirical question that requires additional data before conclusions can be drawn.

Moreover, the analysis is essentially an egonet strategy, focusing on the co-authors of one central author. Even if there were a common perspective within the egonet, this does not imply a paucity of other reviewers outside the egonet (i.e. who are not co-authors) to provide different perspectives. It is highly risky to draw conclusions about a complete network based on a single egonet. In summary, there may or may not be compromised reviewing in various research domains but this network analysis cannot provide sufficient leverage to show it.

A more complete network analysis involving editors' and reviewers' links, in the context of a domain-wide co-authorship network, together with data on individual positions taken on controversial research issues, would be one way to proceed, although admittedly some of this data would be difficult to obtain.”

W.5.7 Sharabati dissertation

[SHA2008] Walid Sharabati, Multi-Mode and Evolutionary Networks, 10/31/08. PhD Dissertation, GMU.

digilib.gmu.edu:8080/handle/1920/3384?mode=full See [DEE2010p].

This 230-page dissertation shows much work. Some comes from [SHA2006], which is fine, but not the WR pages. It has more basic scholarship problems. *His Committee might have guided him more. One might worry that in covering so much, in-depth literature review got lost.*

pp.1-3

The text here is described in **W.2.3**, derived from earlier antecedents.

p.9

“I conjecture based on two papers published recently [55, 56] that certain styles of co-authorship lead to the possibility of group-think, reduced creativity, and the possibility of less rigorous reviewing processes.”

The two references are:

[55] Y. Said, E. Wegman, W. Sharabati, and J. Rigsby, Implications of co-author networks on peer review, (2007).

[56] _____, Social networks of author-coauthor relationships, Computational Statistics and Data Analysis 52 (2007), 2177{2184, DOI: 10.1016/j.csda.2007.07.021.”

Reference [55] is:

“Implications of co-author networks on peer review,” with Yasmin H. Said, Walid K. Sharabati, John T. Rigsby in Classification and Data Analysis, Macerata, Italy: EUMEdizioni Università di Macerata, 245-248, 2007”

I have been unable to find an online copy of that.

Reference [56] is called [SAI2008] here, but using online date of 2007.

This again repeats Meme-b, with no supporting evidence.

Did no one on Committee question this?

p.9-10

“Of all the work that has been done on social networks, very few scientists had considered coauthorship networks. The main goal of analyzing coauthorship networks is to be able to answer the question of “who-wrote-with-whom” and with what frequency.

This appears in [SHA2006] in an even stronger form, discussed there in more detail, Theme-M. *Every PhD likes to think their work opens major new areas. Did his Committee really believe this? I am slightly surprised no one noticed the weakness of reference and citation, next:*

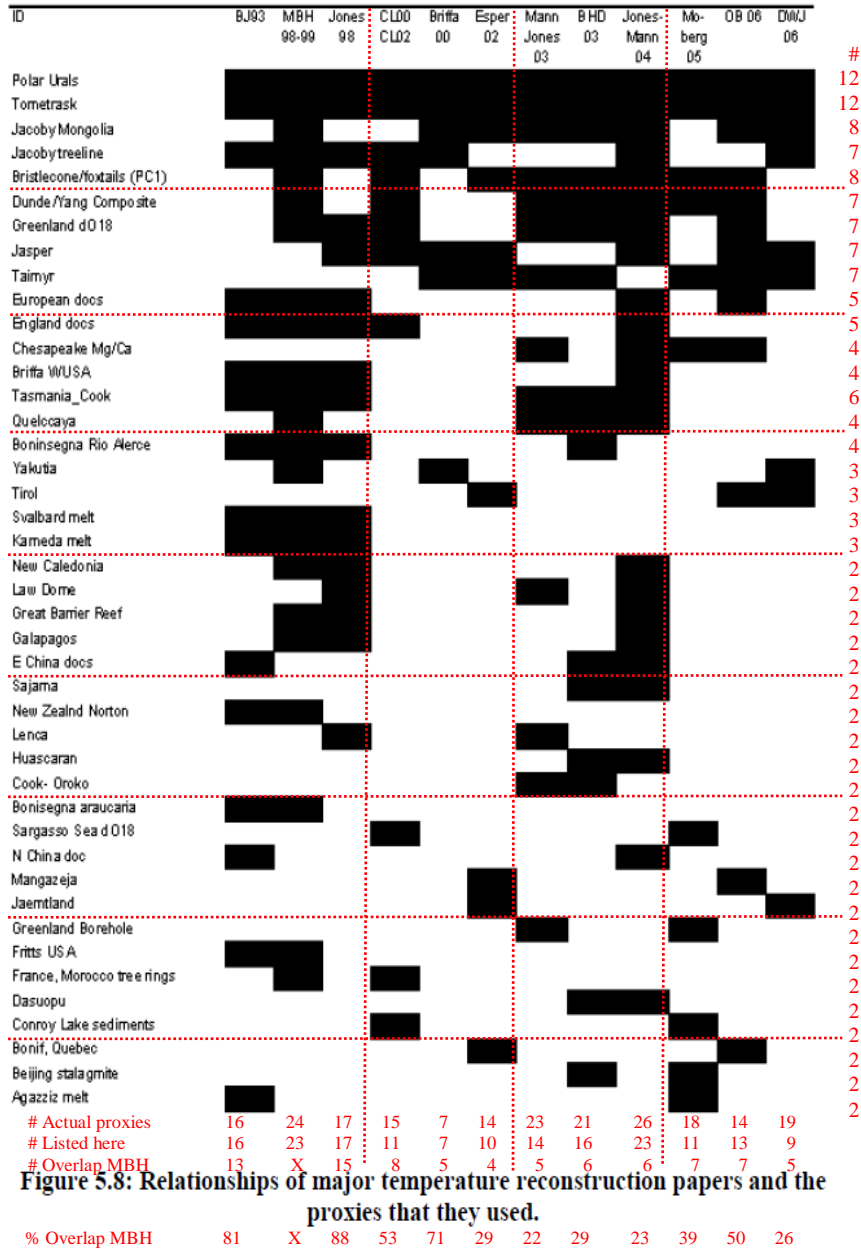
pp.209-213 Bibliography

Of the 67 references, a quick scan found that only 26 seemed to be referenced, with page numbers shown, The Bold are GMU or Wegman-related sources, leaving 20 others.

[2]	129	Barabasi, Albert
[5]	10	Borner
[6]	129	Borner
[8]	15	Carley
[11]	130	Cioffi-Revilla (GMU)
[12]	31,129	CIS
[13]	129	DBLP
[14]	5	De Nooy, Mrvar, Batelgj
[21]	31,146	FARS
[24]	82	Gile and Handcock
[28]	110	Hanneman and Riddle
[30]	26	Seock-Ho Kim
[34]	34	Krackhardt and Carley
[41]	22,23	Marchette and Priebe
[44]	24	Mukha
[48]	129	PubMed
[49]	24	Robertson
[50]	24	Robins, Pattison, Kalish, Lusher
[51]	129	Roth
[53]	11,13,21	Said
[55]	9,18,29	Said, Wegman, Sharabati , Rigsby
[56]	9,18	Said, Wegman, Sharabati , Rigsby
[57]	24	Simpson
[60]	24	van Duijn, Gile, Handcock
[61]	4,6,6,7,135	Wasserman and Faust
[62]	11,12	Wegman and Said
?	27	Mielke (1979)
?	27	Faust and Romney (1985)
?	31,75	Martinez (2002)

The WR cites ~ 50% of its references. [SHA2008] cites ~40%, although a few may easily have been missed. For the range of topics covered, the number of references seems low. The co-authorship literature alone could easily include that many. People often include a few uncited general background texts, but this seems a high percentage.

W.5.8 Figure 5.8 of WR



“Discussion: The social network analysis of authors’ relations suggests that the “independent reconstructions” are not as independent as one might guess. Indeed, the matrix outlined in Figure 5.8 illustrates the proxies that are used more than one time in twelve major temperature reconstruction papers. The black boxes indicate that the proxy was used in a given paper. It is clear that many of the proxies are re-used in most of the papers. It is not surprising that the papers would obtain similar results and so (p.47) cannot really claim to be independent verifications. As a graphical comparison of a number of the reconstructions, see Figure 5.9 below taken from D’Arrigo et al. (2006).”

Obvious local issues with Figure 5.8

This is a direct evolution of MM05x, p.17, Meme-c 1.

Many” and “most” rather imprecise

“It is clear that many of the proxies are re-used in most of the papers” seems misleading. It is certainly not clear. At best, it seems strange, imprecise language for statisticians. Of 12 papers, “most” means at least 7, but only 9 of 43 proxies are used 7 or more times. Is 9/43 “many”? Most of the 43 proxies (22) are used in 2-3 studies. Even stranger is:

Unique proxies must have been known, but ignored, 51% of the data “the proxies that are used more than one time” says nothing about proxies used only once. The chart’s originator reads all the papers, accumulates a master list of proxies, checks off usage in each paper, then shows the 43 used more than once. The remainder of the list is used only once, but that list is a necessary byproduct of the procedure. The chart is annotated at bottom, showing the #Actual proxies in papers, the #Listed here and #Overlap with MBH. Numbers are used as found, in papers, assuming the chart’s creator is competent in matching proxies, so that each Actual but not Listed is truly unique. Totaling (#A - #L) gives 44. **The WR omits 44 of 87 cases, 51% of the total, so it is clear that “most” of the proxies are used only once.** Given this assumption, the next page shows the way Figure 5.8 should look. It is mostly white space. *Is it usual practice to ignore the half of the data that weighs against the conclusion? The discussion is carefully worded, but the graphic is powerful, especially with black bars, compared with more typical X’s and seems misleading.* The last red annotations show the percentage of each paper’s proxies that overlap with MBH, i.e. (#A/#O). Unsurprisingly, newer papers show lower overlap percentages as more series have been obtained.



(9)
7-12

(16)
3-6

(18)
2

(44)
1

Proxies must be valid, and more independent ones are better, but...
 These proxies represent huge field data collection efforts, costing more than simple lab experiments or computer simulations. Should people ignore good data? A serious WR analysis could have assessed the validity and local correlations of proxies and the degree and relevance of overlaps, but that would have required actual field knowledge. Of course, paleoclimate researchers do such statistical studies often, as is easily seen by studying (not just lightly paraphrasing) papers cited by the WR.

Normal statistics in science

In general, as science builds, people expect to see that:

- Good data is selected, bad data eliminated, with reasons explained, not just ignored. Data should not be omitted from graphics because it contradicts the preferred hypothesis. Graphics are stronger than words.
- The same data is analyzed by different methods and ideally by different people, not just the same code run again. Statisticians often develop alternate methods that legitimately give different results. For example, see en.wikipedia.org/wiki/Normality_test .
- Different subsets of the data are studied to do sensitivity analysis. Statisticians do this all the time also, using resampling techniques: en.wikipedia.org/wiki/Resampling_%28statistics%29

These are standard techniques in statistics and science, including paleoclimatology, which has to use noisy, expensive data from the real world. It cannot just rerun lab or computer experiments.

“cannot claim to be independent verification.”

Demanding complete non-overlapped proxies (100% “independence” in the purest statistical sense) seems a straw man argument. People would ridicule an explicit claim that legitimacy required 100% non-overlap. would have been ridiculed. If that is what they meant, no one need to do all the work to check data and draw this graph. Are they trying to declare the entire field of paleoclimate reconstruction to be invalid?

Narrow focus on MBH-vs-MM??

The WR was supposed to have a narrow focus on MBH-vs-MM. How did that turn into analysis of 12+ different reconstructions and 87 proxies? Meme-h. That is a great deal of work, unless someone else helped, but it does fit real mission #2.

Who actually did this work?

Two obvious possibilities

Odd characteristics of Figure 5.8 and to some extent Figure 5.9 raise an issue of the real creator (s) of this, especially in light of the WR's cursory rephrasings, frequent errors shown elsewhere. Two possibilities are:

- The WP studied all the papers. Hopefully, given the high profile of the WP and their criticism of others' data archiving, they have archived their own work. Maybe they used numerous (unacknowledged) posts from McIntyre. OR
- Someone else, McIntyre, did the research and either supplied the data, links to numerous website URLs or even the actual chart, but was never acknowledged. *If so, that lack raises yet another issue about the objectivity and independence of the WP.*
- Of course, in mid-2005, the referenced, but uncited MM05x, pp.17-18 discussed this exact topic, listing 10 studies of which 6 are carried forward to this list, gray-highlighted on next page. Even if the WP started from that paper, it would take much work.

Nonobvious naming, especially for neophytes

It is relatively easy to find the proxy counts, but actually matching names is more work than it might seem, given terminology differences among papers. As an example, D'Arrigo, et al (2006) is mentioned in that paragraph. DWJ06 p.4 lists the 19 chronologies used, compared to the 9 shown in WR Figure 5.8. Of those 9, 5 (Polar Urals, Tornetrask, Taimyr, Yakutia, Jaemtland) are the same, modulo spelling differences. Others are:

WR	DWJ06
Jacoby Mongolia	Mongolia
Jacoby treeline	Yukon (I think)
Tirol	Alps
Jasper	Icefields

It took some searching to find those, especially since "Tirol" and "Jasper" never appear in DWJ06. I only recognized Icefields from having driven the (Columbia) Icefields Parkway from Lake Louise to Jasper.

Strange font

This is minor, but if one magnifies the PDF to 200% and compares Figure 5.8 with others, it seems to use a different, grainier font. That may be a hint or purely coincidental.

McIntyre on "Independence"

Perusal of McIntyre's Climate Audit website is instructive. During 2004-2006 (and after), many of his posts discussed the issues here, including independence of proxies. Meme-c¹, Meme-d¹ had been promoted by McIntyre well before the WP.

climateaudit.org/2004/12/03/other-multiproxy-studies

"We are sometimes asked about other multiproxy studies which are held to somehow support Mann. A couple of comments. First, **if Mann's calculations are wrong, the fact that other studies get similar results is neither here nor there.** Equally, a critique of MBH98 doesn't refute these other studies, nor have we claimed this. Second, **I'm not convinced that these studies are anywhere near as mutually supporting as claimed.** ...

These studies are less "independent" than they appear. Many proxies recur in nearly all studies (e.g. Tornetrask, Polar Urals, Tasmania). If you look at all the authors, there is much overlap. Mann is in 4 of the studies; in addition to Jones et al 1998 and the two articles with Mann, Jones is a co-author in Briffa et al. 2001 and supplied much of the data to Crowley and Lowery. Bradley and Jones have been frequent co-authors."

climateaudit.org/2005/08/04/independence-of-proxy-selection/

MBH98,99; Jones98, Mann,Jones03, CL00, Briffa00.

climateaudit.org/2005/10/28/jones-et-al-1998-the-proxies/

climateaudit.org/2006/02/10/the-proxies-of-osborn-and-briffa-2006

climateaudit.org/2006/12/18/the-independent-2006-multiproxy-studies

(after WR, but still discussed)

New codes, odd citations, McIntyre familiarity

Following are the 12 headings from Figure 5.8. The 3 underlined codes are obvious, but unlike MBH98, MM03, etc) are not defined or used *anywhere else* in the WR, just here. Some codes appear at Climate Audit, which discusses every paper (except CL02, whatever that is), most quite often. This section lists the 12, with a few sample posts, usually including the oldest in each group.

I did not try to find all of them, but it should be clear to any reader that McIntyre spent much of his time studying these proxies endlessly and knew them quite well, far better than anyone on the WP.

The code at the front is the Cited Code of **W.8.2, W.8.3**.

Of the 12 columns, (not counting CL02), we find:

- R** 5 Reference cited strongly, Summarized or discussed
MBH98,99; Esper02; Mann,Jones03; Moberg06; OB06.
- r** 1 Reference cited weakly, WP may have read paper or not.
DWJ06
- u** 5 Cited only on p.28 copied from Mann ,et al (2005) or here in
p.46 Figure 5.8 - might be in Bibliography without real study.
BJ93; Jones98; CL00; BHD03; Jones,Mann04
- U** 1 Uncited
Briffa00 (or if really Briffa (2001), it would be **u**.)

Figure 5.8 suddenly appears with little connection to the rest of its chapter, shows different codes used only here. **It includes 6 papers of which the WR offers no other evidence of studying.**

DWJ06 is unclear, as it is only mentioned in pp.46-47.

u BJ93: Bradley, Jones (1993)

climateaudit.org/2006/07/02/what-was-first-about-mbh98 (BJ93)

R MBH98,99

Many.

u Jones 98: Jones (1998)

climateaudit.org/2004/12/03/other-multiproxy-studies Many.

u CL00: Crowley, Lowery (2000)

climateaudit.org/2005/02/05/crowley-and-lowery-2000

climateaudit.org/2005/02/26/detection-and-attribution-hegerl-et-al-2003 (CL00)

climateaudit.org/2005/07/01/the-crowley-mcintyre-letters (CL00)

climateaudit.org/2005/11/21/splices-in-crowley-and-lowery (CL00)

climateaudit.org/2005/12/16/hegerl-et-al-2003-a-re-posting (CL00)

(**U CL02:** none found, might be Crowley, et al (2003) (Crowley, Baum...), but not Crowley (2002) about Milankovich cycles.)

U Briffa 00: Briffa (2000) is found at Climate Audit, along with many Briffa (2001), which uses two more proxies than shown in Figure 5.8.

climateaudit.org/2004/10/26/spaghetti-diagrams

climateaudit.org/2006/02/13/briffa-large-scale-decline-in-ring-widths

R Esper 02: Esper et al (2002)

climateaudit.org/2004/12/03/other-multiproxy-studies/

climateaudit.org/2005/09/14/esper-et-al-2002-2/

R Mann Jones 03: Mann, Jones (2003)

climateaudit.org/2004/12/03/other-multiproxy-studies Mann

u BHD03: none found, but Bradley, et al (2003) (Bradley, Hughes, Diaz):

climateaudit.org/2005/03/27/briffas-tornetrask-reconstruction

climateaudit.org/2005/03/28/altitude-at-briffas-polar-urals

climateaudit.org/2005/10/29/is-gavin-schmidt-honest

climateaudit.org/2006/03/08/hughes-at-nas

u Jones, Mann 04: Jones, Mann (2004)

climateaudit.org/2004/10/26/spaghetti-diagrams Many

climateaudit.org/2004/12/03/other-multiproxy-studies

R Moberg 05: Moberg (2005)

climateaudit.org/2005/02/09/moberg-et-al-2005 many more

R OB06 Osborn, Briffa (2006)

climateaudit.org/2006/02/09/review-of-osborn-and-briffa-2006

climateaudit.org/2006/02/10/the-proxies-of-osborn-and-briffa-2006

r DWJ06: D'Arrigo, et al (2006)

climateaudit.org/2006/02/11/darrigo-et-al-on-bristlecone-calibration

climateaudit.org/2006/03/07/darrigo-making-cherry-pie

climateaudit.org/2006/04/12/jacoby-and-darrigo-archive-data

Two obvious possibilities, again

The WP was new to paleoclimate. McIntyre had spent years studying every relevant paper and proxy and frequently contending that neither the researchers nor the proxies were “independent.”

Either the WP did all the work themselves and then discarded half the data or they got much unacknowledged help from McIntyre.

Either case bends ASA Ethical Guidelines, **A.8**.

The Figure 5.8 caption echoes McIntyre’s comments at his website.

All this is Meme-c¹, Link-M¹

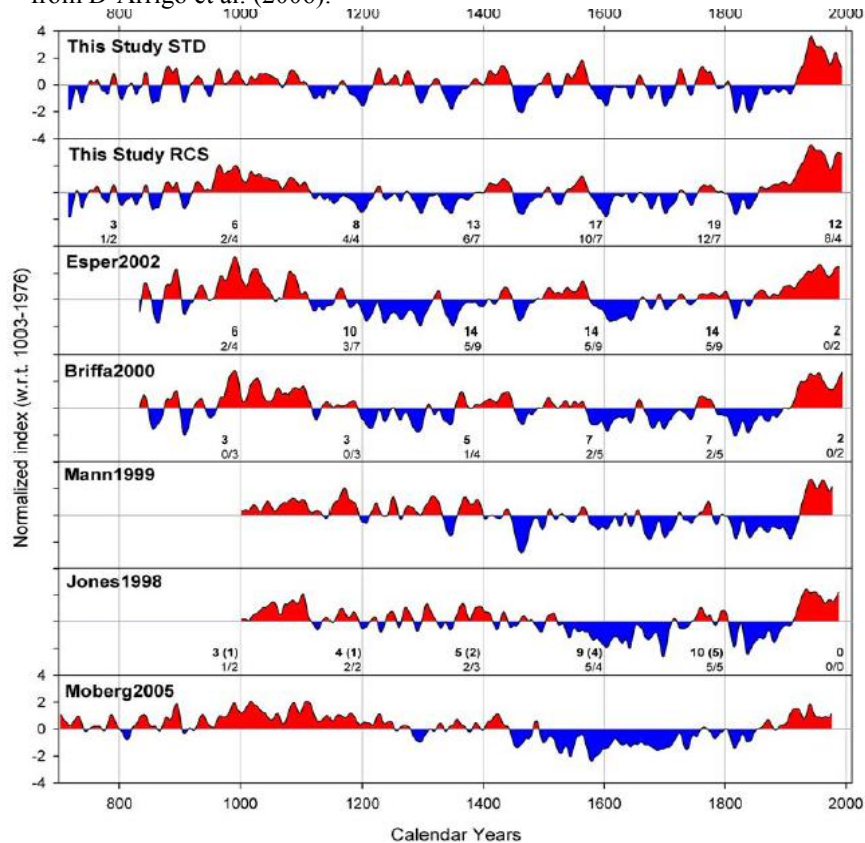
The discussion shortly turns to WR Figure 5.9, which has its own oddities. But first, the caption for Figure 5.8, split between p.46 and p.47 says:

“As a graphical comparison of a number of the reconstructions, see Figure 5.9 below taken from D’Arrigo et al. (2006).”

From its placement, one might think that D’Arrigo et al (2006) was cited in support of Figure 5.8, but that is rather arguable, as seen next.

W.5.9 Figure 5.9 of WR

“cannot really claim to be independent verifications. As a graphical comparison of a number of the reconstructions, see Figure 5.9 below taken from D’Arrigo et al. (2006).”



“Figure 5.9. A comparison of several different reconstructions. From D’Arrigo et al. (2006)

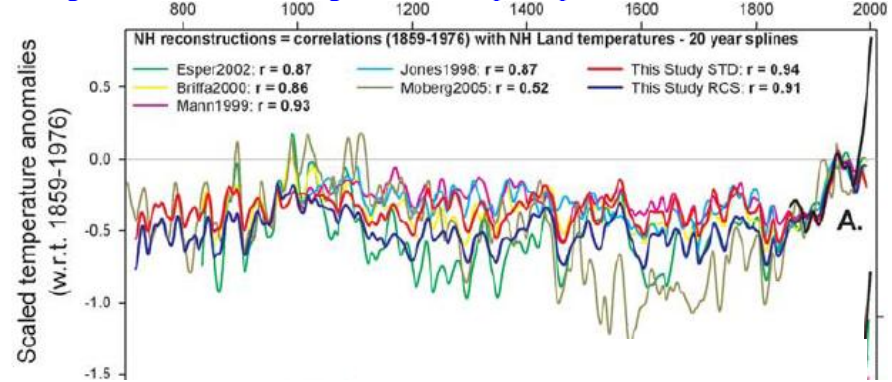
Discussion: There are variations in the temperature reconstruction indicating the fundamental uncertainty in the reconstruction process. Essentially all agree that there was a medieval warm period centered on AD 1000 and a little ice age from at least 1600 to 1850. There is consensus in these reconstructions that the global average temperature has risen over the last 400 years. However, what must be added is that temperatures were below average in AD 1600. Both

Esper et al. (2002) and Moberg et al. (2005) indicate that current global temperatures are not warmer than the medieval warm period.”

This chart may be reasonable in DWJ06, but as presented, it seems almost guaranteed to confuse the casual reader and lead them to incorrect interpretations. If the paper is important enough to devote a full page to it, why is it not Summarized? The caption misrepresents the paper.

1) The WR cites DWJ06 nowhere else but pp. 46 47.

2) This specific chart, especially if poorly explained, can be visually confusing. It is not actually in the paper itself, but in Supplementary Material, mentioned with no explanation as “See also” in a footnote, p.9: <ftp://ftp.agu.org/apend/jd/2005JD006352/2005JD006352-sf01.tif> It is an alternate presentation of the DWJ06 Figure 6. (a), shown here: www.geos.ed.ac.uk/homes/rgroves/wilsonpub4.pdf



3) The scale at left differs from most others.

4) DWJ06 seriously explains reasons why studies vary, often for quite good reasons, like geography.

“There are variations in the temperature reconstruction indicating the fundamental uncertainty in the reconstruction process.”

Uncertainty is normal, but analyzing and bounding it is what real researchers do. The WP simply dismisses this with the general term “uncertainty,” Meme-e. *The casual reader sees a poorly explained chart and “uncertainty.”* Anti-science tactics have long over-emphasized uncertainty, from the tobacco wars onward.

5) This supports Meme-56 both in text and by choice of this chart rather than the chart in the actual paper.

“Essentially all agree that there was a medieval warm period centered on AD 1000”

<ECB> Figure 5.8 shows clear MWP periods, but often not with the same timing or geography Theme-G. DWJ06 offers substantial, credible discussion of this. Compare this chart to the one in the paper. The former shows no modern temperature records or any hint that 2006 temperatures would have been off the chart Theme-H. In DWJ06’s context, that is fine, but using an obscure chart, with a different scale, out of context and poorly captioned, seems likely to mislead most readers.

6) The following has several problems:

“There is consensus in these reconstructions that the global average temperature has risen over the last 400 years. However, what must be added is that temperatures were below average in AD 1600”

The reader should look at the chart closely. There is consensus that:

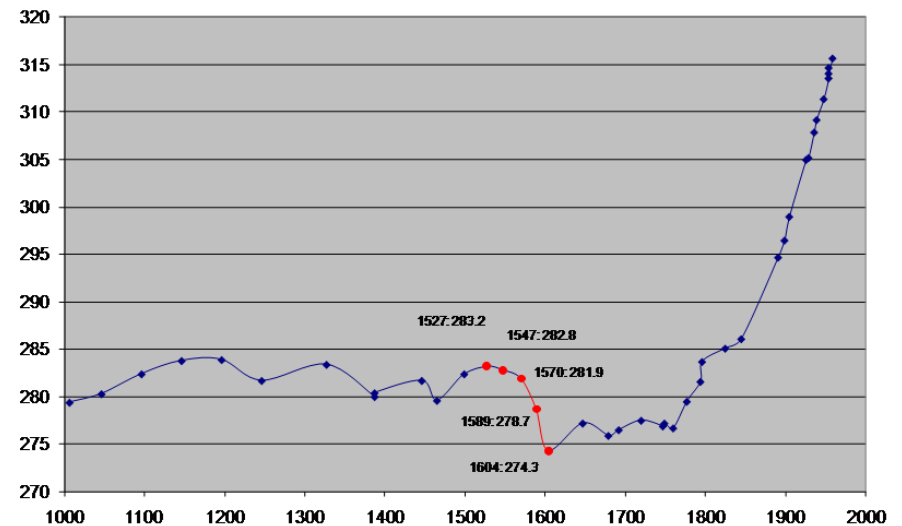
- it was very cool around 1600AD,
- temperature jiggled around 1600AD-1800AD (all reconstructions except Moberg show one or more red peaks),
- temperatures have risen, with jiggles, since the early 1800s.

All that is well in accord with the general shape of the hockey stick 1600-2000, albeit with plausible arguments over depth and geographies of LIA.

 For the world to warm over a long period, forcing changes are needed. There is no magic “return to normal” from a cold period, much as some people wish to think so, Meme-32, Theme-B. The chart at right illustrates CO₂ one of the important forcings. The reader may see some relationship between this chart and the others, although with “physically reasonable lags.” as per the WR Summary of Mann, et al (2000). The chart is from i39.tinypic.com/if0m5g.jpg

The red part shows the unusual sharp dip in CO₂ from the fairly stable 280-285ppm range seen since 1000AD. William Ruddiman has offered a hypothesis that a substantial part of that dip was anthropogenic, large-scale reforestation following massive die-off in the Americas from spread of European diseases. That is currently being argued in journals.

Law Dome CO2 ppm
John R. Mashey, 05/15/10
 cdiac.ornl.gov/ftp/trends/co2/lawdome.combined.dat, DSS raw



7) The WR often confuses NH extratropics with NH, and NH with global. Here, they use NH extratropics (Esper) and then NH (Moberg) as global, and also change “pre-1990” to “current.”

“Both Esper et al. (2002) and Moberg et al. (2005) indicate that current global temperatures are not warmer than the medieval warm period.”

<ECB> Esper, et al (2002) p.2252 says: **(Bold mine)**:

“This comparison suggests that MBH is not necessarily missing a MWP. Rather, it has a reduced expression of the LIA compared with RCS. ... evidence for a large-scale MWP (sensu lato) has been reconstructed, and it **approaches the magnitude of 20th-century warming in the NH up to 1990.** ...”The MBH reconstruction includes temperature estimates from the tropical and subtropical NH (2), which is not represented in the RCS record. This may explain some of the observed differences. ... the large multicentennial differences between RCS and MBH are real and would seem to require a NH extratropical forcing to explain them, one that attenuates toward the equator. ... thermohaline circulation”

<ECB> Moberg et al (2005) do not support this either. They say:

“Here we reconstruct Northern Hemisphere temperatures... ... high temperatures—similar to those observed in the twentieth century before 1990— occurred around AD 1000 to 1100.”

The WR keeps losing that 1990 qualifier.

D'arrigo, et al follow-up

FURTHER DISCUSSION ON: TREE-RING TEMPERATURE RECONSTRUCTIONS FOR THE PAST MILLENNIUM

Follow-up on the National Research Council Meeting on "Surface Temperature Reconstructions for the Past 1000- 2000 Years," March 2-3, 2006, Washington D. C.

www.ideo.columbia.edu/res/fac/trl/downloads/Publications/D%27AWilsJac.nrc.followup.pdf

This is a useful commentary, by researchers perfectly willing to critique MBH99 methods via normal scientific argument.

« **Virtually all of these studies, despite different methodologies and only partially overlapping data sets, have reached the same conclusion: that recent warming in the Northern Hemisphere appears to have been unprecedented over the past millennium** and that this **warming is most likely a result of the anthropogenic release of greenhouse gases into the atmosphere**. The unusual nature of reconstructed 20th century temperatures is typically robust even if a number of individual series are excluded, and the **reconstructions largely fall within each other's respective uncertainty limits**. ...

Several recent opponents of anthropogenically forced global warming are familiar with statistics but have not personally developed tree-ring or other proxy data or reconstructions themselves. They claim that there are methodological artifacts that could bias, in particular, the Mann et al. (1999) "hockey stick" reconstruction, and by inference, other reconstructions as well. However, the methods utilized by the various other studies are often quite different...

There has also been accusation of bias in site selection or so-called "cherry picking," in which it has been argued that dendrochronologists only include those sites that show global warming for use in the tree-ring reconstructions.

It is clear that the D'Arrigo, et al discussion strongly disagrees with the WR. See the D'Arrigo section in **A. 2** for more detail.

At this point, it is unclear whether:

- The WP studied the paper, but did not understand it. Given the number of other errors and misunderstandings , this is possible.
- The WP studied the paper, understood it, but ignored it. There are enough explicit Biases that this is also possible <MB>.
- The WP never really studied the paper, but was handed a pointer to the (obscure) D'Arrigo figure, likely selected by McIntyre, who was

certainly familiar with it. Link-M. If so, at the very least, an Acknowledgement might have been in order.

W.5.10 Rezazad dissertation

Once again, DC discovered a problem, this time in [REZ2009].

Hadi Rezazad earned a PhD from GMU under Wegman Spring 2009: *Enhancement of Network Robustness and Efficiency through Evolutionary Computing, Statistical Computation and Social Network Analysis (2009)* www.galaxy.gmu.edu/stats/colloquia/AbstractsFall2009/CollSept25.html

He was the 2009 Convocation Speaker:

volgenau.gmu.edu/events/convocation/past_convocations.php

Like Said (2005) and Sharabati (2008), his dissertation won the CS/CDS Outstanding PhD Dissertation Award:

www.galaxy.gmu.edu/stats/awards.html

However, pp.10-18 once more include text strikingly similar to that in **W.2.3**, including "statues." The text is mostly identical to the WR, roughly marked with red boxes in **W.2.3**.

DC recently did the full side-by-side and discussed the topic generally in [DEE2010p], showing additional awkward scholarship beyond the plagiarism.

deepclimate.files.wordpress.com/2010/09/rezazad-wegman-social-network.pdf

An oddity of this work is yet another irrelevant injection of SNA terminology into computer network analysis, which has a long history of its own. Calling computer nodes "actors" is not a contribution.

W.6 Findings

This excerpts some relevant parts of the Findings. [TAM2010] is a good starting place for statistics discussions, avoided here.

WR p.48

“1. In general we found the writing of MBH98 somewhat obscure and incomplete. The fact that MBH98 issued a further clarification in the form of a corrigendum published in *Nature* (Mann et al. 2004) suggests that these authors made errors and incomplete disclosures in the original version of the paper. This also suggests that the refereeing process was not as thorough as it could have been.”

 “obscure” From **Fig. 2.2**, their reviews of MHB98 (MBH99) was rated 97% (100%) **SS**, 70% (66%) **ID**, the two highest **SS** ratings of all Summaries. Their efforts introduced numerous errors, meaning changes, and biases, **W.11.4**.

<EB>The WP never contacted MBH or other paleoclimate scientists. *Did the WP ever wish to understand this work?* Meme-b●

 “Corrigendum” MBH had to clarify a paper. This sounds bad, especially to a general audience unfamiliar with science publishing. *Perhaps this is a rare event?* A search at MBH98’s publisher, *Nature*, for “Corrigendum” for calendar year 2009, yields over 300 entries, ~6/week. www.nature.com

Regarding refereeing, the reader should consider the overall quality of review displayed in the WR itself, the strange “review” of the WR and the 6-day “review” of [SAI2008]. The admitted errors in MBH98 took years to find. No experienced person expects peer review to find such things.

2. In general, we find the criticisms by MM03, MM05a and MM05b to be valid and their arguments to be compelling. We were able to reproduce their results and offer both theoretical explanations (Appendix A) and simulations to verify that their observations were correct. We comment that they were attempting to draw attention to the deficiencies of the MBH98-type methodologies and were not trying to do paleoclimatic temperature reconstructions8.”

<EB> Wegman was contacted by Coffey [COF2009]. The WP used sources provided through Spencer, most likely originating with MM+TT. They consulted with McIntyre, as they originally had problems running the software. The **SS/ID** percentages for MM papers were noticeably lower than for the rest of the papers. They repeated arguments found in uncited MM sources, no matter how incorrect.

They claim that MM were not doing a reconstruction, but this contradicts WR pp. 75-76, discussed in **W.4.4**:

“Having accounted for the major errors, MM03 reconstructed the temperature history.”

 It is *strange* to claim the following papers to be irrelevant. This was discussed at length later [WEG2006c, pp. 10-12], *not very convincingly*.

G121Footnote “8 MM05a was critiqued by Wahl and Ammann (2006) and the Wahl et al. (2006) based on the lack of statistical skill of their paleoclimate temperature reconstruction. Thus these critiques of the MM05a and MM05b work are not to the point”

“4. In response to the letter from Chairman Barton and Chairman Whitfield, Dr. Mann did release several websites with extensive materials, including data and code. The material is not organized or documented in such a way that makes it practical for an outsider to replicate the MBH98/99 results. For example, the directory and file structure Dr. Mann used are embedded in the code. It would take extensive restructuring of the code to make it compatible with a local machine. Moreover, the cryptic nature of some of the MBH98/99 narratives means that outsiders would have to make guesses at the precise nature of the procedures being used.”

Hopefully, the members of the WP can produce every bit of code and data used in writing the WR, and it will be found to be well-documented and easily portable to any desired system, W4.1.

They had trouble with McIntyre’s code, so they consulted him. They had trouble with Mann’s code, so they never contacted him.

WR p.49

“As mentioned in our introduction, much of the discussion on the ‘hockey stick issue has taken place on competing web blogs. Our committee

believes that web blogs are not an appropriate way to conduct science and thus the blogs give credence to the fact that these global warming issues are have migrated from the realm of rational scientific discourse. Unfortunately, the factions involved have become highly and passionately polarized.”

Blogs are certainly no way to conduct science, although sometimes credible blogs can expose absurd papers much faster than peer-reviewed journals. In this case, science was being conducted, as usual in peer-reviewed literature. Anti-science was being conducted almost entirely in books, OpEds, blogs and dubious journals. The anti hockey-stick effort was well under way in 2002 before McIntyre got involved.

“Generally speaking, the paleoclimatology community has not recognized the validity of the MM05 papers and has tended dismiss their results as being developed by biased amateurs. The paleoclimatology community seems to be tightly coupled as indicated by our social network analysis, has rallied around the MBH98/99 position, and has issued an extensive series of alternative assessments most of which appear to support the conclusions of MBH98/99.”

 The WP was sufficiently new to SNA that it had plagiarized most of its text from famous book and then did a poor analysis, Meme-b¹, W.2.3, W.5.2, W.5.6.3.

 Given the quality of Summaries, there is little evidence of paleoclimate understanding in the WP. An objective committee with a modicum of expertise might have recognized a common occurrence in real science, as groundbreaking papers (MBH98/99) stirs vigorous discussion and research, alternate variations and arguments, hardly “rallying around.” Of course, I have actually at least looked at many of those papers. *It remains unclear which of the WP actually ever studied the papers.*

“Our committee believes that the assessments that the decade of the 1990s was the hottest decade in a millennium and that 1998 was the hottest year in a millennium cannot be supported by the MBH98/99 analysis. As mentioned earlier in our background section, tree ring proxies are typically calibrated to remove low frequency variations. The cycle of Medieval Warm Period and Little Ice Age that was widely recognized in 1990 has disappeared from the MBH98/99 analyses, thus making possible the hottest decade/hottest year claim. However, the methodology of MBH98/99 suppresses this low frequency information. The paucity of data in the more remote past makes the hottest-in-a-millennium claims essentially unverifiable.”

<EB> *The WP seemed to really want to use that 1990 figure, claimed to digitize it, distorted it yielding an even-warmer MWP, W.4.2.. Presumably, since they had not actually read [IPC1990], they likely got this from MM.* Later, Wegman claimed it was a cartoon just used as an example. Meme-56¹, Theme-A⁴, Theme-G⁴.

“Although we have not addressed the Bristlecone Pines issue extensively in this report except as one element of the proxy data, there is one point worth mentioning. Graybill and Idso (1993) specifically sought to show that Bristlecone Pines were CO2 fertilized. Bondi et al. (1999) suggest [Bristlecones] “are not a reliable temperature proxy for the last 150 years as it shows an increasing trend in about 1850 that has been attributed to atmospheric CO2 fertilization.” It is not surprising therefore that this important proxy in MBH98/99 yields a temperature curve that is highly correlated with atmospheric CO2. We also note that IPCC 1996 stated that “the possible confounding effects of carbon dioxide fertilization need to be taken into account when calibrating tree ring data against climate variations.” In addition, as use of fossil fuels has risen, so does the release of oxides of nitrogen into the atmosphere, some of which are deposited as nitrates, that are fertilizer for biota. Thus tree ring growth would be correlated with the deposition of nitrates, which, in turn, would be correlated with carbon dioxide release. There are clearly confounding factors for using tree rings as temperature signals.”

<EB> Has the WP demonstrated paleoclimate knowledge sufficient to argue this point? Meme-107¹, Meme-e¹:

This also contradicts mentions elsewhere of Meme-h¹. Wegman was clearly no tree-ring expert, even by 2007, A.4. (Bondi is a typo for Biondi).

<e> “IPCC 1996” This was 10 years old at the time, with no reference, Theme-A⁴.

Did they actually read this, or simply get the claim from someone else?

The discussion of nitrates is very strange. Nitrates are only even mentioned in the WR in this paragraph, p.2, and on p.13:

“In addition, oxides of nitrogen are formed in internal combustion engines that can be deposited as nitrates also contributing to fertilization of plant materials. It is clear that while there are temperature signals in the tree rings, the

temperature signals are confounded with many other factors including fertilization effects due to use of fossil fuels.”

DC showed in [DEE2009a, DEE2009b, p.2] that this text was inserted amidst text plagiarized from Bradley (1999), sometimes with errors and contradictions. Neither Bradley (1999) nor Cronin (1999) mention nitrate fertilization as an issue. Nitrogen fertilization is discussed in [IPC2001, pp.196-197, 215], although without reference to tree-rings. Otherwise, the only Important Paper to discuss this was MM05a, but this discussion did not appear in the one peer-reviewed paper, MM05b. This is a complex topic, because for example, increased NO_x emissions by vehicles tend to increase acid rain, not good for trees. The whole basis for this is likely the (unrefereed) MM05a, or perhaps McIntyre’s Climate Audit, which had mentioned it several times prior to the WR:

climateaudit.org/2005/08/10/owens-lake-water-diversion-for-la-and-bristlecones

“The very unusual 20th century growth rate of the bristlecones was attributed by Graybill and Idso [1993] to CO₂ fertilization. In our E&E article, we surveyed other possible non-climatic factors which had not been eliminated, ranging from nitrate and phosphate fertilization to 19th century sheep grazing-”

climateaudit.org/2005/09/16/369

“We expanded considerably on this issue on our EE article, where, in addition to CO₂ fertilization, we noted other possible non-temperature factors including increased precipitation, phosphate fertilization, nitrate fertilization etc.”

climateaudit.org/2006/03/14/cook-et-al2004-more-cargo-cult

“But the 20th century ramp here is largely created by two high-altitude low-latitude foxtail sites, where CO₂ and other fertilization (phosphate, nitrate) issues have been specifically identified by specialists.”

The WP seems to have decided McIntyre was an authority on nitrate fertilization, but if so, they might have actually referenced him or actual peer-reviewed articles in credible journals.

Although the following appeared several years later, it rather strongly argue against “bristlecone are no good due to 20th century fertilization effects” argument, and the second is in PNAS.

www.realclimate.org/index.php/archives/2009/11/a-treeline-story

www.pnas.org/content/early/2009/11/13/0903029106.abstract?sid=1c81cc57-d8a5-47ac-9652-9664d86f01cf⁷⁰

In addition, [RUS2010] addressed the same or similar arguments, unsurprisingly still being promoted by McIntyre in 2010.

These specifically include allegations and findings:

[RUS2010, pp.55-56]

“The criticism here is often captured by the proposition that today’s temperatures are not unusual compared to the MWP.

That Yamal and other chronologies constructed by CRU are unrepresentative of temperature trends (in recent years), and had an undue influence on all of the lines appearing in Chapter 6 of the 4th IPCC Report.

That a majority of the reconstructions would look significantly different if certain component series were replaced with others, and that if this were done then the conclusions reached in respect of the likelihood associated with ranking of recent warmth with respect to the past would be significantly different.”

“14. Finding: We are unaware of any analysis to demonstrate that any of the above conditions are fulfilled for Yamal or any of the series cited in relation to CRU work (i.e. Tornetrask, Taymir). The Review is naturally aware that partial studies and comments referring to CRU’s published work appear elsewhere. However these criticisms of CRU’s work are not in peer reviewed journals, and we have not found that these are anywhere assembled into a coherent, comprehensive and scrutinised case which demonstrates the proposition in respect of any of the series cited.”

The WP consistently downplays, changes or overrides peer-reviewed work by active researchers in favor of non-peer-reviewed work by McIntyre, but often without actually attributing it to him.

⁷⁰ This is by Salzer, Hughes (of MBH), Bunn, and Kipfmüller.

WR p.50

“We note here that we are statisticians/mathematicians who were asked to comment on the correctness of the methodology found in MBH98/99. In this report we have focused on answering this question and not on whether or not the global climate is changing. We have discussed paleoclimatology only to the extent that it was necessary to make our discussion of the statistical issues clear.”

Meme-h🔴, Theme-N🔵.

In one way this is nonsense, given the **Page tally, §2.7**. In another, it is probably true if their goal was to find MBH98/99 guilty.

“The instrumented temperature record makes it clear that global temperatures have risen since 1850 CE. How this present era compares to previous epochs is not clear because the uncertainties in the proxies. However, it is clear that average global temperature increases are not the real focus. It is the temperature increases at the poles that matter and average global or Northern Hemisphere increases do not address the issue.

For professional statisticians to claim that uncertainty means nothing is known *is a very strange*. In a sentence, they dismiss a huge body of research. Paleoclimate scientists are quite interested in regional effects, and many WR references actually address such issues. The WP seems to try to avoid any acceptance of the research that showed the MWP varied temporally and geographically, even as they cited and even Summarized the papers that showed this again and again, Theme-G🔵.

This may well echo discussion in [ESS2002, p106-112], which eventually yielded a confused article that confused degrees and radians:
rabett.blogspot.com/2007/03/once-more-dear-prof.html
www.realclimate.org/index.php/archives/2007/03/does-a-global-temperature-exist/

We note that according to experts at NASA’s JPL, the average ocean height is increasing by approximately 1 millimeter per year, half of which is due to melting of polar ice and the other half due to thermal expansion.

The latter fact implies that the oceans are absorbing tremendous amounts of heat, which is much more alarming because of the coupling of ocean circulation to the atmosphere. (See Wunsch 2002, 2006).

Is it good scholarship to include in Findings previously undiscussed topics, using vague and/or irrelevant references? Exactly how does this relate to

evaluating pre-instrumental era temperature reconstructions?

JPL’s work includes TOPEX/POSEIDON:
sealevel.jpl.nasa.gov/mission/topex.html

However, it does bring forth the idea that sea level was only rising 1mm/year, rather than the higher numbers found in [IPC2001 11.3.2.3, pp.663-664]. According to [IPC2007, pp.49-50], whose Second Order Draft [IPC2006] was available to the WP:

“The global average rate of sea level rise measured by TOPEX/POSEIDON satellite altimetry during 1993 to 2003 is 3.1 +/- 0.7mm yr-1.”

pds.lib.harvard.edu/pds/view/7768990?n=482

So, where did 1.0mm/year originate? Some possibilities are obvious. [ESS2002, pp.257-264] downplayed sea-level rise, quoting extensively from ex-President of INQUA, N.-A. Mörner. He was described as asked to serve as expert reviewer.⁷¹ Mörner has also written in support of water-dowsing. INQUA later said it did not support his views on climate change:
en.wikipedia.org/wiki/Nils-Axel_M%C3%B6rner :

“Mörner asserts that satellite altimetry data indicate a mean rise in the order of 1.0 mm/yr from 1986 to 1996,^[6] whereas most studies find a value around 3 mm/yr.”

<EB> Mörner is not JPL. MM is not JPL. A vague mention of JPL without citation is not scholarship, especially when it disagrees with results repeatedly published by JPL.

The oceans indeed absorb most of the extra energy, but some eventually returns to the atmosphere.⁷² Wunsch (2002) and especially Wunsch (2006) are really not relevant, **W.11.8, W.8**.

McIntyre had written about Wunsch prior to the WP, so a plausible reason for this is Link-M🔴.

⁷¹ Anyone can be an “expert reviewer” merely by requesting a copy of IPCC draft reports. Many of those labeling themselves IPCC expert reviewers are not experts and have often had most of their suggestions rejected as wrong or irrelevant. Real experts rarely emphasize this activity in their backgrounds. Be careful of anyone who claims “IPCC expert reviewer” as an important identification.

⁷² This is part of the “physically reasonable lags” mentioned in Mann, et al (2000)

W.7 Conclusions and Recommendations

WR p.51

“**Conclusion 1.** The politicization of academic scholarly work leads to confusing public debates. Scholarly papers published in peer reviewed journals are considered the archival record of research. There is usually no requirement to archive supplemental material such as code and data. Consequently, the supplementary material for academic work is often poorly documented and archived and is not sufficiently robust to withstand intense public debate. In the present example there was too much reliance on peer review, which seemed not to be sufficiently independent.”

This is a clear statement of Meme-b, in a report almost entirely authored by Wegman and his former/current graduate students, demonstrably filled with plagiarism, poor scholarship, errors, and biases, that avoided meaningful review, then mis-used commenters as “reviewers.”

In any case, far more important than peer review on one paper is the collection of reconstructions performed by multiple research groups using different combinations of proxies and different methods.

“**Recommendation 1.** Especially when massive amounts of public monies and human lives are at stake, academic work should have a more intense level of scrutiny and review. It is especially the case that authors of policy-related documents like the IPCC report, *Climate Change 2001: The Scientific Basis*, should not be the same people as those that constructed the academic papers.”

 IPCC and NRC have intense reviews. IPCC logs every question and answer. For discussion of the review of the WR, presented to Congress specifically to affect policy, see **A.1.**

“**Conclusion 2.** Sharing of research materials, data, and results is haphazard and often grudgingly done. We were especially struck by Dr. Mann’s insistence that the code he developed was his intellectual property and that he could legally hold it personally without disclosing it to peers. When code and data are not shared and methodology is not fully disclosed, peers do not have the ability to replicate the work and thus independent verification is impossible.”

 Have Wegman, et al made available all the code they used in the WR? See Ritson discussion, **W.4.1.**

“**Conclusion 3.** As statisticians, we were struck by the isolation of communities such as the paleoclimate community that rely heavily on statistical methods, yet do not seem to be interacting with the mainstream statistical community.”

<EB> As discussed in [MAS2010, A.10.4], the science community is not helped by statisticians ignorant of the relevant science. Fortunately, many statisticians take the effort to learn enough to be useful in some specific domain and some have been quite involved with climate science research groups. Wegman even knew several of these, but seems not to have asked them very early. The WP did not speak to paleoclimate people and seemingly did not bother to seek the advice of SNA experts before making strong claims in that area.

“**Recommendation 3.** ... In such cases, evaluation by statisticians should be standard practice. This evaluation phase should be a mandatory part of all grant applications and funded accordingly.

This is offered merely for amusement [NOR2006].

“**Conclusion 4.** While the paleoclimate reconstruction has gathered much publicity because it reinforces a policy agenda, it does not provide insight and understanding of the physical mechanisms of climate change except to the extent that tree ring, ice cores and such give physical evidence such as the prevalence of green-house gases. What is needed is deeper understanding of the physical mechanisms of climate change.”

 Paleoclimate results are due to a policy agenda? That is a clear judgment from a team whose errors demonstrate lack of knowledge of paleoclimate, climate science in general, basic physics and even minimal scholarship practices.

The WP manages to make one mistake after another on the science, as shown by frequent occurrence of Memes and Themes. Deeper understanding is always desirable, but one can obtain an adequate start by reading a few parts of the first 80 pages of [IPC2001] for example. Evidence of any such understanding is difficult to find in the WR. This might be called pervasive Culpable Ignorance, Theme-No. **But of course, it is quite likely that the desired conclusions were known in September 2005, given the Coffey-Wegman contact.**

W.8 Bibliography

W.8.1 Overview and Categorization

The 80 WR references, pp.53-59, are all listed concisely in **W.8.2**.⁷³

W.8 offers notes on ~50. No more than 40 of the 80 are cited in the WR.

The left columns collect miscellaneous notes and metrics for Important Papers, from which **W.11.4** is condensed. For all references:

These were added at end of study to summarize the rest of the codes:

- ① (12) Main MM+TT campaign, major support
- ② (12).MM+TT campaign, minor support, miscellaneous anti-science
- ③ (30) Plausible science, but irrelevant or unreferenced
- ④ (26) Science, but usually attacked, cherry-picked, or ignored

WR **Page** is the page of the WR where the reference is found.

Read (*) marks those I have read (or at least skimmed). **Cited on WR**

Pages lists pages of the WR that actually refer to that source. Five papers (MBH98, MBH99, MM03, MM05a, MM05b) and IPCC TAR are labeled “many.”

The following try to help understand where references might have originated. Anything marked X is at least plausibly relevant.

Link- Link to likely sources, sometimes added elsewhere as hint to possible origin, especially for ideas lacking citations.

M① (21) Likely sourced from MM+TT or indirectly via Spencer. These are MM favorites given unusual emphasis in WR or references unlikely to be used in normal scholarship. Some are very grey, such as (vaguely referenced, but seemingly influential) McK05, MM05x.

m② (31) Referenced by MM, clearly known to them, but might easily have been found through normal research.

X (27) Referenced in [NRC2006], plausible source. Some references may have originated there or from MM+TT of Spencer.

That leaves (27) references whose provenance is even less clear.

Ref'd - Reference cited

R (22) Reference cited strongly, summarized or discussed, someone clearly at least looked at it.

r (7) Reference cited weakly, WP may have read paper or not.

u (11) Reference cited only indirectly through copy or heading. Citations are found only in list WR p.28 from Mann, et al (2005), WR p.46 Figure 5.8, **W.5.8**, or indirectly through Bradley (1999).

U (40) Uncited Reference.

Together, (**u+U**) total 51, or 61%. Even (**U**) alone at 50% is very high.

Credibility of reference, if not serious peer-reviewed source

g (7) “Light grey” (7) talks for knowledgeable groups, like NAS, AGU, ECMWF news articles in *Science*, not generally peer-reviewed.

G (10) “Grey” (10) includes popular press articles, even in otherwise credible publications. It includes talks to nonspecialist groups or in other venues lacking relevant peer review, where anything goes. These may be useful sources for media studies, but not credible as evidence for science.

G: (7) “Dark grey” (7) sources with a long history of promoting climate anti-science, like E&E, talks for GMI or OpEds in some newspapers. WR material seems drawn from some, but not properly cited.

G: (1) “Beyond grey” Fringe science/technology or pseudo-science magazines, with no plausible credibility.

The last 3 categories total 18 (23%), enough to raise concerns.

Relevant, relevance or lack thereof, W.8

N (15) Not relevant, clearly

n (10) Not very relevant or (if I did not read), likely not relevant.

s (3) These papers could plausibly have been Summarized.

S (3) These papers *almost certainly should have* been Summarized as they seem far more relevant than some others so treated. More domain knowledge is required to recognize important exclusions, than to recognize irrelevance in a cited paper, **W.8.5**.

Here, 25 of 80 (31%) seem more or less irrelevant, leaving 55 that either were relevant or could have been for a serious science study, §3, although very few are needed for a minimal statistics study.

Bias: is “b” for possible or minor bias, “B” for something substantial, either derived from the maximum found in **W.11.4** or assigned to other papers. Irrelevant or grey sources are usually labeled this way.

Meme and Theme list some of those found.

⁷³ On-line readers may find it useful to print these 3 pages for reference.

W.8.2 Bibliography Tally

		"Important Papers"							Bibliography											
		Words				% Of Total			Cited											
W.8.2(a) Bibliography Tally		Summary	Total	SS	ID	% SS	% ID	# Issues	WR page	Read	Cited on WR pages	Link#	NRC2006	Ref'd	Source	Relevant	Bias	Memos	Themes	Source
	A. Academic Papers and Books																			
2	1 Anderson (2006)								53	*		M		U	G	N		05		Fed. Reserve Bank Rev.
1	2 Anderson et al (2005)								53	*		M		U	G	N	B	19		Working Paper
3	3 Anderson, et al (2003)								53	*				U		N		62,02		Science
3	4 Biondi, et al (1999)		Mis-cited as Bondi						53	*	49	m	X	r						Geophysical Res. Letters
4	5 Bradley (1999)								53	*	10,11,14			R			B		G	Book
3	6 Bradley, Eddy (1991)								53		10,12			u						Book chapter
3	7 Bradley, Jones (1993)		Called BJ93 on p.46						53	*	46	M		u		n				Holocene
4	8 Bradley, et al (2003)		Called BHD03 on p.46						53	*	46	M	X	u			b		G	Science
3	9 Bradley, et al (2003)								53					U						Book chapter
3	10 Briffa, et al (2004)								53			m	X	U						Global&Planetary Change
3	11 Briffa, et al (2001)		Not Briffa , et al (2000) of pp.46-47						53	*	28	m	X	u			b			J Geophysical Res.
4	12 Bürger, Cubasch (2005)	87	395	337	233	85%	59%	1	53	*	25	M	X	R						Geophysical Res. Letters
3	13 Bürger, et al (2006)								54		66			r						Tellus
2	14 CSPP (2005)								54	*				U	G	N	B			(FF/CSPP: ExxonMobil-funded)
2	15 Cohn, Lins (2005)		Mis-cited, slightly						54	*	17	M		r			b			Geophysical Res. Letters
3	16 Cronin (1999)								54	*				U		n	b		G,H	Book (GMU author)
3	17 Crowley (2000)								54	*		m	X	U			b		C,G,H,K	Science
3	18 Crowley (2002)		Milankovitch essay						54	*				U		N				Science
3	19 Crowley, North (1991)		Rather old, given progress since						54			m		U		n				Book
3	20 Crowley, Lowery (2000)		Called CL00 on p.46						54	*	28,46	M	X	u			b		G	Ambio
3	21 Crowley, et al (2003)		Might be mis-cited as CL02 on p.46						54	*	28	M		u		n				Geophysical Res. Letters
4	22 D'Arrigo, et al (2006)		Called DWJ06 on pp.46-47						54	*	46,47	M	X	r			b		F,G,H,J	J. Geophys. Res.
4	23 Esper, et al (2002)	86	340	316	237	93%	70%	1	54	*	24,28,47	m	X	R					F,G,H,J	Science
3	24 Esper, et al (2005)		Mis-cited,(2002) actually used.						54	*		m		U						Quaternary Science Reviews
3	25 Evans, et al (1976)		Reference mangled; Milankovitch						54	*				U		N				Science
3	26 Evans, et al (2002)								55		28			u						Paleoceanography
4	27 Graybill, Idso (1993)								55		49	m	X	R						Global Biogeochemical Cycles
3	28 Huang (2005)								55	*		M		U		n	b			Nature
3	29 Huang, et al (1997)								55	*		M		U		n	B		A	Geophysical Res. Letters

		"Important Papers"							Bibliography										
		Words			% Of Total				Cited										
		Summary	Total	SS	ID	% SS	% ID	# Issues	WR page	Read	Cited on WR pages	Link#	Ref'd	Source	Relevant	Bias	Memes	Themes	Source
3	30	Huang, et al (2000)							55				X	U		n	B		Nature
4	31	Huybers (2005)	Relevant						55	*		m	X	U		S	B		Geophysical Res. Letters
4	32	Jones, et al (1998)							55		28,46	M	X	u					Holocene
4	33	Jones, Mann (2004)							55		46	M	X	u					Reviews of Geophysics
1	34	Lindzen (2005)	Presentation, nonspecialists						55			M		U	G		B	many	Eric Conf. World Fed Scientists
3	35	Luterbacher, et al (2002)							55		28	m		u					Climate Dynamics
3	36	Mann (1998) (Dissertation)	67-	833	782	351	94%	42%	13	55	*	23	m		R		n	B	E PhD Dissertation
4	37	Mann, et al (1998) MBH98	69-	561	546	389	97%	69%	5	55	*	many	m	X	R			b	F,H,J Nature
4	38	Mann, et al (1999) MBH99	71	502	502	333	100%	66%	8	56	*	many	m	X	R			b	59 F,H,J Geophysical Res. Letters
4	39	Mann, et al (2004)	Corrigendum						56		48	m		R					Nature
4	40	Mann, Emanuel (2006)							56			M		U					EOS
4	41	Mann, et al (2000)	72-	832	680	524	82%	63%	8	56	*	80	m		R			B	01,02 F,H,J Earth Interactions
4	42	Mann, Jones (2003)	77	244	233	182	95%	75%	3	56	*	28,46	m	X	R			B	F,H,J Geophysical Res. Letters
4	43	Mann, et al (2005)	84-	476	406	225	85%	47%	0	56	*	50	m		R				J of Climate
1	44	McIntyre, McKittrick(2003)MM03	75-	447	206	149	46%	33%	5	56	*	many	m	X	R	G		B	59 E&E
1	45	McIntyre, McKittrick(2005a) MM05a	79-	1069	751	399	70%	37%	2	56	*	many	m		R	G		B	A,B E&E
1	46	McIntyre, McKittrick(2005b)MM05b	81	513	321	222	63%	43%	2	56	*	many	m	X	R			B	19 Geophysical Res. Letters
4	47	Moberg, et al (2005)	82-	537	422	272	79%	51%	2	56	*	25,46,47	m	X	R			B	01 H Nature
4	48	Osborn, Briffa (2006) OB06	92	400	347	180	87%	45%	3	56	*	26,46	m	X	R			B	59 G, H Science
4	49	Rutherford, et al (2005)	88-	623	499	243	80%	39%	4	56	*	28	m	X	R			b	F,H,J J of Climate
3	50	Schweingruber, et al (1993)							57		13	m		r					Int J of Biometeoreology
2	51	Tennekes (1991)	Presentation, not-relevant						57			m		U	g		N		ECMWF Workshop
2	52	Valentine (1987)	Fringe science & technology						57					U	G		N		Magnets
4	53	von Storch, Zorita (2005)	Relevant						57	*		m		U		S			Geophysical Res. Letters
4	54	von Storch, et al(2004)	78	395	316	225	80%	57%	5	57	*	25	m	X	R			b	05 Science
4	55	von Storch, et al (2006)	Plausibly relevant.						57	*		m	X	U		s			Science
4	56	Wahl, Ammann (2006)	Very relevant						57	*	48	m	X	r		S		B	Climatic Change
4	57	Wahl, et al (2006)	Plausibly relevant.						57	*	48	m	X	r		s		b	Science
3	58	Wunsch (2002)	74	463	351	223	76%	48%	1	57	*	50			R		n		05 E Book chapter
3	59	Wunsch (2006)	90-	885	647	387	73%	44%	4	57	*	50	M		R		N	b	21,02 E Quarternary (sic) Res
3	60	Zhang, et al (2004)							57		28			u					Holocene

									Bibliography												
		Words				% Of Total			Cited												
		Summary	Total	SS	ID	% SS	% ID	# Issues	WR page	Read	Cited on WR	pages	Link#	Ref'd	Source	Relevant	Bias	Memes	Themes	Source	
B. Other Literature Including Articles in the Popular Press																					
2	61	Colligan (1973)							57					U	G	N	b	08		Science Digest	
1	62	Crok (2005)							58	*			M	U	G		B	19,59		Natuurwetenschap&Techniek	
2	63	Gwynne (1975)							58					U	G	N	b	08		Newsweek	
4	64	IPCC (2001) TAR	Relevant, deserved Summary							58	*	many	m	X	R		s	B			Assessment, very important
3	65	Kerr (2006)							58	*				U	g					Science, News piece	
1	66	Legates (2005)							58	*			M	U	G		B			Financial Post	
2	67	Lindzen (2001)							58	*				U	G	N	B	02,03 08,24		Wall Street Journal	
2	68	Michaels, Douglass (2004)							58	*				U	G		B	20,36		Washington Times	
1	69	Muller (2004)							58	*			M	U	G		b	19		MIT Technology Review	
3	70	NRC (1995)							58		48			R		n				Research-Doctorate Pgms.	
3	71	NOAA (2005)							58	*				U		N				Good assessment, irrelevant	
2	72	Sullivan (1975a)							58					U	G	N	b	08		New York Times	
2	73	Sullivan (1975b)							58					U	G	N	b	08		New York Times	
2	74	Zidek (2006)							58					U	g		b	59		J Royal Statistical Soc	
C. Presentations																					
4	75	Mann (2006)							58					U	g					Nat Academy Science	
1	76	McIntyre (2005) Mcl05							59					U	g					AGU Meeting	
1	77	McIntyre, McKitrick (2006)MM06							59	*				U	g					Nat Academy Science	
1	78	McIntyre, McKitrick (2005)MM05x							59	*			M	U	G		B			GMI	
1	79	McKitrick (2005) McK05							59	*			M	U	G		B			APEC Study Group	
3	80	von Storch, Zorita (2006)							59					U	g					Nat Academy Science	
Tot Totals/Avgs.		20	9515	7662	4774	81%	50%	67	80	53	40	52	27	51	25	25	43	23	18	Counts	
1	12	Pages analyzed	26								50%	65%		64%	31%	31%	54%	29%		Percentages	
2	12	Pages with SS	25																		
3	30	Mann lead Totals/Avgs	2615	2367	1653	91%	63%														
4	26	MM Totals/Avgs	2029	1278	770	63%	38%														

W.8.4 References omitted from Summaries

The general issues are covered in:

www.realclimate.org/index.php/archives/2005/02/dummies-guide-to-the-latest-hockey-stick-controversy

This can be summarized as Theme-K⁴, “Big errors matter, small ones do not.” Here, they really do not.

s: In the Bibliography, von Storch, et al (2006) and Wahl, et al (2006), and the [IPC2001] plausibly might have been Summarized, although at least the third was often discussed.

S: Three peer-reviewed papers argue strongly against MM results, and surely should have been Summarized:

Huybers (2005) and von Storch, Zorita (2005) are peer-reviewed comments on MM05b. They are referenced, but not even cited:

www.realclimate.org/index.php/archives/2005/10/hockey-sticks-round-27

Via different methods both essentially said that some issues emphasized by MM05b made no significant difference.

Wahl, Ammann (2006) or WA06 is a full-length peer-reviewed paper that argues strongly that the various complaints about data errors and PCA decentering make little difference and shows this with clear graphics.

www.cgd.ucar.edu/ccr/ammann/millennium/CODES_MBH.html

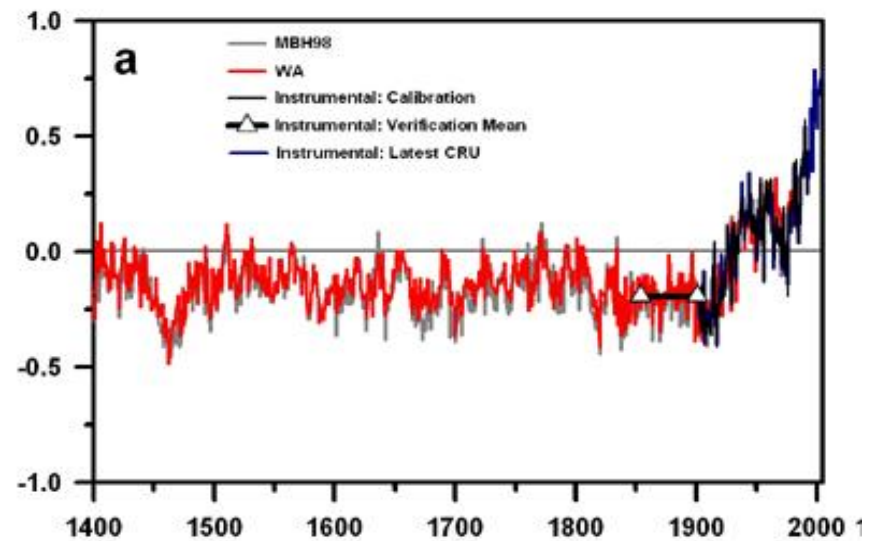
www.cgd.ucar.edu/ccr/ammann/millennium/refs/Wahl_ClimChange2007.pdf

www.realclimate.org/index.php/archives/2006/07/the-missing-piece-at-the-wegman-hearing

Without going into the ensuing back-and forth, Wahl, Ammann (2006) should be an “Important paper” whose charts show various combinations of reconstructions addressing the MM complaints. The following WA06 graph shows the comparison. How much difference does this make? The WR simply dismisses this with a short footnote:

“MM05a was critiqued by Wahl and Ammann (2006) and the Wahl et al. (2006) based on the lack of statistical skill of their paleoclimate temperature reconstruction. Thus these critiques of the MM05a and MM05b work are not to the point.”

The implausibility of MM’s reconstruction is shown in W.4.4.



Statisticians often do sensitivity analyses and use resampling techniques: en.wikipedia.org/wiki/Resampling_%28statistics%29

Having been exposed to thinking of eminent statisticians (including John Tukey of the Quenouille-Tukey “Jackknife” resampling method), it is strange that serious statisticians would make strong statements about the incorrectness of MBH without:

- Running the freely available “R” code and data provided by WA06. The WR had passed along MM05a complaints about MBH98 FORTRAN versus R. So, why not use it?
- Redoing the MBH reconstruction with centered PCA
- Redoing it while deleting subsets of the data

A mentioned elsewhere, the WP seems also to ignore the obvious issue of confidence intervals, Theme-J⁴. Of course, they were not asked to do this, Meme-h¹.

Theme-J⁴ and Theme-K⁴ are very basic ideas. It is strange to see them ignored.

W.8.5 Strangely uncited references

Cronin (1999), p.301, Crowley (2000), and Crowley, Lowery (2000), all mention the evidence against a global synchronous MWP, Theme-G⁴. So do Bradley (1993), Bradley, et al (2003), cited only in the heading of WP **Figure 5.8, W.5.8.**

W.8.6 Strange omissions from Bibliography

[NRC2006, NOR2006] North Report

This report was issued by the NRC panel led by Gerald North before the WR, who referenced some presentations, but not the report itself, which was publicly available no later than 06/22/06. Of course, this was indeed an expert, objective, independent report, of the sort offered by NAS and rejected by Barton and Whitfield. This really rendered the WR superfluous.

[IPC2006] AR4 2nd Order Draft

The final AR4 was not available to the WP, but drafts certainly were, although they were not to be quoted. [IPC2001] was 5 years old. Scholars with no obvious field expertise might want to compare their work versus the latest views of the experts, Theme-N⁴.

Many WR specific complaints against MBH were already addressed in the 2nd draft. The reader can compare that with the final [IPC2007, 6.6].

Perhaps the WP was unaware of the IPCC process or these drafts?

Perhaps no one mentioned these? By agreement, anyone can comment on IPCC drafts and every comment must be recorded and answered.

McIntyre made 71 (46) comments on the 1st (2nd) drafts:

fts.lib.harvard.edu/fts/search?Q=mcintyre&G=7797046&T=pds-results.xsl&F=H&R=%2Fpds%2Fsearch%2F7797046&O=D

sfts.lib.harvard.edu/fts/search?Q=mcintyre&G=7786989&T=pds-results.xsl&F=H&R=%2Fpds%2Fsearch%2F7786989&O=D

I did a quick sample and found some of the same uncertainty-magnifying Themes that appear in the WR, as on ocean oscillations, abrupt climate shifts, etc. Most were rejected as irrelevant or adequately covered.

[DEN2005, WAS1994, WIK2006 and WIK2006a] are never even referenced, even as the WR spends 14 pages on SNA.

W.8.7 Strangely unnoticed earlier attacks

The WP seems not to have noticed earlier efforts to use changing sets of arguments to attack the hockey stick, in dubious, not peer-reviewed literature. *Given that much of their reference material was likely selected by MM+TT via P.Spencer, this is probably not surprising, but they might have referenced [ESS2002], at least. The WR seems to derive many Memes from dubious sources, but rarely cites or even references them.* A few examples are extracted from [MAS2010], using references there. Chris de Freitas [DEF2002] included an attack on the hockey stick in a petroleum journal paper refereed by Willie Soon and Sonja Boehmer-Christiansen. About the same time, Soon and Baliunas submitted oil-funded [SOO2003] to de Freitas for review. These astrophysicists' emphasis on a hot, synchronous MWP may well have been an early root of the widespread Meme-56¹. That paper also includes 7 of the WR's poorly cited or uncited references, hinting that it may be the original source, via MM or Spencer. Meanwhile, Christopher Essex and McKitrick were working on [ESS2002]. All have substantial histories of cooperation on climate anti-science activities.

Essex was in effect (mostly) replaced by McIntyre. MM were brought to Washington, DC, several times, introduced to experienced climate anti-science advocates (like Soon, Baliunas, Singer, Michaels) for "coaching," introduced to key politicians and helped with publicity efforts. [MIC2005] featured a McKitrick article. MM were well plugged into the tight network of climate anti-science advocates, together labeled MM+TT because they are often difficult to disambiguate.

No mention of any of that appears in the WR.

One might think that a competent researcher would at least try:

Google: Ross McKitrick.

Doing so quickly finds [ESS2002] as highly relevant background.

Of course, inclusion of this book would have seriously lessened credibility.

The attack on the hockey stick started very early, using whatever (changing) reasons could be found. They were fortunate to find someone in McIntyre willing to dedicate much of his time since 2002 to this effort. Essex and McKitrick have other jobs.

W.8.8 Comments on notable references

To avoid double-counts, <eEbb> are only shown for non-Summarized.

A. Academic Papers and Books

🟡#1 MUGN

Anderson, Richard G. (2006) “Replicability, real-time data, and the science of economic research: FRED, ALFRED, and VDC,” *Federal Reserve Bank of St. Louis Review*, 81-93

ideas.repec.org/a/fip/fedlrv/y2006ijanj81-93nv.88no.1.html

Link-M🔴 (see #2, next), Meme-05🟡,”

This paper argues for (fine) goals of more sharing and archiving, noting:

“Their recommendations are reproduced here because, although they sound scientific and sensible, most have been ignored in economic science.”

Despite the common use of time-series and other statistical techniques, economics is not climate science, which incorporates well-known physics constraints such as conservation laws. Physical sciences have long histories of creating models with increasingly better approximations to the real world. *Social sciences present more difficulties. Different economics “schools of thought” tend to persist for many years.*⁷⁴ Complaints about computing in one discipline do not automatically imply identical problems elsewhere. People with (relatively) narrow computational experience sometimes over-generalize to other disciplines involved with computing:⁷⁵

⁷⁴ See en.wikipedia.org/wiki/Schools_of_economic_thought. Even simple physics models do a good job of predicting long-term temperature change as a function of greenhouse gas increases. *I am no economist, but am less convinced of economics models that assume century-long GDP growth rates similar to those of the last century, ignoring effects from oncoming changes like Peak Oil. In some cases, the only way to verify that omission is indeed source code examination (of DICE for example, whose source is available.) To an outsider it seems that respected economics models sometimes reach persistently-different conclusions. Models like those of Ayres and Warr make sense to me, but seem like minority views.*

⁷⁵ As a Silicon Graphics Chief Scientist, I worked closely with people building models and managing large datasets in a wide range of science, engineering, operations research and financial-modeling disciplines, including both climate modelers and Wall Street “rocket scientists.” Unsurprisingly, people often do not understand the issues in other disciplines, on which I wrote: www.realclimate.org/index.php/archives/2008/09/simple-question-simple-answer-no/#comment-97878

🔴#2 MUGNB, ”

Anderson, Richard G., Greene, William H., McCullough, Bruce D., and Vinod, H. D. (2005) “The role of data and program code archives in the future of economic research,” Federal Reserve Bank of St. Louis, Working Paper 2005-014B.

The original paper was presented at January 9, 2005 AEA meeting. papers.ssrn.com/sol3/papers.cfm?abstract_id=763704 Version 2005-14B, March 2005.

ideas.repec.org/p/fip/fedlwp/2005-014.html Version 2005-14C, July 2005
research.stlouisfed.org/wp/2005/2005-014.pdf

climateaudit.org/2005/04/22/anderson-et-al-2005-on-replication

Meme-18🔴.

Most of the paper and its references discuss economics and related statistics practices, in which the authors CVs show expertise. Amidst a discussion of economics and software is the following *odd* footnote, p.5:

“The global-warming debate provides an illustration outside economics. In an important article, Mann, Bradley and Hughes (1998) presented evidence of temperature warming during the twentieth century, relative to the previous several centuries. Their article became prominent when one of its charts (a hockey-stick shaped scatter plot, with a “shaft” consisting of historical data and a “blade” consisting of upward-sloping twentieth century data) was featured prominently in the 2001 report of the U.N. Intergovernmental Panel on Climate Change (the Kyoto treaty). As expected, high visibility invites replication and tests of robustness. In a series of papers, McIntyre and McKittrick (2003, 2005a, 2005b) have chronicled their difficulties in obtaining the data and program code; the publishing journal, *Nature*, did not archive the data and code. After some delay, the authors provided the data (see Mann et al., 2004) but have declined, at least as of this writing, to furnish their statistical estimation programs despite their statement that the statistical method is the principal contribution of their article, specifically, to “...take a new statistical approach to reconstructing global patterns of annual temperature back to the beginning of the fifteenth century, based on calibration of multiproxy data networks by the dominant patterns of temperature variability in the instrumental record.” (Mann et al. 1998, p. 779). McIntyre and McKittrick’s examination suggests that Mann et al.’s statistical procedure (a calibrated principal components estimator) lacks power and robustness; specifically, that the procedure induces hockey-stick shapes even when the true data generating process has none.”

*An economics “Working Paper” is not a strong reference. With all due respect to the clear relevance of economics to policy, might it be wise to show more knowledge of paleoclimate before simply passing along MM material? Would any likely reviewers have had such knowledge?*⁷⁶

 This paper contains 7 (and only these 7) paleoclimate references.:

“Mann, Michael E., Raymond S. Bradley, and Malcolm K. Hughes (1998).

“Global-scale temperature patterns and climate forcing over the past six centuries,” *Nature*, vol. 392, April, pp. 779-787.

___ (1999). “Northern Hemisphere Temperatures During the Past Millennium: Inferences, Uncertainties, and Limitations,” *Geophysical Research Letters*, 26, pp. 759-762.

___ (2004). “Corrigendum: Global-scale temperature patterns and climate forcing over the past six centuries,” *Nature*, 430 (105).

McIntyre, Stephen and Ross McKittrick (2003). “Corrections to the Mann et al. (1998) Proxy Data Base and Northern Hemispheric Average Temperature Series,” *Energy and Environment*, 14, pp. 751-771.

___ (2005a). “The MM Critique of the MBH98 Northern Hemisphere Climate Index: Update and Implications,” *Energy and Environment*, 16 (1), forthcoming.

___ (2005b). “Hockey Sticks, Principal Components and Spurious Significance,” *Geophysical Research Letters*, forthcoming.

McKittrick, Ross (2004). “The Mann et al. Northern Hemisphere ‘Hockey Stick’ Climate Index: A Tale of Due Diligence,” mimeo, Department of Economics, University of Guelph, October.” (sic)

The (2005) paper cites none of the post-1999 work by Mann or other climate scientists. MM or McKittrick are cited 4 times, twice in the obscure and poorly regarded E&E. Senior researchers should know that brand-new results sometimes get refuted, but they cite two “forthcoming” MM papers, the first published in E&E 01/05, and the second 02/12/05 in GRL. They reference a McKittrick (2004) “mimeo,” but never cite it.

This scholarship seems almost as strange of that of the WR.

⁷⁶ I have designed supercomputers often used for climate research, read many peer-reviewed articles in climate science, often downloaded freely-available online datasets and software, studied key comparisons of different temperature reconstructions. I interact frequently with climate scientists in person or by email. *Economics may well have the issues described in the paper, but climate science surely has less of them. I will happily withdraw this comment, given evidence of knowledge of the paleoclimate field. I looked through C.V.s, but could find none.*

They simply repeat MM’s views, ignoring substantial paleoclimate progress from 1998 to 2005. How did MM’s material get into this paper? *Given references to forthcoming papers and a “mimeo” one might speculate that author (s) knew economist McKittrick.*

McIntyre writes, 04/22/05 of a source one might not expect him to know: climateaudit.org/2005/04/22/anderson-et-al-2005-on-replication

“Richard G. Anderson, William H. Greene, Bruce D. McCullough and H. D. Vinod have some very interesting comments in a recent Federal Reserve Bank of St Louis Working Paper about the importance of archiving data and code, in which they cite our work approvingly.”

MM05x seems a very strong, if uncited source for the WR.

McIntyre, p.31 says:

“Bruce McCullough has written a working paper for the Federal Reserve Bank of St. Louis on this recently for economics and cites our work as being an interesting example illustrating this process in another area.”

MM06 also referenced it, but how did this uncited reference get in the WR?

This seems an example of “Meme-laundry” whereby Memes are passed through a “laundry” that includes them in a more credible-sounding source unlikely to be peer-reviewed well for this topic.

Using this paper as the laundry, a plausible sequence might be:

McKittrick → author (s), who include references in this paper →

Mentioned at Climate Audit by McIntyre →

Referenced in MM05x →

Referenced in MM06, Referenced in WR, not cited there, but certainly gets wider visibility than MM05x or this paper. *A second wash cycle?*

Others in this category could be #34, #66, #67, #68, #69, #78, #79, and perhaps #74, except that the WR would be the main laundry.

Anderson was at the US Federal Reserve, Greene at NYU, McCullough at Drexel U, and Vinod at Fordham U. *All seem fairly experienced, with many publications. It would be helpful if such folks (especially any paid by US tax money) avoided offering opinions on unfamiliar science. Such scholarship does not engender credibility.*

However, there appears to be a closer connection, *which may or may not have existed when this started:*

McCullough's C.V. is:

www.pages.drexel.edu/~bdm25

McCullough is Associate Editor at:

International Journal of Forecasting (1999 - present)

J. Scott Armstrong (Wharton) was 1985 co-founder, **A.12.5.**

Computational Statistics and Data Analysis (2003 - present)

Wegman has been advisor, author since 1986, **W.5.6.**

Journal of Economic and Social Measurement (2004 - present)

Foundations and Trends in Econometrics (2005-present)

Co-author Greene is Editor-in-Chief.

Computational Statistics (2005-present)

2008

Richard Anderson, William H. Greene, B. D. McCullough and H. D. Vinod

"The Role of Data/Code Archives in the Future of Economic Research"

Journal of Economic Methodology **15** (1), 99-119, 2008

www.pages.drexel.edu/~bdm25/agmv.pdf

This includes all 7 paleoclimate references, including the mimeo.

This looks like the third wash cycle, finally in a peer-reviewed journal.

Green is Editor-in-Chief of *Foundations and Trends in Econometrics*.

2009

B. D. McCullough and Ross R. McKittrick

Check the Numbers: The Case for Due Diligence in Policy Formation

The Fraser Institute, February

www.pages.drexel.edu/~bdm25/DueDiligence.pdf

This includes a cornucopia of climate anti-science references, via thinktank Fraser Institute [MAS2010]. The mimeo reference finally disappears.

3#3 UN

Anderson, Theodore L., Charlson, Robert J., Schwartz, Stephen E., Knutti, Reto, Boucher, Olivier, Rodhe, Henning, and Heintzenberg, Jost (2003) "Climate forcing by aerosols -- a hazy picture," *Science*, **300 (5622), 1103 - 1104.**

www.sciencemag.org/cgi/reprint/300/5622/1103.pdf (paywall)

Meme-64, Meme-02.

This paper argues about aerosol-based uncertainties of modern attribution studies, references none of the WR's Important Papers. It has minimal connection with paleoclimate reconstructions, which are not generally attribution studies. *Perhaps it was included for:*

"Until this is achieved, the possibility that most of warming to date is due to natural variability, as well as the possibility of high climate sensitivity, must be kept open."

Perhaps that would provide support for the Memes above, but it was not cited, and in any case, aerosol understanding progressed strongly. By [IPC2007], this specific concern seemed diminished:

www.ipcc.ch/pdf/assessment-report/ar4/wg1/ar4-wg1-chapter2.pdf

(Search for Anderson, just before 2.4.5.3.)

3#4 mXr

Biondi, F., Perkins, D. L., Cayan, D. R. and Hughes, M. K. (1999)

"July temperature during the second millennium reconstructed from Idaho tree rings," *Geophysical Research Letters*, **26 (10), 1445-1448.**

www.agu.org/pubs/crossref/1999/1999GL900272.shtml (abstract)

wolfweb.unr.edu/homepage/fbiondi/BiondiEtal1999.pdf

WR mis-cites this, p.49 as Bondi, et al (just a typo).

Link-m : MM06, p.4.

climateaudit.org/2006/03/16/mann-at-the-nas-panel

climateaudit.org/2006/08/30/wahl-and-ammann-again-1

4#5 RB

Bradley, R. S. (1999) *Paleoclimatology: Reconstructing Climates of the Quaternary* (sic), 2nd Edition, San Diego: Academic Press.

Theme-G

<ECB> DC showed strong evidence that the WR plagiarized this source, but with changes weakening or even inverting conclusions.

[DEE2009a, DEE2009b, DEE2010, DEE2010j].

<e>Of course, “Quaternary” is misspelled as “Quarternary.” McShane and Wyner (2010) repeat this same error, **A.10**.

③ #6 u

Bradley, R. S. and Eddy, J. A. (1991) “Records of past global changes,” in *Global Changes of the Past* (R. S. Bradley, ed.) Boulder: University Corporation for Atmospheric Research, 5-9.

“Table 2 found in Bradley (1999), which was reproduced from Bradley and Eddy (1991).” - WR, p.10.

This seems a redundant citation, since they got it from Bradley (1999).

③ #7 Mun

Bradley, R. S. and Jones, P. D. (1993) “‘Little Ice Age’ summer temperature variations: Their nature and relevance to recent global warming trends,” *Holocene*, 3, 367-376.

www.geo.umass.edu/faculty/bradley/bradley1993b.pdf

This well-known paper is uncited, except (presumably) in Figure 5.8 on p.46 as BJ93. Relevance is low due to early date.

Link-M①

climateaudit.org/2006/07/02/what-was-first-about-mbh98

④ #8 MXub

Bradley, Raymond S., Hughes, M. K., and Diaz, H. F. (2003) “Climate change: Climate in medieval time,” *Science*, DOI: 10.1126/science.1090372, 404-405.

www.sciencemag.org/cgi/reprint/302/5644/404.pdf (paywall)

 This succinct Perspectives piece clearly explains the history of the idea of the MWP (i.e., Lamb) and the evidence that the MWP was not globally synchronous, even if it was warm around Europe. WR ignored this, Theme-G④.

This is presumably BHD03 in Figure 5.8 on p.46.

Link-m②

climateaudit.org/2005/03/27/briffas-tornetrask-reconstruction

climateaudit.org/2005/03/28/altitude-at-briffas-polar-urals

climateaudit.org/2005/10/29/is-gavin-schmidt-honest

climateaudit.org/2006/03/08/hughes-at-nas

③ #9 U

Bradley, R. S., Briffa, K. R., Cole, J. E., Hughes, M. K., and Osborn, T. J. (2003) “The climate of the last millennium,” in *Paleoclimate, Global Change and the Future* (Eds. K. D. Alverson, R. S. Bradley and Thomas F. Pedersen), 105-141, New York: Springer-Verlag.

③ #10 mXU

Briffa, K. R., Osborn, T. J., Schweingruber, F. H. (2004) “Large-scale temperature inferences from tree rings: A review,” *The Global and Planetary Change*, 40, 11-26.

Link-m②,

climateaudit.org/2006/02/13/briffa-large-scale-decline-in-ring-widths

③ #11 mXub

Briffa, K. R., Osborn, T. J., Schweingruber, F. H., Harris, I. C., Jones, P. D., Shiyatov, S. G., and Vaganov, E. A. (2001) “Low-frequency temperature variations from a northern tree-ring density network,” *Journal of Geophysical Research*, 106, 2929-2941.

www.agu.org/pubs/crossref/2001/2000JD900617.shtml

www.ncdc.noaa.gov/paleo/pubs/briffa2001/briffa2001.html

“The 20th century is clearly shown by all of the palaeoseries composites to be the warmest during this period.” Abstract .

 WR ignored this, Theme-H④.

This is not the Briffa (2000) cited in Figure 5.9, p.47, by D’Arrigo, et al (2006), and likely not (but possible typo) Briffa 00 in Figure 5.8, p.46: Briffa, K. (2000), Annual climate variability in the Holocene: Interpreting the message from ancient trees, *Quat. Sci. Rev.*, 19, 87–105.

post.queensu.ca/~biol527/Briffa%20and%20Keith%202000.pdf

Link-m②, See W.5.8.

climateaudit.org/2004/10/26/spaghetti-diagrams and many more

④ #12 MXR

Bürger, Gerd and Cubasch, Ulrich (2005) “Are multiproxy climate reconstructions robust?” *Geophysical Research Letters*, 32, L23711, doi:10.1029/2005GL024155.

coast.gkss.de/staff/storch/pdf/b%FCrger.cubasch.grl.2005.pdf

Link-M①, *This is a favorite.*

climateaudit.org/2005/12/14/burger-and-cubasch-are-multiproxy-climate-reconstructions-robust

climateaudit.org/2006/01/09/ammann-at-agu-if-you-had-one-question
climateaudit.org/2006/01/25/the-hockey-team-at-daily-kos
climateaudit.org/2006/01/28/rutherford-mann-et-al-2005
climateaudit.org/2006/01/26/burger-and-cubasch
 RC: www.realclimate.org/index.php/archives/2006/04/a-correction-with-repercussions problems with this paper

③ #13 r

Bürger, G., Fast, I. and Cubasch, U. (2006) “Climate reconstruction by regression – 32 variations on a Theme,” *Tellus*, 58A 227-235.

② #14 UGNB

‘**Center for Science and Public Policy (2005)**
“Climate change and the insurance industry: A critical look at the Cere report - Availability and affordability of insurance under climate change, a growing challenge for the U.S.”
The Center for Science and Public Policy, October 28, 2005.’
ff.org/centers/csspp/pdf/ceres.pdf
CSPP, was then an ExxonMobil funded center within FF, run by Robert Ferguson.
 This is a very grey source. I make no comment on the quality of the report itself, as it is irrelevant:

② #15 Mrb

Cohn, Timothy A. and Lins, Harry F. (2005) “Nature’s style: naturally trendy,” *Geophysical Research Letters*, 32, L23402, doi: 10.1029/2005GL024476, 2005.
www159.pair.com/cohns/TimCohn/Publications/GRL2005Naturallytrendy.pdf
 Link-M①,
climateaudit.org/2005/12/18/cohn-and-lins-grl-2005
climateaudit.org/2005/12/19/pelletier-2002-on-temperature-autocorrelation
climateaudit.org/2005/12/22/more-on-realclimate-on-cohn-and-lins/
 These hydrologists offer much statistical analysis, then add a paragraph at the end:

“But could this warming be due to natural dynamics? Given what we know about the complexity, long-term persistence, and non-linearity of the climate system, it seems the answer might be yes. Finally, that reported trends are real yet insignificant indicates a worrisome possibility: natural climatic excursions

may be much larger than we imagine. So large, perhaps, that they render insignificant the changes, human-induced or otherwise, observed during the past century.”

 This paper has problems, but can be used to support Meme-02① .
www.realclimate.org/index.php/archives/2005/12/naturally-trendy
www.realclimate.org/index.php/archives/2008/08/hypothesis-testing-and-long-term-memory

③ #16 Unb

Cronin, T. M. (1999) *Principles of Paleoclimatology*, New York: Columbia University Press
 Theme-G④, Theme-H④.

This is a credible textbook from a credible author, contemporaneous with (if not as detailed on reconstructions as) Bradley (1999). Cronin had been an Adjunct Professor at GMU then, although apparently not in 2005-2006. The extensive Bibliography contains very few 1998 references, and does not mention MBH98. Chapter VI discusses the Holocene period, but offers little support for a globally synchronous MWP, as per comments pp.300-301, Cronin writes (p.302):

“Finally, I wish to comment on perhaps the most ominous issue, that of current trends in climate. The significance of the twentieth century rise in global mean temperatures is in need of a full text of its own. Many studies show that observed twentieth-century warming is anomalous, often equaling or exceeding even the regional warmth reconstructed for periods during the Medieval Warm Period.”

 WR ignored this, Theme-G④.

③ #17 mXUb

Crowley, Thomas J., (2000) “Causes of climate change over the past 1000 years,” *Science*, 289, 270-277.
www.sciencemag.org/cgi/reprint/289/5477/270.pdf

Theme-G④, Theme-C④, Theme-H④ are quite clear in Figure 6 and discussion. Theme-J④ appears in **Figure 1**.
 Crowley writes:

“...peak Northern Hemisphere warmth during the Middle Ages was less than or at most comparable to the mid-20th-century warm period (~1935–1965). This result occurs because Medieval temperature peaks were not synchronous in all records...”

 WR ignored this, Theme-G④.

#18 UN

Crowley, Thomas J. (2002) “Cycles, cycles everywhere,” *Science*, 295, 1473-1474.

www.sciencemag.org/cgi/reprint/295/5559/1473.pdf (paywall)

This Perspectives article discusses arguments about Milankovitch (orbital) cycles, *with no obvious relevance*.

#20 MXub

Crowley, T. J. and Lowery, T. S. (2000) “How warm was the Medieval Warm Period? A comment on ‘Man-made versus natural climate change,’” *Ambio*, 29, 51-54.

[ambio.allenpress.com/perlserv/?request=get-document&doi=10.1639%2F0044-](http://ambio.allenpress.com/perlserv/?request=get-document&doi=10.1639%2F0044-7447%282000%29029[0051%3AHWWTMW]2.0.CO%3B2&ct=1)

[7447%282000%29029\[0051%3AHWWTMW\]2.0.CO%3B2&ct=1](http://ambio.allenpress.com/perlserv/?request=get-document&doi=10.1639%2F0044-7447%282000%29029[0051%3AHWWTMW]2.0.CO%3B2&ct=1)

“Despite clear evidence for Medieval warmth greater than present in some individual records, the new hemispheric composite supports the principal conclusion of earlier hemispheric reconstructions and, furthermore, indicates that maximum Medieval warmth was restricted to two-three 20–30 year intervals, with composite values during these times being only comparable to the mid-20 th century warm time interval. Failure to substantiate hemispheric warmth greater than the present consistently occurs in composites because there are significant offsets in timing of warmth in different regions; ignoring these offsets can lead to serious errors concerning inferences about the magnitude of Medieval warmth and its relevance to interpretation of late 20 th century warming.”

 WR ignored this, Theme-G.

#21Mun

Crowley, T. J., Baum, S. K., Kim, K.-Y., Hegerl, G. C., and Hyde, W. T. (2003) “Modeling ocean heat content changes during the last millennium,” *Geophysical Research Letters*, 30, 1932.

www.agu.org/pubs/crossref/2003/2003GL017801.shtml

www.geos.ed.ac.uk/homes/ghegerl/Crowley.2003GL017801.pdf

“Observational studies show a significant increase in ocean heat content over the last half century. Herein we estimate heat content changes during the last millennium with a climate model whose forcing terms have been best-fit to surface proxy data. The model simulates the observed late 20th century ocean heat content increase and a comparable Little Ice Age minimum. When glacial advances are factored in, these results imply a sea level fall after the Middle Ages that is consistent with some geologic data. The present ocean heat content

increase can be traced back to the mid-19th century, with a near-linear rate of change during the 20th century.” - Abstract
 “Greenhouse gas forcing dominates the 20th century rise in ocean heat content.” - p.4. Theme-H

This paper is mostly about the important topic of modeling ocean heat content, but only a small part is about proxy reconstruction.

#22 MXrb

D’Arrigo, R., Wilson, R., and Jacoby, G. (2006) “On the long-term context for late twentieth century warming,” *J. Geophys. Res.* 111, D03103, doi:10.1029/2005JD006352. (paywall)

www.agu.org/pubs/crossref/2006/2005JD006352.shtml

www.geos.ed.ac.uk/homes/rgroves/wilsonpub4.pdf

WR Figure 5.9 is from Supplementary Material, which uses a different scale than most other graphs here.

[ftp://ftp.agu.org/apend/jd/2005JD006352/2005JD006352-sf01.tif](http://ftp.agu.org/apend/jd/2005JD006352/2005JD006352-sf01.tif)

The abstract says (**Bold mine**):

“Previous tree-ring–based Northern Hemisphere temperature reconstructions portray a varying amplitude range between the “Medieval Warm Period” (MWP), “Little Ice Age” (LIA) and present. ... Results indicate clear MWP (warm), LIA (cool), and recent (warm) episodes. **Direct interpretation of the RCS reconstruction suggests that MWP temperatures were nearly 0.7°C cooler than in the late twentieth century, with an amplitude difference of 1.14°C from the coldest (1600–1609) to warmest (1937–1946) decades.**

However, we advise caution with this analysis. Although we conclude, as found elsewhere, that recent warming has been substantial relative to natural fluctuations of the past millennium, we also note that owing to the **spatially heterogeneous nature of the MWP, and its different timing within different regions, present palaeoclimatic methodologies will likely “flatten out” estimates for this period relative to twentieth century warming, which expresses a more homogenous global “fingerprint.” Therefore we stress that presently available paleoclimatic reconstructions are inadequate for making specific inferences, at hemispheric scales, about MWP warmth relative to the present anthropogenic period and that such comparisons can only still be made at the local/regional scale.**”

This article is very clear about the geographic and temporal variability of the MWP. It comments usefully on differences among reconstructions,

 WR ignored this, Theme-G.

#23 mXR

Esper, J., Cook, E. R., Schweingruber, F. H. (2002) “Low-frequency signals in long treering chronologies for reconstructing past temperature variability,” *Science*, 295, 2250-2253.

www.wsl.ch/staff/jan.esper/publications/Science_2002.pdf

Theme-F, Theme-G, Theme-H.

#24 mU

Esper, Jan, Wilson, Robert J. S., Frank, David C., Moberg, Anders, Wanner, Heinz, and Luterbacher, Jürg (2005) “Climate: past ranges and future changes,” *Quaternary Science Reviews*, 24, 2164-2166.

www.wsl.ch/staff/jan.esper/publications/QSR_Esper_2005.pdf

This seems a reasonable paper, discussing the general issue of reducing the uncertainties around variability. Theme-F, Theme-G, Theme-H.

#25 UN

Evans, D. L., Freeland, H. J., Hays, J. D., Imbrie, J. and Shackleton, N. J. (1976) “Variations in the Earth’s Orbit: Pacemaker of the Ice Ages?” *Science*, 198, 528-530.

<e> No such paper exists, the WR mangled two references together:

Hays, J. D., Imbrie, J. and Shackleton, N. J. (1976) “Variations in the Earth’s Orbit: Pacemaker of the ice ages?” *Science*, 194, 1121-1132.

www.sciencemag.org/cgi/reprint/194/4270/1121.pdf (paywall)

www.es.ucsc.edu/~rcoe/eart206/Hays_OrbitPacemaker_Science76.pdf

This classic, vastly cited paper is about Milankovitch (orbital) cycles, followed by a comment about a year later:

Evans, D. L., Freeland, (1977) “Variations in the Earth’s Orbit: Pacemaker of the Ice Ages?” *Science*, 198, 528-530.

Neither is very relevant.

#27 mXR

Graybill, D.A., and Idso, S.B. (1993) “Detecting the aerial fertilization effect of atmospheric CO₂ enrichment in tree-ring chronologies,” *Global Biogeochemical Cycles*, 7, 81–95.

Link-m.

climateaudit.org/2005/02/09/bristlecone-adjustment-2

climateaudit.org/2005/02/20/graybill-idso-1993

climateaudit.org/2005/08/28/bristlecone-dc13-the-nail-in-the-coffin/

climateaudit.org/2005/10/03/graybill-and-funkhouser-1993-on-bristlecones

#28 MUnb

Huang, Shaopeng (2005) “Climate blog could score with newer hockey stick,” *Nature* 800 (24 February 2005); doi: 10.1038/433800b.

www-personal.umich.edu/~shaopeng/nature433800b.pdf

climateaudit.org/2005/02/25/realclimate-discovers-checking-is-a-good-thing Link-M.

This was a comment on an earlier *Nature* editorial on the creation of RealClimate, where Huang found an error in a borehole citation. He told RC about it, and they fixed it. This seems neither very relevant nor very weighty, but McIntyre wrote about it.

 Might this included to show “RealClimate makes mistakes”?

#29 MUnB

Huang, Shaopeng and Pollack, Henry N. (1997) “Late Quaternary temperature changes seen in world-wide continental heat flow measurements,” *Geophysical Research Letters*, 24 (15), 1947-1950.

www-personal.umich.edu/~shaopeng/97GL01846.pdf

This is not very relevant because boreholes are only mentioned once in passing in the WR, and are generally irrelevant to MBH-vs-MM. This was quickly followed by a paper from the same research group (Pollock, et al (1998), see W.4.3) wherein they pulled back the limits of comparable reconstructions from 20,000 years to 500 years. Those results disagreed with MM, but were ignored.

 It is difficult to think of a reason for citing this, except for its uses in McKittrick (2005) and McIntyre, McKittrick (2005), Link-M, Theme-A.

#30 XUnB

Huang, Shaopeng, Pollack, Henry N., and Shen, Po-Yu (2000) “Temperature trends over the past five centuries reconstructed from borehole temperatures,” *Nature*, 403 (17 February 2000), 403, 756-758. www.nature.com/nature/journal/v403/n6771/abs/403756a0.html

I have only read the Abstract, but this is obviously a continuation of the earlier work. See #29 above and W.4.3. This later, uncited paper in effect disagrees strongly with MM. [NRC2006] discussed boreholes as part of a much broader context than the WR,

 The WR ignored this, Theme-A.

④#31 mXUSB

Huybers, H. (2005) “Comment on ‘Hockey sticks, principal components, and spurious significance’ by McIntyre and McKittrick,” *Geophysical Research Letters*.

web.mit.edu/~phuybers/www/Hockey/Huybers_Comment.pdf

 This peer-reviewed criticism was ignored by the WR.

④#32 MXu

Jones, P. D., Briffa, K. R., Barnett, T. P. and Tett, S. F. B. (1998) “High resolution paleoclimatic records for the last millennium: Integration, interpretation, and comparison with General Circulation Model control run temperatures,” *Holocene*, 8, 455-471.

hol.sagepub.com/cgi/content/short/8/4/455

④#34 MUGB Meme-s: too many to list

Lindzen, Richard (2005) “Understanding common climate claims,” to appear *Proceedings of the 2005 Erice Meeting of the World Federation of Scientists on Global Emergencies*

www.geocraft.com/WVFossils/Reference_Docs/Lindzen_2005_Climate_Claims.pdf

climateaudit.org/2008/08/18/erice-seminar Link-M● (This was later, but McIntyre knew of Erice and Lindzen.)

Lindzen’s contrarian views are well-known, and this is a talk in an obscure venue, not for climate scientists. It certainly does reference MM. It also references Singer’s claims about Revelle, Happer’s claims about politicization of science, and a Soon paper in E&E. It quotes Milloy. This paper offers “Republicans versus sunspots” chart (p.8). It thanks Soon for help. WFS itself has many participants and does not seem a climate anti-science group: www.federationofscientists.org

However, WFS was cofounded by (still-President) nuclear physicist Antonio Zichichi, who has signed various climate anti-science petitions (Manhat2008, CATO2009) and was listed as a Heartland expert (HeartExp#1). He is well-plugged into climate anti-science efforts.

 This a fairly grey source, especially given the speaker. *It seems very unlikely that normal scholarship would find and use this source. It is difficult to believe that a few statisticians new to this issue would find this by themselves.*

③#36 mRnB

Mann, Michael E. (1998) *A Study of Ocean-Atmosphere Interaction and Low-Frequency Variability of the Climate System*, a dissertation presented to the faculty of the graduate school of Yale University.

This dissertation is essentially irrelevant to MBH-vs-MM, certainly far less relevant to Summarize than several papers. The Summary has 13 Issues, including some editing errors that betray serious misunderstanding Strangely, % ID is noticeably lower than the Mann-led papers, although % SS is similar. Thus, more effort seems to have spent rewording the (irrelevant) dissertation than the (relevant) papers, Theme-E④.

④#37 mXRb

Mann, Michael E., Bradley, Raymond E., and Hughes, Malcolm K. (1998) “Global-scale temperature patterns and climate forcing over the past six centuries,” *Nature*, 392, 779- 787.

www.elmhurst.edu/~richs/EC/FYS/Mannetal.OriginalPaper.pdf

Theme-F④, Theme-H④, Theme-J④.

④#38 XRb

Mann, et al (1999) MBH99

Mann, Michael E., Bradley, Raymond S., and Hughes, Malcolm K. (1999) “Northern hemisphere temperatures during the past millennium: Inferences, uncertainties, and limitations,” *Geophysical Research Letters*, 26 (6), 759-762.

www.ltrr.arizona.edu/webhome/aprilc/data/my%20stuff/MBH1999.pdf or www.ncdc.noaa.gov/paleo/pubs/mann_99.html

Theme-F④, Theme-H④, Theme-J④.

④#41 mRB

Mann, Michael E., Gille, Ed, Bradley, Raymond S., Hughes, Malcolm K., Overpeck, Jonathon, Keimig, Frank T. and Gross, Wendy (2000) “Global temperature patterns in past centuries: An interactive presentation,” *Earth Interactions*, 7, Paper No. 4, 1-29.

www.ncdc.noaa.gov/paleo/ei/eint_vol4_0004_1_29_2.pdf

Theme-F④, Theme-H④, Theme-J④.

#42 XRB

Mann, M. E. and Jones, P. D. (2003) “Global surface temperature over the past two millennia,” *Geophysical Research Letters*, **30**, 1820.

www.meteo.psu.edu/~mann/shared/articles/mannjones03.pdf

Theme-F⁴, Theme-H⁶, Theme-J⁴.

#43 mR

Mann, Michael E., Rutherford, Scott, Wahl, Eugene, and Ammann, Caspar (2005) “Testing the fidelity of methods used in proxy-based reconstructions of past climate,” *Journal of Climate*, **18**, 4097-4107.

www.meteo.psu.edu/~mann/shared/articles/MRWA-JClimate05.pdf

Theme-J⁴.

The WR p.28 paraphrases the first paragraph of p.4107, thus including 5 citations cited nowhere else, Briffa, et al (2001), Crowley, et al (2003), Evans, et al (2002), Luterbacher et al (2002), Rutherford, et al (2005), Zhang, et al (2004), plus 2 more, Crowley, Lowery (2000), Jones, et al (1998), cited only on p.28 and in Figure 5.8, p.46.

The WR also mis-paraphrases

“simple so-called composite-plus-scale (CPS) methods “
into

“*simple* climate-plus-scale (CPS) methods”

Given the topic, it was appropriate for the WR to reference the next two, but normally, *E&E is considered at best “grey literature” or “journal of last resort for out-of-mainstream papers.” It is far more dubious on this topic than many other grey sources, hence labeled G.*

#44 mXGB

McIntyre, Stephen and McKittrick, Ross (2003) “Corrections to the Mann et al. (1998) proxy data base and Northern hemispheric average temperature series,” *Energy and Environment*, **14**, 751-771.

www.uoguelph.ca/~rmckitri/research/MM03.pdf

Meme-18¹.

#45 mRGB

McIntyre, Stephen and McKittrick, Ross (2005a) “The MM critique of MBH98 Northern hemisphere climate index: Update and implications,” *Energy and Environment*, **16** (1), 69-100.

multi-

science.metapress.com/content/w152x48065n16q43/?p=4d0d9a155b7d4c9e827b69c51c3a75b6&pi=4 (paywall)

www.uoguelph.ca/~rmckitri/research/MM.EE2005.pdf (Gone??)

Meme-18¹, Meme-c¹, Meme-d¹.

#46 mXRB

McIntyre, Stephen and McKittrick, Ross (2005b) “Hockey sticks, principal components, and spurious significance,” *Geophysical Research Letters*, **32**, L03710, doi:10.1029/2004GL021750 #48

climateaudit.files.wordpress.com/2009/12/mcintyre-grl-2005.pdf

Meme-18¹.

In some sense, this should have its own color, as it is a peer-reviewed article in a credible journal, but it is the only one like that from MM in this list, so it is kept as red.

#47 mXRB

Moberg, A., Sonechkin, D.M., Holmgren, K., Datsenko, N.M., Karlen, W., Lauritzen, S.E. (2005) “Highly variable Northern Hemisphere temperatures reconstructed from low and high-resolution proxy data,” *Nature*, **433** (7026):613-617.

www.nature.com/nature/journal/v433/n7026/pdf/nature03265.pdf

stephenschneider.stanford.edu/Publications/PDF_Papers/MobergEtAl2005.pdf

Meme-01², perhaps. Theme-H⁴

They write:

“We find no evidence for any earlier periods in the last two millennia with warmer conditions than the post-1990 period—in agreement with previous similar studies... This does not imply that the global warming in the last few decades has been caused by natural forcing factors alone, as model experiments that use natural-only forcings fail to reproduce this warming.”

Moberg, et al disagree with MBH99, not so much on the MWP, but on the depth of the LIA, as in Esper (2002). These arguments are normal within-science arguments about variability.

4#48 mXRB

Osborn, Timothy J. and Briffa, Keith R. (2006) “The spatial extent of the 20th-century warmth in the context of the past 1200 years,” *Science*, **311**, 841-844.

www.sciencemag.org/cgi/reprint/sci;311/5762/841 (paywall)

www.sciencemag.org/cgi/data/311/5762/841/DC1/1

www.realclimate.org/index.php/archives/2006/02/a-new-take-on-an-old-millennium

Meme-56¹, Theme-G⁴, Theme-H⁴.

WR mis-edit loses the clear geographic restrictions of the MWP.

4#49 mXRb

Rutherford, S., Mann, M. E., Osborn, T. J., Bradley, R. S., Briffa, K. R., Hughes, M. K., and Jones, P. D. (2005) “Proxy-based Northern Hemisphere surface reconstructions: Sensitivity to method, predictor network, target season, and target domain,” *Journal of Climate*, **18**, 2308-2329.

www.meteo.psu.edu/~mann/shared/articles/RuthetalJClimate05.pdf

5#51 mUgN

Tennekes, H. (1991) “Karl Popper and the accountability of numerical weather forecasting,” in *New Developments in Predictability*, ECMWF Workshop Proceedings, ECMWF, Shinfield Park, Reading, U.K.

ECMWF is a credible organization, but climate modeling differs from weather forecasting, and a 1991 paper on that topic seems irrelevant.

Henrik Tennekes is a retired long-time AGW contrarian, and his recent essays appear at sites like SEPP and SPPI, *not a sign of credibility*:

scienceandpublicpolicy.org/commentaries_essays/tennekes_climate_models.html

He is mentioned by McKittrick in #79, Link-M¹.

5#52 UGN

Valentine, Tom (1987) “Magnetics may hold key to ozone layer problems,” *Magnets*, **2** (1) 18-26.

*This uncited reference alone raises a serious question of basic scholarly competence*⁷⁷. *It is utterly bizarre, especially in a report criticizing the*

quality of review elsewhere. I could not find an online copy, but a 1987 ozone article is at best irrelevant bibliography-padding.

“MAGNETS *In Your Future*” was an obscure fringe-science magazine, for which Valentine wrote articles and later served as Editor. He had a long history of writing on fuel-less engines, psychic surgery (books, see Amazon) and conspiracy theories, for a tabloid, *The National Tattler*. His Bio states of that work:

“(Miracle editor—had to come up with a miracle a week!)”

Some examples and background are:

web.archive.org/web/20050208000510/tomvalentine.com/html/about_tom1.html his Biography

www.rexresearch.com/evgray/1gray.htm#1 “Man Creates Engine That Consumes No Fuel...”

www.rexresearch.com/elxgnx/elxgenx.htm “electrogenic agriculture”

www.rexresearch.com/nemes/1nemes.htm#magnets invention suppression

His later talk show often promoted “black helicopters” conspiracies:

en.wikipedia.org/wiki/Black_helicopter

For more discussion, and credits to various people, see :

scienceblogs.com/deltoid/2010/05/wegman_and_black_helicopters.php

<E> *The WP should be asked if someone else gave them this, if they found it themselves, if anyone actually read it, why it is mentioned at all, and why it is labeled an academic paper along with papers in Science or Nature..*

4#53 mUS

von Storch, Hans and Zorita, Eduardo (2005) “Comment on ‘Hockey sticks, principal components, and spurious significance, by S. McIntyre and R. McKittrick,” *Geophysics Research Letters*, **32**, L20701, doi:10.1029/2005GL022753, 2005.

www.agu.org/pubs/crossref/2005/2005GL022753.shtml

coast.gkss.de/staff/zorita/ABSTRACTS/2005_von_Storch_et_al__Comment_on_hockey_stick_GRL.pdf

 von Storch, et al have often argued over variability, strengths and weaknesses of various reconstructions, but within normal science. This peer-reviewed criticism was ignored by the WR, hence the label.

⁷⁷ It probably deserves a color code all its own.

④#54 mXRb

von Storch, H. E., Zorita, E., Jones, J., Dimitriev, Y., González-Rouco, F., and Tett, S. (2004), “Reconstructing past climate from noisy data,” *Science*, 306, 679–682.

www.sciencemag.org/cgi/reprint/306/5696/679.pdf (paywall)

Meme-05🟡, perhaps.

www.realclimate.org/index.php/archives/2006/04/a-correction-with-repercussions It has problems.

④#55 mXUs

von Storch, H. E., Zorita, E., Jones, J. M., Gonzalez-Rouco, F. and Tett, S. F. B. (2006) “Response to Comment on ‘Reconstructing past climate from noisy data’,” *Science*, 312, 529c.

www.sciencemag.org/cgi/reprint/306/5696/679.pdf

④#56 mXrSB

Wahl, E. R. and Ammann, C. M. (2006) “Robustness of the Mann, Bradley, Hughes reconstruction of the Northern hemisphere surface temperatures: Examination of criticisms based on the nature and processing of proxy climate evidence,” *Climatic Change*, in press.

www.cgd.ucar.edu/ccr/ammann/millennium/refs/Wahl_ClimChange2007.pdf

www.cgd.ucar.edu/ccr/ammann/millennium/CODES_MBH.html :

“In Summary, it can be clearly stated that none of the warm 15th century reconstructions turn out to be statistically and climatologically meaningful. The centering issue raised by McIntyre and McKittrick in GRL (2005a), if approached properly (i.e. using full standardization of individual records), is influencing the reconstruction in a minor way and is in fact confirming the robustness of the MBH reconstruction within its own framework.”

 This full-length, peer-reviewed paper was effectively ignored by the WR, dismissed in a one-line footnote, Theme-K🟡. See also:

www.realclimate.org/index.php/archives/2006/07/the-missing-piece-at-the-wegman-hearing

This entry follows that in the WR, but since this was published in 2007, it is often referenced as Wahl and Ammann(2007) in later discussions.

④#57 mXrsb

Wahl, E. R., Ritson, D. M., and Ammann, C. M. (2006) “Comment on ‘Reconstructing past climate from noisy data’,” *Science*, 312, 529b.

www.sciencemag.org/cgi/reprint/312/5773/529b.pdf (paywall)

This is a comment on von Storch, et al (2004), but is plausibly relevant.

See also Ritson discussion, W.4.1.

③#58 Rn

Wunsch, C. (2002) “Ocean observations and the climate forecast problem,” in *Meteorology at the Millennium*, (R. P. Pearce, ed.) London: Academic Press, 233 (-245, (sic)).

ocean.mit.edu/~cwunsch/papersonline/rmsbookpaper.pdf

Meme-05🟡, Theme-E🟡.

③#59 MRNb

Wunsch, C. (2006) “Abrupt climate change: An alternative view,” *Quaternary* (sic) *Research*, 65, 191-203.

ocean.mit.edu/~cwunsch/papersonline/abrupt2006.pdf

Link-M🟡, Meme-21🟡, Meme-02🟡, Theme-E🟡

climateaudit.org/2005/08/20/a-new-preprint-by-wunsch-on-abrupt-climate-change

B. Other Literature Including Articles in the Popular Press

It is strange to see so many popular press articles referenced, but none of them are cited anyway. It is strange that the (heavily reviewed, high-credibility) **IPCC TAR** and a major **NOAA report** are listed here, amidst articles in *Newsweek* and the *Washington Times*.

Four popular press articles during 1973-1975 (#61, #63, #72, #73 below) discuss the global cooling of that time and cannot be relevant here. Those are often used to support anti-science Meme-08🟡 “Scientific consensus predicted impending ice age in the 1970s,” implying climate science was wrong then, hence untrustworthy.

It did not and they were not wrong. This was a popular press issue.

www.skepticalscience.com/ice-age-predictions-in-1970s.htm

ams.allenpress.com/perlserv/?request=get-abstract&doi=10.1175%2F2008BAMS2370.1

<eb> Why would these be here, except to introduce this Meme?

Legates, Lindzen, Michaels and Douglass have long histories of providing climate anti-science in non-peer-refereed places. The Financial Post, WSJ,

and Washington Times have a long history of offering them OpEd slots. The histories are described in [MAS2010].

● #61 UGNb

‘Colligan, Douglas (1973) “Brace yourself for another ice age,” *Science Digest*, 73 (2), 57-61.’

<eb> Meme-08●

● #62 MUGB

Crok, Marcel (2005) “Proof that mankind causes climate change is refuted: Kyoto protocol based on flawed statistics,” *Natuurwetenschap & Techniek*, February 2005.

climateaudit.org/2005/03/04/german-translation-of-marcel-croks-article-with-new-comments

www.uoguelph.ca/~rmckitri/research/Climate_L.pdf

Crok had studied chemistry, but from Google Scholar had never published any peer-reviewed papers. He was Editor-in-Chief of a tennis magazine, and (2003-2009) a science writer for NWT, a Dutch popular science magazine, not a peer-reviewed journal. He was in contact with McIntyre by 1Q05, hosted him on one trip and the article’s title speaks for itself.

 Did MM provide this, either directly or through P.Spencer? The reference is not to the original Dutch version, but to an English translation, hosted at McKitrick’s website mentioned by McIntyre, Link-M●, Meme-18● Meme-56●.

● #63 UGNb

Gwynne, Peter (1975) “The cooling world,” *Newsweek*, April 28, 1975.

<eb> Meme-08●

● #64 mRsB

Intergovernmental Panel on Climate Change (2001) *Climate Change 2001: Third Assessment Report, IPCC (Especially Chapter 2: ‘Observed climate variability and change,’ Folland C. K. and Karl, T. R., coordinating lead authors).*

www.grida.no/publications/other/ipcc_tar/?src=/climate/ipcc_tar/wg1/index.htm This is usually cited here as [IPC2001].

This plausibly should have a Summary of its own, but at least belongs in group A.

 It is very odd to place it with articles in Newsweek and OpEds, but in any case, the WR generally ignored it.

● #65 Ug

‘Kerr, Richard A. (2006) “Yes, it’s been getting warmer in here since the CO2 began to rise,” *Science*, 312, 1854.’

www.sciencemag.org/cgi/reprint/312/5782/1854.pdf (paywall)

This straightforward 1-page article discusses the NRC results and uncertainty, as in the difference between 66% confidence or higher levels of confidence in the millennial unusualness of current temperatures. However, it is a news article, not peer-reviewed.

● #66 MUGB

Legates, David (2005) “Where’s the data?: holding science to prospectus standards would stop climate researchers from launching misrepresentations like the ‘Hockey Stick’,” *Financial Post*, September 20, 2005.

 The same piece appeared in the National Post and was available on McIntyre’s website the same day. It is an OpEd promoting MM.

Link-M●

climateaudit.org/2005/09/20/legates-op-ed

● #67 UGNB (2005)

Lindzen, Richard (2001) “Scientist’ report doesn’t support the Kyoto Treaty,” *Wall Street Journal*, June 11, 2001.’

eaps.mit.edu/faculty/lindzen/OpEds/LindzenWSJ.pdf

Meme-02●, Meme-03●, #08, #24.

 He writes:

“Science, in the public arena, is commonly used as a source of authority with which to bludgeon political opponents and propagandize uninformed citizens. This is what has been done with both the reports of the IPCC and the NAS.”

This is irrelevant, *seems just a climate anti-science Meme-carrier.*

🟡#68 UGB

Michaels, Patrick J. and Douglass, David H. (2004) “Global-warming sciences meltdown,” *Washington Times*, Aug 16, 2004, page A17.

 The *Washington Times* is not the *Washington Post*:

www.sourcewatch.org/index.php?title=Washington_Times

Meme-20🟡, -36 (at least).

That was unlocateable, so I used the following, assuming it similar:

Patrick J. Michaels, S. Fred Singer, and David H. Douglass, “Meltdown for Global Warming Science,” August 19, 2004.

www.cato.org/pub_display.php?pub_id=2789

🔴#69 MUGb

Muller, Richard (2004) “Global warming bombshell,” *MIT Technology Review*, www.technologyreview.com/BizTech/wtr_13830,296,p1.htm

This is a popular technology magazine, not peer-reviewed.

 Muller is a UC Berkeley physicist who apparently accepted MM views and passed them along. He repeated some of this in his 2008 book *Physics for Future Presidents*, despite results of the intervening years.

Most of his AGW discussion otherwise matched mainstream climate science. *Muller is certainly an accomplished, eclectic physicist, but these publications offer no evidence that he has followed the hockey stick issues in any real detail*, despite being a member of North’s NRC panel, A.1.1. McK05 mentioned him as a contact, Link-M🔴, Meme-18🔴.

🟢#70 Rn

NRC (1995) *Research-Doctorate Programs in the United States*, National Academy of Sciences, Washington, DC.

www.nap.edu/readingroom.php?book=researchdoIDage=

This is a credible source, but is only used for the seemingly irrelevant footnote on p.48 about Yale and Penn State.

Perhaps it is here to add an NRC reference?

🟢#71 UN

NOAA (2005) *SAP-1.1 Prospectus for Temperature Trends in the Lower Atmosphere: Understanding and Reconciling the Differences*, Climate Change and Assessment Program Synthesis and Assessment Product

1.1 www.climate-science.gov/Library/sap/sap1-1/default.php

This is a valuable, credible source, but has no obvious relevance, as it deals with modern temperature issues, post-1950, not paleoclimate reconstructions. It references none of the WR Important Papers. The Third Draft was posted 3/15/06 and the Final Report 05/02/06, long past the Prospectus. *It seems like “bibliography-padding.”*

 Of course, studying this should leave zero doubt of the reality of the “blade” of the hockey stick, Theme-H🔴.

🟡#72 UGNb “Ice age predicted in the 1970s.”

Sullivan, Walter (1975a) “Scientists ask why world climate is changing: Major cooling may be ahead,” *The New York Times*, p. 92, May 21, 1975.

<eb> Meme-08🟡.

🟡#73 UGNb “Ice age predicted in the 1970s.”

Sullivan, Walter (1975b) “Climatic changes by aerosols in atmosphere feared,” *The New York Times*, pg. 1, September 14, 1975.

<eb> Meme-08🟡.

🟡#74 Ugb

Zidek, James V. (2006) “Editorial: (post-normal) statistical science,” *Journal of the Royal Statistical Society (A)*, 169, pp. 1, 1-4.

www3.interscience.wiley.com/journal/118612253/abstract

Meme-56🔴

Published online 16 Dec 2005. I did not read this (*at \$30, it did not seem worthwhile*), but extracts were found at McIntyre’s website:

climateaudit.org/2006/03/26/two-editorials He quotes Zidek:

“Even the Intergovernmental Panel on Climate Change is not spared. McIntyre (2005) asserted that the hockey stick diagram prompted the Panel to change its 1990 conclusion that the mediaeval period was warmest in the last millennium, in favour of the 1990s. However, he wrote that he and his co-author, Ross McKittrick, have shown that the hockey stick diagram resulted from flawed analysis, in particular from data mining methods that would have produced hockey stick patterns even in a random series (see for example McIntyre and McKittrick (2005)).”

www.stat.ubc.ca/~jim UBC, British Columbia

www.stat.ubc.ca/~jim/jzall.html Publications

Zidek’s extensive list of publications neither shows obvious involvement with paleoclimate up to that point nor includes this (rarely cited) piece.

 He seems a productive statistician who relied on then-recent work by MM to support strong statements about a relatively unfamiliar domain. Perhaps Canada is the connection?

C. Presentations

Talks are generally not peer-reviewed. Certainly talks for NAS or AGU carry more weight than for GMI or the “APEC Study Group.” None are cited anyway. Those labeled as talks for NAS (#75, #77, #80) were for the NRC Committee run by Gerald North.

④ #75 Ug

Mann, Michael E. (2006) “Climate Over the Past 1-2 Millennia,” presentation at the National Academy of Science, March 3, 2006

④ #76 Ug McI05

McIntyre, Stephen (2005) “Low-Frequency Ranges in Multiproxy Climate Reconstructions,” AGU December 2005 Meeting

④ #77 Ug MM06

McIntyre, Stephen and McKitrick, Ross (2006) “Surface Temperature Reconstructions for the Past 1,000-2,000 Years,” presentation at the National Academy of Science, March 3, 2006.

www.climateaudit.info/pdf/NAS.M&M.pdf

This included the sketch from [IPC1990].

Follow-up comments are:

ruby.fgcu.edu/courses/twimberley/EnviroPhilo/NASPanel.pdf

④ #78 MUGB_MM05x

‘McIntyre, Stephen and McKitrick, Ross (2005) “The Hockey Stick Debate: Lessons in Disclosure and Due Diligence,” September 7, 2005’

www.marshall.org/pdf/materials/316.pdf

This is important enough to have its own section, **W.8.9**. Many of the Memes can be found here.

④ #79 MUGB McK05

McKitrick, Ross (2005) “What is the ‘Hockey Stick’ debate about?” APEC Study Group, Australia, April 4, 2005.

www.uoguelph.ca/~rmckitri/research/McKitrick-hockeystick.pdf

This was a talk for an Asia-Pacific Economic Cooperation Group. Talks to economic groups are not very strong references for climate science.

Most of this material is also part of MM05x, although a few fascinating changes occur, as the correct attribution to Deming (*JSE*) is replaced by Denning (*Science*).

 for numerous Memes, Link-M④.

③ #80 Ug

von Storch, Hans and Zorita, Eduardo (2006) “Reconstruction of historical temperatures during the last millennium - analysis of methods - significance of anthropogenic climate change,” presentation at the National Academy of Science, March 3, 2006

W.8.9 MM05x, THE KEY SOURCE**78 MUGB_MM05x**

‘McIntyre, Stephen and McKittrick, Ross (2005) “The Hockey Stick Debate: Lessons in Disclosure and Due Diligence,” September 7, 2005’

www.marshall.org/pdf/materials/316.pdf

DC reminds me there were 2 talks, but this is this the record of one:

climateaudit.org/2005/05/07/upcoming-washington-trip

“Ross McKittrick and I will be making two presentations in Washington on May 11 sponsored by Cooler Heads Coalition/George Marshall Institute: 12.20 at the National Press Club and 3 pm somewhere on Capitol Hill.”

MM05x seems a plan for the WR, but is never actually cited, just listed as (vague) reference at the end. MM were GMI “Experts,” unmentioned.

Figure 4, p.5 also shows the 1990 IPCC graph, **W.4.2**. Figure 5, p.6 shows the Huang, et al (1997) borehole graph **W.4.3**.

September 7, 2005 date is intriguing. Wegman was contacted 09/01/05.

Was this document one of the earliest provided to the WP?

 for numerous Memes, Link-M. *Obvious memes are shown here:*

Meme-02 [change]**p.6**

“So if you want to sell the story that we are now in uncharted territory as far as the climate goes and that we are experiencing unusually rapid and unprecedented warming conditions, it is very hard to do that if you have the Medieval Warm Period sitting in the background suggesting that this isn’t at all unprecedented.”

Meme-03 [consensus]**p.26**

“So part of the appearance of consensus is created just by deleting an unfavorable portion of a record.”

I don’t recall this showing up in WR, but in any case, DC refuted it:

deepclimate.org/2010/06/29/revisiting-tar-figure-2-21-part-1-another-false-claim-from-steve-mcintyre

p.35

Enron – “So consensus can be fragile.”

Meme-18 [hockey]**p.4**

“I am taking some pains to emphasize how important the hockey stick graph was to the IPCC.”

Meme-21 [change]**p.6**

“So if you want to sell the story that we are now in uncharted territory as far as the climate goes and that we are experiencing unusually rapid and unprecedented warming conditions, it is very hard to do that if you have the Medieval Warm Period sitting in the background suggesting that this isn’t at all unprecedented.”

Meme-32 [oldice]**p.5**

“...showing a long Medieval Warm Period, then a Little Ice Age and then a recovery to the present (Figure 4).”

Meme-56 [MWP] **This is really popular in the WR.****p.4**

“...it is helpful to go back to the earlier IPCC report and look at what we might call the Medieval Warm Period problem.”

p.5

Figure 4 seems the *likely* antecedent of WR Figure 4.5, discussed **W.4.2**.

“This was reflected in the IPCC 1990 report which has a schematic illustration of the state of the climate,...”

p.6

MM misattribute a never-confirmed quote by David Deming in fringe science Journal of Scientific Exploration (JSE) to D. Denning (sic) in *Science*, although the earlier McK05 got it right. The key claim is that some climate scientist emailed Deming to say:

“We have to get rid of the Medieval Warm Period.”

This Meme continues to be promoted [MON2010].

Meme-107 [diverge]**p.24**

“There is an increase up to about 1960 and then the proxy comes down. How do Briffa et al. explain that? They say there is some unknown anthropogenic factor causing this series to go down.”

Meme-a [ipcc=hs]

p.4

“I am taking some pains to emphasize how important the hockey stick graph was to the IPCC.”

Meme-b [badpeer]

p.18 shows coauthorships

p.28

“Going back to the original representation of what had been done: the representations made to the government are that this has been rigorously reviewed, that every step along the way has been checked, that we have engineering quality and due diligence. Well, we don’t.”

p.30 More on peer review

“Bruce McCullough has written a working paper for the Federal Reserve Bank of St. Louis on this recently for economics and cites our work as being an interesting example illustrating this process in another area.”

The WR references that as Anderson, Greene, McCullough, Vinod (2005). That seems a “Meme-laundrying” in which McKittrick gave material to McCullough, **W.8.8**. See also **A.12.5** for more on McCullough.

Meme-c [no-indy], leads to WR §5.8, **W5.8**.

p.16

“Another argument is that there are other studies which arrive at the same conclusion. I have two responses to that. Even if these other studies were correct, which I don’t think they are, that wouldn’t salvage Mann.”

p.17

“First of all, the studies and proxy data are not independent...”

Meme-d [one scientist]

p.16

“I think it is an absolute scandal, not just for Mann and his associates, but for the entire discipline that one particular scientist is allowed to be a prima donna on this.”

Meme-e [confounding]

p.11

“As a result, the dominant pattern in that hockey stick graph is non-climatic; it is not a temperature signal.”

p.15

“There is a very definite alternative possibility in this hockey stick, suggesting that all we are doing is getting a non-climatic effect on bristlecone pines which is being carried forward into the reconstruction and that the reconstruction itself is not an authentic signal of temperature.”

Meme-f [faux fight]

MM could not do this themselves, but the whole point of the MM effort was to replace the (tired) astrophysics-based Meme-01 [It’s the sun] [sun] of Baliunas and **Soon** by a statistics-based effort, ideally finding some respected statistician to help.

Meme-g [generalspecific]

p.9

“It has to do with the way the data is standardized or centered only against the ending portion of the data series, rather than against the whole length of the data series.”

This is the (real) decentering problem, but as Wahl and Amman (2007) showed, it makes very little difference in this case. MM throw out the data they don’t like, then generate red noise with inappropriate parameters.

Meme-g [statisticians]

Theme-H, see **A.2**. Wegman followed this well.

Theme-N

“*Question*: Is it conceivable, however, in the 21st century that the additional amounts of greenhouse gases that have been poured into atmosphere as a result of human activity may tip that balance?”

McIntyre: We are not commenting on that.”

These are related manifestations of the same approach – ignore awkward questions and never, ever admit that AGW might be real.

p.8, Figure 6

MM reconstruction boosts pre-1500 warming, **W4.4**.

p.9 Figure 7 graphs, same code seems used for WR §4.4, **W4.1**.

p.10 Bristlecone pines, Graybill, Idso, CO₂ fertilization

p.13-14-MM mention von Storch, Dutch science magazine (i.e., really Crok (2005)), Muller (2004), Cubasch, Tennekes.

W.9 App. A. Math. Underpinnings of PCA

The writing style here differs strongly from the rest of the WR and I believe was written by Scott. Most of WR pp.61-63 is a straightforward description of the basic mathematics of PCA, with only minimal reference to MBH:

“1995 and a training period 1902-1980 in which all the proxy variables are available. The data matrix is centered using the training data rather than the overall means. Because the training period has higher temperatures, this biases the overall data lower for the period 1400-1995, thus inflating the variance. In this case the right singular vectors, Z , are no longer the eigenvectors.”

This is true, but stops short of the follow-on analysis, as per Wahl, Ammann (2007) or [TAM12010]. It confirms the admitted decentering issue, but says nothing about the results if that issue were fixed.

One might guess that not many members of the House studied this mathematical Appendix very carefully. For the intended audience, one might normally expect to see a pointer to some standard source on PCA, followed by a nontechnical discussion. Most of the WR seems to have been created by Said and Wegman, except this Appendix A s was requested to make the WR look more impressive?

W.10 App. B. Request ... Chairman Boehlert

The first part simply repeats the request from Rep. Boehlert and the NRC's resulting statement of task [NRC2006, p.139].

“Note: Although the House Committee on Science initiated the Academy study, the Academy decided not to address the specific questions of the House Committee on Science and decided to focus of the Academy study away from the specific questions and address broader issues.”

The NRC answered the specific questions in the context of the general issues, as NRC committees tend to do, rather than ignoring them.

“We attempt here to give answers to the House Committee on Science questions.

- What is the current scientific consensus on the temperature record of the last 1,000 to 2,000 years?

Ans: There is strong evidence from the instrumented temperature record that temperatures are rising since 1850 and that global warming is a fact. How

accurate the reconstructions over the past millennium are is a matter of debate and **we do not believe there is a consensus** on this issue.”

 This is a remarkably binary statement for professional statisticians, §1.4, W.4.4. From data available to the WP, most points of most reconstructions fell within the error bars of MBH99, and if anything, most reconstructions showed lower MWP than did MBH99. Instead, the answer was “no consensus.”

What would it would it take to ever say “consensus?” Would every reconstruction have to agree within 0.1C at every date? Within .01C?

- What are the main areas of uncertainty and how significant are they?

Ans: The proxy data have many factors encoded in them, one of which is temperature. However, the temperature proxy is confounded with many other factors that have not been teased out including carbon dioxide fertilization effects. The high level of variability in the proxy data as well as the lack of low frequency effects make the reconstructions more problematic than the advocates of these methods would have us believe. In addition the lack of a really substantial stationary, instrumented temperature record handicaps the calibration.”

<eb> Once again, “confounding” is introduced, and once again, CO₂ fertilization is asserted by statisticians who are not tree-ring experts. The WP copied text from Bradley (1999), but apparently did not study it to understand the long efforts to handle the various confounding factors, W.6, Meme-e❶. *The WP repeatedly claims to focus on the statistics, but then asserts judgment regarding paleoclimate.*

- What is the current scientific consensus on the conclusions reached by Drs. Mann, Bradley and Hughes?

Ans: Based on the literature we have reviewed, there is no overarching consensus on MBH98/99. **As analyzed in our social network, there is a tightly knit group of individuals who passionately believe in their thesis. However, our perception is that this group has a self-reinforcing feedback mechanism and, moreover, the work has been sufficiently politicized that they can hardly reassess their public positions without losing credibility.**”

 This again offers a binary “no consensus,” but goes on to Meme-b❶. *Recalling the composition of the WP and its helpers, and the other scholarship demonstrated here, how would the reader evaluate this comment?*

“• What are the principal scientific criticisms of their work and how significant are they?

Ans: Our perception is that principal components (statistical) analysis was used incorrectly and, based on this, **unsupported inferences** were drawn about the current magnitude of global warming relative to the historical past. We hasten to repeat that the Earth is getting warmer. What does not appear to be true is that the process mechanism is as well understood as some scholars would have us believe. In addition the use of some proxies does not appear to be as carefully managed as we might like.”

 MBH99 used decentered PCs, agreed before. In the peer-reviewed literature, Wahl and Ammann (2006) showed that it did not really matter, but the WP essentially ignored that work, [TAM2010].

<eB> “What does ... believe.” *What sort of statement is that? Can anyone refute a vague charge like that?* There is no evidence that the WP had the slightest knowledge of climate science’s understanding of physical processes, Theme-B⁴, Theme-C⁴.

“• Has the information needed to replicate their work been available?

Ans: In our opinion, the answer is no. As mentioned earlier, there were gaps in MBH98.”

Perhaps, had they talked to Mann as they did to McIntyre, this might not have been a problem. In addition, they managed to ignore or denigrate all the other reconstructions, in order to focus on old papers.

“• How central is the debate over the paleoclimate temperature record to the overall scientific consensus on global climate change (as reflected in previous reports from the Academy)?

Ans: **In a real sense the paleoclimate results of MBH98/99 are essentially irrelevant to the consensus on climate change.** The instrumented temperature record since 1850 clearly indicates an increase in temperature. Whether this is unprecedented in the last millennium seems less clear **and to what extent the natural planetary processes can mitigate the excess green-house gas release is unknown.** What is more important in our view is real insight into and understanding of the processes of global warming.”

 Does this make any sense? If MBH98/99 was irrelevant, why did the WP spend a great deal of effort writing the WR? The WP keeps emphasizing physical processes. Why did they not talk to climate scientists? Why did they not carefully study the IPCC TAR, for example? Theme-B⁴, Theme-C⁴, Theme-H⁴, Theme-N⁴.

“• How central is the work of Drs. Mann, Bradley, and Hughes to the consensus on the temperature record?

Ans: MBH98/99 has been politicized by the IPCC and other public forums and has generated an unfortunate level of consensus in the public and political sectors and has been accepted to a large extent as truth. Within the scholarly community and in certain conservative sectors of the popular press, there is at least some level of skepticism.”

<EB> “politicized by the IPCC”?

The reader might revisit §1.2-1.6 for the reasons behind the intense attack on the hockey stick.

It is fascinating to learn that the WP thought that the opinions of conservative sectors of the popular press mattered to scientific truth. The work got “politicized” because a determined multi-year effort was mounted against it by MM+TT and others, culminating in the WR.

What is truth? Who is the “scholarly community”? Theme-J⁴, Theme-K⁴. Paleoclimate scientists worry about error bars, not absolute truth. The real issue is that the hockey stick was a clear graphic that made sense to the public, a serious problem for some people.

W.11 App. C. Summaries of Important Papers

W.11.1 Summarization practices

The WR's "Summaries of Important Papers," pp.69-92, includes 17 references (16 papers, one PhD dissertation), of course identified.

Good scholars show they understand a source by explaining it mostly in their own words. Direct quotes sometimes make sense, but should be relatively minimal and clearly marked. A *good* summary explains the relevance and importance of a paper. It might explain the paper's relationship to others, although such discussion is often gathered elsewhere, as it was, somewhat, in the WR. Experts know which papers are important and why, and can easily do this. Others may have trouble.

Summarization has its own good practice issues, which may differ somewhat from unacknowledged sourcing and whose interpretation seems to vary more among organizations:

www.plagiarism.org/plag_article_plagiarism_faq.html

riceowl.rice.edu/guidance.cfm?doc_id=11767 Rice U example

The US Office of Research Integrity (ORI) discusses ethical writing:

ori.hhs.gov/education/products/plagiarism.

ori.hhs.gov/education/products/plagiarism/7.shtml

ori.hhs.gov/education/products/plagiarism/8.shtml

GMU's English Department offers good comments on plagiarism and paraphrasing, especially relevant to the GMU-led WR:

writingcenter.gmu.edu/resources-template.php?id=1

"The English Department Statement of Plagiarism:

Plagiarism means using the exact words, opinions, or factual information from another person without giving the person credit. Writers give credit through accepted documentation styles, such as parenthetical citation, footnotes, or endnotes; a simple listing of books and articles is not sufficient. Plagiarism is the equivalent of intellectual robbery and cannot be tolerated in the academic setting."

"What is paraphrasing, and how do I do this?"

First, for paraphrasing, a good idea is to read the original, make sure that you understand it, lay it aside, and then write it down in your own words imagining that you are explaining it to someone who will read your paper. **If you are having trouble putting it into your own words, then you probably don't**

understand it well enough to write about it. When you are finished, cite the author according to the style you are using.

Always remember, borrowing (both language and syntax) too heavily from a source, even if you cite it, is plagiarism. A good thing to keep in mind is to use no more than two of the author's original words."

Unacknowledged cut-and-paste raises serious concern, as does labeling seemingly irrelevant papers "Important." Of course, summaries must often use key technical terms from the originals, so mere re-use of some key terms may be reasonable or even inescapable, but not large blocks of text. Large-scale plagiarism raises issues of research ethics and sometime legal issues, like copyright infringement or mis-use of research funding. Frequent errors raise concerns about the basics of scholarship common across academe. One-sided patterns of errors raise concerns regarding objectivity.

"Anti-Anti Plagiarism" software applications rearrange text slightly to defeat plagiarism checkers, such as:

sourceforge.net/projects/aaps

Similar effects can be produced by hand-done "pseudophrasing" :

www.iawp.ucr.edu/teacher_support/On+Plagiarism.pdf , p.6.

Gray Scott's discussions of "False citations and Masquerading Quotes" (p.7) and "Doctored Quotes" (p.9) are also useful.

W.11.2 Description of comparisons

W.11.8 displays side-by-side comparisons of all 26 pages. Reasonable people can disagree whether individual cases might be considered poor scholarship, perfectly acceptable practice, plagiarism or misrepresentation. Each Summary is quoted in full in the left column, with identifiable antecedents quoted on the right. In-line commentary is shown as:

"Indented, italic, in smaller font."

Each "Important Paper" is summarized on WR (page x -y) and listed in its bibliography (page z), shown here as **[pp.x-y, z]**. Each paragraph is labeled, but reformatted to align texts for quick visual comparison.

ID (IDentical) text uses **cyan-highlighted regular font** for identical words extracted in order from the antecedent. These words are identified by manual approximation of a local “longest common subsequence” comparison. Words whose choice seems arbitrary are especially findable, as opposed to difficult-to-avoid technical terms. Nearby words spelled identically (ignoring hyphens) and in order are marked **cyan** also. If many such are found together, technical terms and common words get included. The goal is to be accurate, but conservative.

SS (Striking Similarity) text is shown in regular font, thus including all **ID**, plus local paraphrases needing little knowledge. Whole blocks are labeled **SS** if they include substantial **ID** text and mostly minimal changes.

Yellow-highlighted SS words (**TC**) are Trivial Changes, akin to those done by students to foil plagiarism checkers. Some inject errors, inconsistencies or ambiguities. Obvious phrase movements are not highlighted, but are certainly considered **SS**.

Italics text seems reasonable summarization, text whose wording seemed inescapable and that for which no obvious antecedent is found. *Italics* might represent acceptable practice or not, but gives benefit of the doubt. Underlining shows possible “Issues,” such as cut-and-paste mis-Editing, Changes of meaning, or possible Bias. These are briefly explained by inline *Notes*, collected in **Table 2**.

Text is highlighted identically in both columns, and aligned to ease visual comparison. Antecedent ~~strike through~~ shows obvious simple deletions.

Total words = # **SS** + # *Italics*.

#**SS** = #**ID** + # **TC** + # (white, regular).

The total text of Summaries is rated ~50% ID, 81% SS.

Wegman, et al, p.69, Paragraph 2

Overall the network includes 112 proxies, and each series has been **formatted** into annual mean anomalies relative to the reference period used for this data, 1902-1980.

Certain **tree-ring datasets** have been represented by a **small** number of leading principal components.

1.<e> Replacing “formed” with “formatted” seems an odd word choice

MBH98, p.779

The long instrumental records have been **formed** into annual mean anomalies relative to the 1902–80 reference period...

Certain **densely sampled regional dendroclimatic data sets** have been represented ~~in the network~~ by a **smaller** number of leading principal components...

Words: (40, 30, 21, 75%, 53%), Issues: 1. This means: (**Total** words, **SS** words, **ID** words, %**SS**, %**ID**), **Issues: # of distinct issues (underlined).**

WR, p.80, Paragraph 4

They also note the limited due diligence of paleoclimate journal peer review and that it would have been prudent to have checked the **MBH98 data and methods against original data before accepting the findings as the main endorsement of the Intergovernmental Panel on Climate Change.**

1. <CB>. MBH98 as the main endorsement of the IPCC? Even MM did say that Meme-18. This major Meaning change exceeds the MM comment, but it fits Meme-18 and its surrounding uses.

MM05a, p.90

recognizing the limited due diligence of paleoclimate journal peer review, it would have been prudent for someone to have actually checked **MBH98 data and methods against original data before** adopting MBH98 results in the **main IPCC** promotional graphics.

The “results in the main IPCC promotional graphics” part seems fair. The WR made an explicit change amidst **ID** text.

Words: (45, 45, 28, 100%, 62%), Issues: 1. (**Total** words, **SS** words, **ID** words, %**SS**, %**ID**), **Issues: # of distinct issues (underlined)**

In the two examples, “1902-1980” (out-of-order), “**datasets**” ← “**data sets**” or “**small**” ← “**smaller**” (different spelling) are not counted as **ID**, but are **SS**. (DC uses a slightly more expansive definition for **ID**.) The first sentence is straightforward, with no obvious antecedent, so is *italic*. The first example’s metrics 75% **SS**, 53% **ID** approximate well those of the Summaries text as a group.

“Issues” are numbered within each Summary, and coded lowercase for questionable or lesser issues, uppercase for more important ones. The reader might examine these in context, as some are inherently subjective.

<**e or E**> shows an Error in editing an obvious antecedent, sometimes yielding odd English or problems like forgetting to change “our” to “their.” Some of these bear on basic research competence and care.

<**c or C**> shows a Change of meaning, of which some might be lack of understanding. Some would be very difficult to call accidents, falling more into Theme-N₄.

<**b or B**> shows Bias, where changes either weakened MBH and related papers or strengthened MM and other criticisms. Combinations ranged from <**e**>, a simple editing error, to <**CB**>, serious Change of meaning, clear Bias, in the examples shown.

Meme-n and Theme-n₄ list the first few found, if any.

Totals for each Summary in the WR.

Each page shows metrics for that page, and the last page shows the multi-page totals, carried to **W.11.3** and **W.11.4**. Words are counted as per Microsoft Word™, and percentages given. A few other notations may appear. The next 3 cases relate to Theme-E₄ on ocean oscillations:

Misplaced. (1 case) The Mann dissertation might plausibly be included as a reference, but does not seem very relevant.

Questionable Relevance. (2 cases) Wunsch (2002, 2006) may be good papers by a credible author, but seem irrelevant and especially odd to label as “Important Paper.”

Dubious Source. (2 cases) Two papers were published in *Energy&Environment* (E&E), whose quality of peer review (or lack thereof) cause it not to be included in the Web of Science, for example. Such papers tend to be ignored by field researchers, but MM03 and MM05a must be included as they are integral to the WR. However, the issue of journal reputation cannot be completely ignored, especially when the WR strongly denigrates the quality of peer review elsewhere. The typography for (the text boxes of) **W.11.8** has been explained above. Elsewhere, possibly subjective opinion is shown in *Italics*.

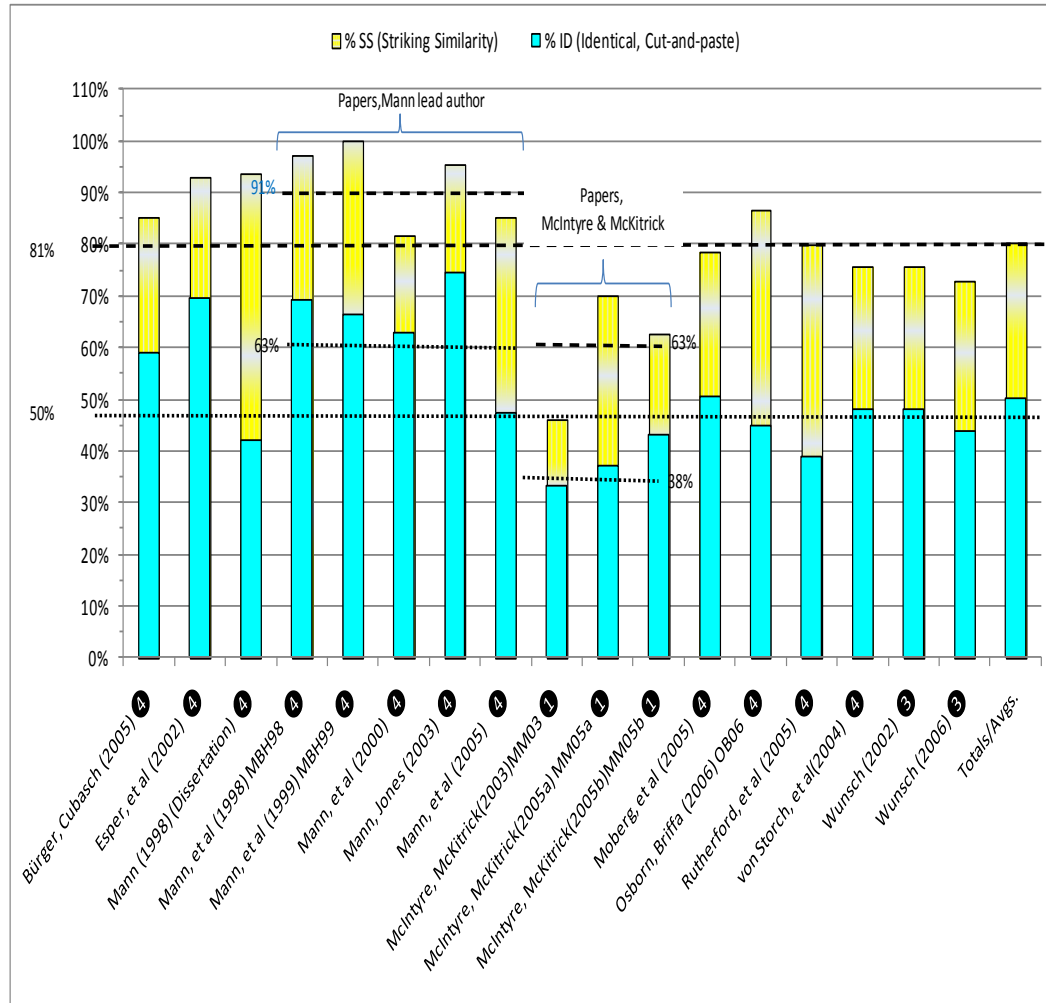
The next sections analyze and summarize data from **W.11.8**. At this point, readers can follow two different paths. Some can continue, later sampling **W.11.8** to assess believability of my conclusions. Those who prefer to first form their own impressions can skip to **W.11.8**, read as much as they can tolerate, and then return here. Few people are likely to study more than a small fraction of **W.11.8**, as the scholarship style is pervasive and quickly becomes repetitive. In order to gather clear evidence I had to look at everything. Most readers need not, but every detail is necessarily recorded, given the serious implications.

W.11.3 Text analysis of WR Summaries

The next lists and graphs analyses of the Summaries. As a group, papers with Mann as lead author scored 91% **SS**, 63% **ID**, shown by the lines. Mann (1998) is a dissertation, not a paper, so it is enough different to omit from the overall statistics Of the papers.

Those authored by MM scored 63% **SS**, 34% **ID**, shown similarly. Finding an antecedent is a clear positive, whereas not finding one is a weaker negative. Mann results are thus more definite than MM results. Even allowing for that, those two groups are treated differently. The reader might examine MBH99, a rather important paper. In side-by-side comparison, 66% of the Summary is **ID** with the rest as simple rewordings, so **SS** rates 100%. The WP may have understood that paper, but that summary provides little evidence of such understanding. MBH98 is about the same. *This seems a cursory process for two of the key papers.* Mann-led papers generate 24 of 54 “Issues” in papers. Adding Mann (1998) raises that to 37 of 67. *By contrast, MM summaries seem somewhat more like that expected in normal scholarship, actually studied a little.*

		"Important Papers"							
		Page, WR	Words				% Of Total		
			Total	SS	ID	% SS	% ID	# Issues	
"Important Papers" Metrics									
4	12	Bürger, Cubasch (2005) ④	87	395	337	233	85%	59%	1
4	23	Esper, et al (2002) ④	86	340	316	237	93%	70%	1
3	36	Mann (1998) (Dissertation) ④	67-	833	782	351	94%	42%	13
4	37	Mann, et al (1998) MBH98 ④	69-	561	546	389	97%	69%	5
4	38	Mann, et al (1999) MBH99 ④	71	502	502	333	100%	66%	8
4	41	Mann, et al (2000) ④	72-	832	680	524	82%	63%	8
4	42	Mann, Jones (2003) ④	77	244	233	182	95%	75%	3
4	43	Mann, et al (2005) ④	84-	476	406	225	85%	47%	0
1	44	McIntyre, McKittrick(2003)MM03 ①	75-	447	206	149	46%	33%	5
1	45	McIntyre, McKittrick(2005a) MM05a ①	79-	1069	751	397	70%	37%	2
1	46	McIntyre, McKittrick(2005b)MM05b ①	81	513	321	222	63%	43%	2
4	47	Moberg, et al (2005) ④	82-	537	422	272	79%	51%	2
4	48	Osborn, Briffa (2006) OB06 ④	92	400	347	180	87%	45%	3
4	49	Rutherford, et al (2005) ④	88-	623	499	243	80%	39%	4
4	54	von Storch, et al(2004) ④	78	395	316	225	76%	48%	1
3	58	Wunsch (2002) ③	74	463	351	223	76%	48%	1
3	59	Wunsch (2006) ③	90-	885	647	387	73%	44%	4
Totals/Avgs.			26	9515	7662	4772	81%	50%	67
Mann lead Totals/Avgs				2615	2367	1653	91%	63%	0
MM Totals/Avgs				2029	1278	768	63%	38%	0
Pages analyzed			26	All pages of WR Appendix C					
Pages with SS			25	p.76 (MM03) only has 1 sentence					



W.11.4 Summaries issue tally

This lists the 67 “Issues” found in Summaries, including 33 Errors <eE>, 24 Change of meaning <cC>, and 37 possible Bias issues <bB>. Each shows the page in the WR, # Issues found, classification of the Issues, Memes and Themes.

Some of these are marginal, some are very clear. Readers are urged to consider the weight of overall patterns, and examine these in context.

W.11.4(a) Summaries Issues Tally	Page WR	# Issues	Edit?	Change	Bias?	Memes	Themes	Issue
Bürger, Cubasch (2005)	87	1	e					1 "Using the 1902-1980 as the calibration period." Careless editing.
Esper, et al (2002)	86	1	e	c			F,G,H,J	1 "Climate variability" is odd wording for temperature.
Mann (1998)	67	13			b		E	1 "Attempts"
	67			c	b			2 "confounding" ← "conflicting", not quite the same.
	67				b			3 "contends", minor but negative connotations, seen elsewhere.
	67		e					4 "10-15 year" ← "16-18 year" seems odd
	68				b			5 ">"what he believes to be" weakening
	68		E	C	B			6 "this model..." converts a strength of model into a caveat
	68		e	c				7 "Gyre-scale" ← "gyre-advective", not a synonym, made precise term vague
	68		E	C				8 "dampen the meridional" ← "dampen the strengthening" serious error
	68				b			9 "contends"
	68		e	c				10 "decadal" ← "interdecadal" (10-20 years differs from 15-30 years)
	68				b			11 "contends"
	68				b			12 "contends"
	68				b			13 "defends"
Mann, et al (1998) MBH98	69	5			b		F,H,J	1 "attempt to estimate". They did estimate. "Attempt" seems a weakening.
	69		e					2 "formatted" ← "formed", odd wording from poor editing.
	69				b			3 "only enough" wording seems to add weakening connotation.
	69		E					4 "in our network" - ID, forgot to edit "our" to "their".
	69			c				5 "In their (NH) reconstruction." sometimes careless with NH vs global
Mann, et al (1999) MBH99	71	8	e				F,H,J	1 "In this article..." is almost a repeat of previous sentence,bad ID editing.
	71				b			2 "attempt to reconstruct..." ←reconstructed. Weakening connotation.
	71		e					3 "warmer even before the period" β awkward ID editing.
	71		e	c				4 "The explained variance..." misstates the antecedent.
	71		e					5 "is consistent with time" ← "holds up over time", odd ID wording.
	71		E					6 "Our reconstruction..." forgot to edit to "their".
	71		E	C	B	59		7 "beginning in the 14th century" ← "following the 14th century", not same
	71				b			8 "further conclusions cannot be drawn" ← "more confident...". Weakening.

W.11.4(b) Summaries Issues Tally	Page	WR	# Issues	Edit?	Change	Bias?	Memes	Themes	Issue
Mann, et al (2000)	72		8	E				F,H,J	1 Several paragraphs were copied (almost) directly from MBH98 summary.
	72			e					2 " <u>formatted</u> " β "formed", odd wording from ID. Same as MBH98.2.
	72			E					3 " <u>in our network</u> " - ID, forgot to edit "our" to "their", same as MBH98.4.
	72			E					4 " <u>we resolve</u> " - ID, forgot to edit to "they find" (as done in MHB98)
	73			E	C	B	02		5 " <u>only greenhouse gas forcing</u> " β- "only human greenhouse gas forcing". Some GHG changes are natural. Significant meaning change amidst ID.
				e	c				6 " <u>irradiation</u> " ← "irradiance" change for sake of change, different meaning.
	73				c	B	01		7 "...solar... <u>less significant</u> " ← "considerably less significant" Weak.
	73				c	B			8 " <u>anthropogenic...contributing to recent warming</u> ", Weaker than original.
Mann, Jones (2003)	77		3		c	B		F,H,J	1 " <u>previous Mann reconstructions</u> " - lost context of others' reconstructions, Weak. This confused reconstructions with model simulations.
	77					b			2 " <u>needed to decrease</u> " ← "should help decrease", Weaker, although minor.
	77					b			3 " <u>definitely</u> " ← "less definite" - Weaker, although minor.
Mann, et al (2005)	84		0						No issues found.
McIntyre, McKittrick(2003)MM03	75		5		c				1 MM03 mostly ref's MBH98; WR sometimes confuses with MBH99 or IPCC.
	75					b			2 " <u>attempted to reconstruct</u> " ← "construct". Stronger than source.
	75					b			3 "Having accounted for the major errors..." Stronger, judgement call, minor.
	75					b			4 " <u>data unreliable and obsolete</u> ". This is a sweeping statement. Strong.
	75			E		B	59		5 MM03 created curve with 1450AD noticeably higher than today, See A.4.2 .
McIntyre, McKittrick(2005a) MM05a	80		2			B	B		1 " <u>the limited due diligence of paleoclimate journal peer review</u> " copied
					C	B	A		2 "accepting the findings as the main endorsement of the IPCC" ← "adopting MHB98 results in the main IPCC promotional graphics." Meme#B .
McIntyre, McKittrick(2005b)MM05b	81		2			b	19		1 " <u>one man</u> " added in middle of ID text. Strengthen?
	81					B			2 " <u>thereby refuting the conclusions made by MHB98</u> " is very Strong.
Moberg, et al (2005)	83		2	e				H	1 " <u>Mann et al 1998</u> " ← "Mann and Jones", mis-edit.
	83				C	B	01		2 " <u>especially as a reponse to solar irradiance</u> " changes emphasis, Meme#01 . Moberg et al clearly include other forcings, not over-emphasizing solar.

W.11.4(c) Summaries Issues Tally	Page	WR	# Issues	Edit?	Change	Bias?	Memes	Themes	Issue
Osborn, Briffa (2006) OB06	92		3	e				G, H	1 " <u>entire millenia</u> " ← "the whole of the millenium". Edit error.
	92				c	B			2 " <u>While all these studies supported the claim...</u> " changed strong statement on
	92			E	C	B	59		3 " <u>analysis geographically restricted</u> " ← "(MWP) "geographically restricted"
Rutherford, et al (2005)	88		4			b		F,H,J	1 " <u>flawed</u> " ← "limited". Weakened.
	88			e					2 " <u>constructions</u> " ← "reconstructions" not same, bad editing.
	88			e					3 " <u>this type of Northern Hemisphere temperatures</u> ", awkward edit of source.
	89					b			4 " <u>maintained</u> " slightly negative connotation, minor
von Storch, et al(2004)	78		5	e					1 " <u>Millenia</u> " ← "1000 years", editing error.
	78			e	c		05		2 " <u>models like MBH98</u> " ← "methods like MBH98". Methods, models not same.
	78			e					3 "estimations" ← "variations", poor edit, confusion, strange wording.
	78				c	b			4 " <u>validity</u> " ← "limitations", Stronger.
	78					b			5 " <u>Similar problems</u> " lost von Storch qualifiers, Stronger criticism.
Wunsch (2002)	74		1			b	05	E	1 " <u>One might be suspicious</u> "... overgeneralizes from circulation to models
Wunsch (2006)	90		4	e	c		05	E	1 " <u>taking place at the end of the Ice Age</u> ", bad edit, D-O events confused.
	90			e	c				2 " <u>impact</u> " ← "spacial extent", different meanings, confusion in edit.
	90			e	c				3 " <u>poleward circulation</u> " ← "poleward of 25 N": different meanings.
	91					b	21		4 General: Meme# 21 (D-O)? , #02 (natural), or #05 (models unreliable)
Totals (eE, mM, bB)			67	33	24	37	14	13	(E, M, B): (11, 6, 11)

W.11.5 Comments on Summaries

Via **W.11.8**, a reader may recognize obvious Errors, Changes in meaning and possible Bias. Most Summary text seems created by copying and lightly editing the originals. One might expect a few errors as people try to tighten wording, but WR often adds or lengthened words, sometimes introducing Issues. Many low-level copy-edits could be done by someone with zero knowledge of the subject. *Some Trivial Changes seem like those done by students to make it harder for a software plagiarism checker to find an unknown source⁷⁸, but that explanation also makes little sense. The sources are identified, so no effort is needed to find them, just the patience to look carefully, so the purpose of minor tweaks is not obvious.*

Perfect reports are hard to find, **but this part of the WR has pervasive problems in basic scholarship.** It has simple, obvious text-editing Errors. It sometimes Changes meanings, leaving the reader in doubt as to level of understanding. Biases are pervasive and occasionally very strong.

The 26 pages of Summaries contain 67 Issues, in addition to more elsewhere.. (Errors+Changes+Biases) totaled 94, so many Issues are combinations. The following discusses a selection, handling the 16 papers as a group, then Mann (1998) separately.

Minor Errors in Editing <e>

Minor errors <e> mostly seem like simple editing carelessness, mis-cites or odd wordings compared to the originals. Sometimes, as in Issues (2, 24, 34, 59, 64, 65, 66) the errors introduce relatively unimportant Changes of meaning <c>.

More Important Errors in Editing <E>

In 4 cases (19, 26, 31, 32, <E>) the WP fails to edit “we” to “they” or equivalent. Those demonstrate the cut-and-paste nature of the Summaries and might be expected to be caught by typical proofreading. These seem errors of competence and review, not purposeful.

In Issue (29, <E>) two large paragraphs of the MBH98 Summary are inappropriately re-used in the Mann, et al (2000) Summary.

⁷⁸ The simplest test is to quote an interesting sentence of two from the antecedent and use a search engine to find it, then quote the same passage from the WR that contains several TCs. Sometimes it find the antecedent, often it does not.

Issue (27 <ECB>) is a mis-edit that changes cooling
“following the 14th century” (1400s) to
“beginning in the 14th century” (1300s)

A century’s error might seem minor, but it changes the meaning, as much of the argument in MM03 and the WR focuses on 1400s’ reconstruction issues. Issue (27) plus Issue (44 <EB>) implicitly support Meme-56¹. Starting with the extreme high 1400s temperatures claimed in MM03, the rewording implies that the 1300s were even higher, **W.4.4**. This might be considered minor, but is yet another error that ignores Theme-G⁴.

Issue (33 <ECB>) loses “human” from greenhouse gas forcing. This might be just an edit error, but it changes the meaning substantially. Relevant forcing changes are well-established to be human-induced, but omitting this weakens the text. This might support Meme-02¹, Theme-B⁴, Theme-C⁴. The WR and Wegman’s testimony manage to avoid even once truly admitting strong human influence, **A.2**, even under repeated questioning.

Issue (53 <ECB>) converts a clear statement of MWP:
“geographic restrictions” to
“restricted analysis “
once again supporting Meme-56¹ and ignoring Theme-G⁴.

Other Changes of Meaning <cC>

Some are already discussed above in combinations.

Most of the minor Issues <c> may be simply poor wording, conversion of precise to ambiguous terms, carelessness or simple lack of understanding. Of these, 5 coded <ec> (2, 59, 63, 64, 65) seem like ambiguities or poor editing.

Issues (35, 50) emphasize changes in solar irradiance more than the originals. Both fit Meme-01², “It’s the sun,” most commonly used in papers by **Soon** and/or **Baliunas**.

Issue (35,<cB>) changes
“considerably less significant” to
“less significant“
a subtle change, but still a weakening.

Issue (50 <CB>) changes the meaning importantly and requires a purposeful insertion to convert:

“natural changes in radiative forcings” to
 “especially as a response to solar irradiance.”

Natural changes in forcing also include volcanic aerosols and natural changes in GHG, not just solar effects.

Issue (46 <CB>) is serious. While one might agree or disagree with the other comments from MM05a,

“adopting MBH98 results in the main IPCC promotional graphics”

is at least correct. The WR explicitly changes that to:

“the main endorsement of the IPCC.”

That is a clear introduction of Meme-a¹ and perhaps an example of Meme-d¹, already in use by Senator Inhofe by February 10, 2005:

www.marshall.org/pdf/materials/300.pdf, p.10

“Senator Inhofe. ... “global warming is the greatest single hoax ever perpetrated on the American people. The Senator will present four short speeches questioning the **four pillars** on which the alarmist view of climate change is based: the 2001 National Academy of Sciences report, **the IPCC’s reliance on Michael Mann’s discredited “hockey stick” model**, the Arctic climate impact assessment report, and the flawed data produced by climate models.”

Biases <bB>

The Issues coded all subtly weaken results of MBH, IPCC or mainstream climate science or strengthen criticisms of them. Some quote-mine legitimate arguments or over-emphasize them. Any of these could be perfectly accidental, but the overall pattern is clear. An impartial report might have errors, but one would expect them to be mixed. Originally, the report had a category for Biases in the opposite direction, but it turned out to be empty. Maybe I missed some.

Issues (3, 22, 41;) inserts “attempt” to describe actions of MBH,

“attempt to estimate”
 “attempt to reconstruct”
 “attempted to reconstruct.”

Clearly, MBH could not actually do anything but only “attempt to do.”

In Issue (47,), the WR says:

“Out of the 70 sites in the network, 93% of the variance in the MBH98 PC1 is accounted for by only 15 bristlecone and foxtail pine sites, all with data collected by one man, Donald Graybill.”

Was that fact important for Congress to know? Does it matter? It seems like an attempt to weaken credibility of this data, without the expertise to do so.

Issue (54) changes

“limited ” to
 “flawed,”

I searched various physical and on-line dictionaries, but none listed these words as synonyms for each other. This is clearly purposeful, but more picayune than meaningful.

In Issue (36 <CB>), the WR has:

“recent anthropogenic activities are contributing to the recent warming.”

That is certainly true, is rather weaker in context compared to the general discussion of Mann, et al (2000), Theme-H¹.

In Issue (37<CB>), Mann, Jones (2003) write:

“The reconstruction is consistent with previous reconstructions (and model simulations [e.g., Mann, 2002])”

and the WR changes that to:

The reconstruction is consistent with previous Mann reconstructions.

Mann, Jones list 17 different reconstructions by various combinations of authors. The WR omits that, then confuses model simulations with reconstructions, not equivalent in this context. A reader might easily think that Mann, Jones was consistent only with Mann’s previous work.

In Issue (52 <CB>), the WR has:

“While all these studies supported the claim”

This turned a strong statement about recent measured temperatures into a weaker “claim” about “studies.” See the side-by-side in context.

Mann (1999)

This is handled separately, since it is a long dissertation, not a paper like the 16 others. Although its SS% (94% vs 91%) is near-average for Mann-led papers, its ID% is much lower than average (42% vs 63%). More effort seems applied to explicit rewording here than in the papers. This is

doubly strange, as the writer must have spent more effort to do the rewording, but the dissertation is really irrelevant.

Then, 13 Issues are found in 2 Summary pages.

Picayune ones are (3, 5, 11, 13, 14, 15), i.e.,
 “attempt”
 “contends” (4 cases)
 “defend.”

Issue (8, <ECB>) is a serious mis-statement that converts a strength of Mann’s modeling into weaker-sounding caveat.

Issue (10, <EC>) is the kind of change made by someone who simply does not understand the topic. The WR changes:

“dampen the strengthening or weakening of
 the meridional overturning circulation before it has a chance to grow to
 large scale.”

to

dampen
 the meridional overturning circulation before it can become
 large-scale.

The WR often seems to over- emphasize natural fluctuations, but this is silly. It actually reverses the meaning. Dampening swings in either direction (Mann’s result) differs strongly from dampening the meridional overturning circulation, whose scale is already worldwide, which surely must be considered large-scale. For example, dampening variability of the Gulf Stream and shutting it down are rather different ideas, with very different consequences. One makes swings milder, the other would cool Europe somewhat, if not as much as some have claimed.

The WR chooses verbs to describe MM’s work:

“assess, found errors, claimed, accounted for the major errors, reconstructed, were able to accurately reproduce, prepared the data with improved quality control, concluded.” (MM03)
 “further detail their critique, respond, published, indicate, found, used, addresses the MBH98 claims, flatly contradicts the language used in MBH98, concluded, note the limited due diligence” (MM05)
 “in their critique, note several errors, discuss the incorrect usage, ran, evaluated, indicate.” (MM05b)

The mix of verbs for Mann and MBH differs somewhat:

“**Attempts to clarify**, includes, states, attributes, concludes, determines, **opts**, attributes, goes on to examine, **eventually settles, contends**, found, found, proposes, takes, studies, notes, introduces, **creates what he believes is, estimates, contends, contends, contends, defends.**” Mann (1998)
 “**In an attempt to understand**, use, attempt, calibrated, isolated, find” (MBH98)
 “**attempt to reconstruct**” (MBH99)

MM “reconstructed,,” whereas MBH only “attempted to reconstruct” or “attempt” to do other things. Of course, the MBH papers have the highest fractions of direct cut-and-paste, but changes indicate a clear viewpoint. Reasonable people could differ about many of these in isolation, but the overall pattern is clear, especially in context of antecedent text. It is difficult to reconcile this with [SAI2007, p.6]:

“Our approach was to serve as an honest broker and we made every attempt to approach the issue with an unbiased perspective.”

W.11.6 Strange inclusions in Summaries

The Mann (1998) PhD could plausibly be a reference, but does not seem very relevant and hardly seems worth Summarizing. WR, p.26 says:

“It is also interesting to note that Mann’s dissertation focused on 70 to 100 year climate signal variability, yet his future work does not have a similar component. His subsequent papers focus heavily on tree ring measurements, which provide data on a decadal or yearly scale. In later work, he also makes no mention of the ocean circulation variables, which he describes in his thesis as being integral to the variation in climate. If this type of forcing is a natural variable, it makes the conclusions about atmospheric forcings seem incomplete.”

Ocean oscillations are generally not considered forcings and longer scales certainly make sense for ocean studies, Theme-E. In any case, paleoclimate people continuously argue about proxy combinations. Relevance of this point seems tenuous.

Wunsch (2002, 2006) also have questionable relevance. Wunsch is a credible, well-cited researcher with clear views, as expressed in response to his mis-use by the creators of The Great Global Warming Swindle”:
www.realclimate.org/index.php/archives/2007/03/swindled-carl-wunsch-responds

However, these papers are outside the relevant web of citations. They neither reference nor are referenced by any of the other Important Papers. The WR only mentions Wunsch in one sentence, in an irrelevant and confused paragraph on p.50:

“The latter fact implies that the oceans are absorbing tremendous amounts of heat, which is much more alarming because of the coupling of ocean circulation to the atmosphere. (See Wunsch 2002, 2006).”

Wunsch (2006) seems an especially odd inclusion, as it deals with Dansgaard-Oeschger or D-O events (abrupt warmings) and ongoing arguments about possible Gulf Stream shutdown.. But D-O events have not occurred during the last ~10,000 years, putting them rather far outside MBH99’s study of the last millennium. The WR shows evidence of basic confusion about all this, with 3 <ec> Issues.

The WR quotes Wunsch expressing skepticism on models, but often gets confused among the many kinds of models. In any case, reconstructions (as per MBH99) are not climate models, although the latter are sometimes used as cross-checks. Via quote-mining or confusion, both Wunsch papers might be used to support Meme-05🟡. Wunsch (2006) might support Meme-21🔴 or Meme-02🔴. They may fit Theme-E4. However, McIntyre had written about Wunsch, so maybe these originated there.

The WR seems confused on this, which makes it difficult to understand why these are even references, much less Important ones. If people write scathing attacks on the research practices of another scientific discipline, exhibiting serious confusion over basics is not a good start.

W.11.7 Strange or trivial changes

The yellow-highlighted TC phrases include rewordings, but sometimes introduce errors. Some just seem change for the sake of change, not even towards consistency or conciseness. Most are embedded in large sections of **ID** text, so someone took explicit effort to make these edits. In the examples here (*) marks those where the antecedent term is sometimes used elsewhere in WR, but changed anyway, so not for consistency. Perhaps different people wrote different sections.

Northern Hemisphere	←	NH*
Medieval Warming Period	←	MWP*
ice cores (corals)	←	ice core (coral)
CE	←	AD*
datasets	←	data sets*
First, second...	←	(1), (2), ...
1), 2), ...	←	unnumbered sentences
1), 2), ...	←	(1), (2), ...
1., 2, ...	←	(a), (b), ...
from 1730 to 1980	←	1730-1980
from 1902-1993	←	1902-93
.59	←	0.59 (or other numbers)

W.11.8 Side-by-Side Comparisons

●#12 “Summary of “Are Multiproxy Climate Reconstructions Robust? By Gerd Bürger and Ulrich Cubasch (2005)” [p.87, 53]
coast.gkss.de/staff/storch/pdf/b%FCrger.cubasch.grl.2005.pdf

WR, p.87, Paragraph 1

Bürger and Cubasch review the robustness of multiproxy climate reconstructions, especially with regard to Mann et al. 1998 (MBH98), a prominent and widely discussed paper on the topic.

The MBH98 reconstruction applies an inverse regression between a set of multiproxies on one hand and the dominant temperature principal components (PCs) on the other. The sparse availability of proxies prior to 1450 is accounted for by estimating the regression for seven successive time periods.

Bürger and Cubasch skip this last step in their approximation of the MBH98 settings.

In this study, they use the proxies available for the period 1400-1450 (which includes 18 tree ring and ice core proxies).

Using the 1902-1980 as the calibration period, an empirical model is fitted and applied to the full proxy record. Before estimating the regression model, the proxies undergo a PC transformation,

a measure against collinearity, which can inflate the model error.

WR, p.87, Paragraph 2, 1st Part

Bürger and Cubasch found 64 variants of reconstructed millennial Northern Hemisphere temperatures.

The spread about the MBH98 is immense, especially around the years 1450, 1650, and 1850.

There is no evidence that one variation should be chosen over the others and even

the variant with the best reduction of error statistic (79%) is the variant that most strongly deviates from MBH98.

1.<e>“the 1902-1980 as...” Careless editing.

Burger and Cubasch (2005), p.1

The climate reconstruction employed by MBH98 applies an inverse regression (see below) between a set of multiproxies on the one hand and the dominant temperature principal components (PCs) on the other.

The decreasing availability of proxy data back in time is accounted for by estimating the regression for seven successive time periods.

For reasons of simplicity we skip this latter step in our study, and approximate the MBH98 setting as follow...

We use the proxies that are available in the time period 1400-1450 (18 single dendro and ice core proxies...

... grid points for the 1902-1980 calibration ... phase. From these data an empirical model is fitted, and then applied to the full proxy record. Before estimating the regression model, the proxy predictors undergo a PC transformation

Burger and Cubasch (2005), p.2

This is a useful measure against collinearity, a complication that inflates the model error...

Figure 1 shows the 64 variants of reconstructed millennial NHT as simulated by the regression flavors.

Their spread about MBH is immense, especially around the years 1450, 1650, and 1850.

No a priori, purely theoretical argument allows us to select one out of the 64 as being the “true” reconstruction.

...the best variant, with an RE of 79% ..., is, strangely, the variant that most strongly deviates from MBH.

Words: (209, 166, 125, 79%, 60%), Issues: 1.

“Summary of “Are Multiproxy Climate Reconstructions Robust? By Gerd Bürger and Ulrich Cubasch (2005)” [p.87, 53]

coast.gkss.de/staff/storch/pdf/b%FCrger.cubasch.grl.2005.pdf

WR, p.87, Paragraph 2, 2nd Part

When the setting was moved to AD 1600 instead of 1400, the spread is still quite large in the early part of the reconstruction, even though more proxies are available.

WR, p.87, Paragraph 3

Fundamental to all proxy inferences is the assumption that the variations in the proxy are related to the temperature and somewhat uniformly.

However, the results of this study do not give such a relationship, at least not one that is robust.

Bürger and Cubasch could not find one criterion solely responsible for the spread of variants, but it is possible that a significant source of uncertainty could be the scale mismatch between the full millennial and the calibrating proxy variations. In that case, the regression model leaves its general domain of validity and is applied in an extrapolative manner.

The further the estimated regression laws are extrapolated the less robust the method is. This error growth is especially critical for parameter-insensitive, multi-proxy climate field reconstructions of the MBH98 type. In order to salvage such a method, there must be a mathematical derivation of the model error and more sophisticated regularization schemes that can minimize the error.

Burger and Cubasch (2005), p.3.

... setting of the AD 1600 step where more proxies (57) are available... The spread is particularly large in the earliest part of the simulations...

Burger and Cubasch (2005), p.4.

Fundamental to all dendrochronological inferences on climate is the following principle of uniformitarianism...

The principle obviously generalizes to the broader context of multiproxies, but evidently our results do not give such a relationship, at least not one that is sufficiently robust.

... with none of the criteria being solely accountable for the spread. This uncertainty persists even among the best performing variants, and we believe that we were able to trace it back to a scale mismatch between the full millennial and the calibrating proxy variations. Under such circumstances, the regression model leaves its generic domain of validity and is applied in an extrapolative manner.

Burger and Cubasch (2005), p.5.

The more one leaves that scale and the farther the estimated regression laws are extrapolated the less robust the method is. The described error growth is particularly critical for parameter-intensive, multi-proxy climate field reconstructions of the MBH98 type.... To salvage such methods, two things are required: First, a sound mathematical derivation of the model error and, second, perhaps more sophisticated regularization schemes that can keep this error small.

Words: (186, 171, 108, 92%, 58%).

Words: (395, 337, 228, 85%, 59%), Issues: 1.

Ⓞ#23 “Summary of Low-Frequency Signals in Long Tree-Ring Chronologies for Reconstructing Past Temperature Variability by Jan Esper, Edward Cook, and Fritz Schweingruber (2005 (sic))” [pp.86, 54] vs www.wsl.ch/staff/jan.esper/publications/Science_2002.pdf *The 2005 is a mis-cite.*
www.wsl.ch/staff/jan.esper/publications/QSR_Esper_2005.pdf is the 2005 paper.

WR, p.85, Paragraph 1

In this article Esper et al. address the debate revolving around the reliability of tree-ring records as substantial basis of temperature reconstruction before the 17th century.

The authors’ present analysis of centuries-long ring-width trends in 1205 radial tree-ring series from 14 high-elevation and middle-to-high latitude sites distributed over a large part of the Northern Hemisphere extratropics.

Esper et al. looked at growth trends in tree ring proxies by analyzing individual raw ring-width measurements using Regional Curve

Standardization (RCS) methods.

Successful use of the RCS method usually requires a large number of ring-width series because the method of detrending is not based on any explicit curve fitting to the individual series, but rather over series of a similar region.

However, the series are further broken down into two groups, those that age linearly and those with age trends that are non-linear.

WR, p.85, Paragraph 2, 1st Part

In each of these groups, the smoothed regional curves were estimated from the averaged biological age-aligned data.

The resulting tree ring indices were then averaged into linear and non-linear mean value functions to produce two nearly independent tree-ring chronologies covering the years 800-1990.

Esper et al (2002), p.2250

Critics argue that tree-ring records, the substantial basis of the MBH reconstruction before the 17th century, cannot preserve long-term, multicentennial temperature trends.

We present the analysis of centuries-long ring-width trends in 1205 radial tree-ring series from 14 high-elevation and middle-to-high latitude sites distributed over a large part of the NH extratropics.

...we analyzed the individual raw ring-width measurements using

**Esper et al (2002), p.2251
RCS.**

Successful use of the RCS method generally requires a large number of ring-width series because the method of detrending is not based on any explicit curve-fitting to the individual series as described above. Rather, a single mean biological growth curve, estimated from all the data, is used.

...classifying them into two groups: one with age trends that have a weakly “linear” form (443 series) and one with age trends that are more “nonlinear” (762 series)

Two smoothed RCs were estimated from the averaged biological age-aligned data in the linear and nonlinear groups.

The resulting tree-ring indices were then averaged into linear and nonlinear mean value functions to produce two nearly independent tree-ring chronologies covering the years 800–1990...

Words: (183, 169, 122, 93%, 67%).

“Summary of Low-Frequency Signals in Long Tree-Ring Chronologies for Reconstructing Past Temperature Variability by Jan Esper, Edward Cook, and Fritz Schweingruber (2005)” [pp.86, 54] vs www.wsl.ch/staff/jan.esper/publications/Science_2002.pdf *The 2005 is a mis-cite.* www.wsl.ch/staff/jan.esper/publications/QSR_Esper_2005.pdf *is the 2005 paper. This one is the 2002 paper.*

WR, p.85, Paragraph 2, 2nd Part

Each of these chronologies showed evidence of above average temperatures during the Medieval Warming Period (900-1300), below average temperatures during the Little Ice Age (1200-1850), and large-scale warming after 1850, consistent with instrumental temperature records.

Overall, these results demonstrate that properly selected and processed tree-ring records can preserve such long time-scale climate variability.

WR, p.85, Paragraph 3

Additionally, using RCS methods, *climate variability of the Medieval Warming Period (MWP)* can be reconstructed, and it approaches the magnitude of 20th-century warming in the Northern Hemisphere up to 1990.

Consistent with other analyses of the MWP, it appears to be more temporally variable than the warming trend of the past century. Analysis also supports that the warmest period of the MWP may have begun in the early 900s, with the warmest interval being from 950 to 1045 AD. This finding suggests that past comparisons of the MWP with the 20th-century warming may not have included all of the MWP, especially its warmest period.

1. <ec> “*climate variability*” is very odd wording for “average temperature.”

Esper et al (2002), p.2251

Each shows evidence for inferred above average temperatures during the MWP (900–1300),... below-average temperatures over much of the 1200–1850 interval... Since the year 1850, large-scale warming in the NH extratropics is indicated, in agreement with instrumental temperature records.

Overall, the broad coherency and rich multicentennial variability found in the linear and nonlinear chronologies demonstrate that properly selected and processed tree-ring records can preserve such long time-scale climate variability.

Esper et al (2002), p.2252

evidence for a large-scale MWP (*sensu lato*) has been reconstructed, and it approaches the magnitude of 20th-century warming in the NH up to 1990.

Consistent with other analyses of the MWP ... appears to be more temporally variable than the warming trend of the last century. Our analysis also indicates that the MWP in NH extratropics may have begun in the early 900s... The warmest period covers the interval 950–1045... This finding suggests that past comparisons of the MWP with the 20th-century warming back to the year 1000 (19, 22) have not included all of the MWP and, perhaps, not even its warmest interval.

Theme-F₁: *last paragraph of the paper explains plausible reasons for differences with MBH. That does not get mentioned.*
 Theme-G₁: *this paper says their work shows differences in worldwide timing of MWP. The WR even quotes that, but it does not seem to register.*
 Theme-H₁: *“...large-scale MWP (sensu lato) ...,” p.2252.*
 Theme-J₁: *Figure 3 instructively compares uncertainty bands.*

Words: (157, 146, 117, 93%, 73%), Issues: 1. Theme-F₁, Theme-G₁, Theme-H₁Theme-J₁

Words: (340, 316, 233, 93%, 70%), Issues: 1.

#36 “Summary of Michael E. Mann's Ph.D. Dissertation, *A Study of Ocean-Atmosphere Interaction and Low-Frequency Variability of the Climate System* (1998)” [pp.67-68, 55]. (This is not generally available on-line, I asked Dr. Mann for a copy.)

WR, p.67, Paragraph 1

Dr. Mann attempts to clarify low frequency climate variations so that the effects of other factors (anthropogenic forcing, etc.) on climate can be defined.

Dr. Mann's method includes a simplified theoretical model to provide a description of the effects of ocean circulation on climate as well as

application of multivariate statistical methodology in the reconstruction of oscillatory low-frequency signals, using proxy and instrumental data.

While Dr. Mann states that there is scant robust evidence for periodic climate signals other than the yearly seasonal variations, he notes there does seem to be an oscillatory character in many climatic processes.

Dr. Mann attributes this “quasi-oscillatory” climate signal to linear and non-linear feedback mechanisms. Additionally, these signals are marked by defined, finite, decadal to centennial scale variations.

Dr. Mann concludes, from the investigation of proxy data, that the most likely source of the climate variations is the coupled ocean-atmosphere processes.

1. Attempts. Gratuitous weakening, seen elsewhere.

Mann (1998), p.1

These low-frequency modes or “signals” may compound the detection of anthropogenic climate forcing...

Mann (1998), p.5

...a simplified approach to theoretical modeling that seeks to provide a zonally-averaged but basin-resolved description of the coupled meridional overturning/gyre scale circulation of the world oceans and the simple dynamical feedbacks of the overlying atmosphere... application of the multivariate statistical MTM-SVD methodology to the detection, reconstruction, and subsequent intercomparison of oscillatory low-frequency spatiotemporal signals in both empirical (both instrumental and proxy) and model climate data.

Mann (1998), p.2

Other than the obvious seasonal cycle, there is scant robust evidence for truly periodic climate signals... Many climatic processes nonetheless appear to exhibit some oscillatory character, describing spatially-coherent climatic variations which tend to oscillate between different states owing to a variety of possible linear or non-linear feedback mechanisms. Such “quasi-oscillatory” signals, as we term them, are marked by a well defined dominant timescale of variation, but also by finite, somewhat episodic spells of oscillatory behavior....

Such behavior can be associated with underlying coupled ocean-atmosphere dynamics,...

The investigation of longer-term proxy data appears to establish some robustness of interdecadal ... and century-scale ... climate signals

Words: (147, 147, 51, 100%, 35%), Issues: 1.

#36 “Summary of Michael E. Mann’s Ph.D. Dissertation, *A Study of Ocean-Atmosphere Interaction and Low-Frequency Variability of the Climate System (1998)*” [pp.67-68, 55].

WR, p.67, Paragraph 2

In his analysis, Dr. Mann determines that **the traditional approach of climate signal detection**, *univariate spectral analysis*, *underestimates multi-decadal variations*.

He opts for a multivariate method, **principal component analysis**

combined with **spectral analysis**, for climate signal detection.

However, this method also presents with problems, as the distinct principal components present different climate signals and varying decomposition of the associated noise.

Dr. Mann attributes this to **two consecutive statistical operations** with **confounding optimization properties**.

Dr. Mann goes on to examine **several different methods of principal component analysis** that mitigate these **negative effects**, but eventually settles on multitaper frequency-domain singular value decomposition, or “**MTM-SVD**.”

WR, p.67, Paragraph 3

MTM-SVD isolates statistically significant oscillations that are correlated with the **independent time series**.

This method is useful **in describing spatially-correlated oscillatory signals** that have **arbitrary spatial relationships in**

amplitude and phase. Subsequently, **this approach can detect standing and traveling patterns in a spatial-temporal dataset**

as well as **account for periodic or aperiodic oscillatory patterns**.

Dr. Mann **contends** that this method **allows for an accurate climate reconstruction of spatiotemporal patterns immersed in noise**.

2. “*confounding*” ← “*conflicting*,” not quite the same.
3. “*contends*,” minor but negative connotations, seen elsewhere. “*spatial-temporal*” ← “*spatiotemporal*” trivial change, odd

Mann (1998), p.20

First, however, we review **the traditional approaches to climate signal detection**.

A common approach to spatiotemporal signal detection in geophysical applications is based on some variant of **Principal component analysis (PCA)** in which a Singular Value Decomposition (SVD) is performed on the data matrix followed by **spectral analysis**...

Mann (1998), p.30

The primary weakness results from the performance of **two consecutive statistical operations** which have conflicting **optimality properties**....

Nonetheless, **a variety of generalizations of PCA** have been developed which attempt **to ameliorate** several of the **problems** noted above...

Below we discuss such methods, ... those particular weaknesses or limitations which are overcome by the **MTM-SVD** approach...

Mann (1998), p.35

The **MTM-SVD** approach seeks to **isolate statistically significant narrowband oscillations**, ... that are correlated among a large number of **independent series**...

...it naturally **describes spatially-correlated oscillatory signals with arbitrary spatial relationships in both**

Mann (1998) p.36

amplitude and phase. In this manner, **the approach can faithfully detect either standing and traveling oscillatory patterns in a spatiotemporal dataset**....Moreover, because the methodology **allows for** the detection of either **periodic or aperiodic irregular oscillatory patterns**,...

Mann (1998) p.53

The methodology **allows for a faithful reconstruction of the arbitrary spatiotemporal patterns of narrowband signals immersed in spatially-correlated noise**.

Words: (178, 149, 60, 84%, 34%), Issues: 2.

“Summary of Michael E. Mann's Ph.D. Dissertation, *A Study of Ocean-Atmosphere Interaction and Low-Frequency Variability of the Climate System (1998)*” [pp.67-68, 55].

WR, p.67, Paragraph 4

Using these methods, Dr. Mann found a long-term global warming trend and anomalous atmospheric circulation patterns. These patterns show similarity to a modeled response of climate to increased greenhouse gases. Additionally, Dr. Mann found significant internal 50-100 year oscillations with similar features occurring over several centuries. Similar oscillatory signals have been attributed to variability in the thermohaline circulation and coupled ocean-atmosphere processes in other model simulations. Dr. Mann also found a distinct 10-15 year oscillation in the instrumental data.

This evidence of several interannual climate signals makes the interpretation of data more complicated in terms of a simple linear dynamical mechanism.

WR, p.68, Paragraph 1

Next, Dr. Mann proposes an alternative method for modeling the ocean circulation variations with respect to climate. He takes the equations governing ocean circulation and subdivides them into two sections.

Dr. Mann studies these two dominant modes of circulation, the gyre and mean meridional overturning circulations, separately as well as dynamically coupled.

He notes that this type of modeling is only relevant when the nonuniform effects for each section are properly taken into account.

4. <e> “10-15 year“ ← “16-18 year” seems odd. Maybe other source? sections” ← “components” is slightly odd word change.

Mann (1998) p.130

The dominant mode of secular variation during the last century is a long-term global warming trend associated with some anomalous atmospheric circulation patterns that show similarity to the modeled response of the climate to increased greenhouse gases...

The analysis of multiple centuries of proxy data suggests that true robust 50-100 year internal oscillations with similar features over several centuries. Similar oscillatory signals were attributed to variability in the thermohaline circulation and possible coupled ocean-atmosphere processes in recent model simulation studies. An interdecadal 16-18 year climate signal is clearly evident in the instrumental climatic fields analyzed,...

Mann (1998) p.131

The frequency modulation of the inter-decadal signal evident in these longer-term data seems complicate the interpretation in terms of any simple linear dynamical mechanism.

Mann (1998) p.132

there is merit to exploring an alternative approach to modeling the ocean circulation.

It is conceptually useful to divide the subdivide the ocean circulation into two components... (divide the subdivide (sic))

Mann (1998) p.133

... the gyre and mean meridional overturning circulations of the ocean.

These two dominant modes of circulation

are coupled dynamically as well... Thus, it is important to take a consistent modeling approach to combining the influences of the meridional overturning and gyre-scale ocean circulations.

Words: (175, 153, 75, 87%, 43%), Issues: 1.

“Summary of Michael E. Mann's Ph.D. Dissertation, *A Study of Ocean-Atmosphere Interaction and Low-Frequency Variability of the Climate System (1998)*” [pp.67-68, 55].

WR, p.68, Paragraph 1 (cont)

After deriving these two components, Dr. Mann introduces some approximations and simplifications to allow for their coupling.

From this, Dr. Mann creates what he believes to be a reasonably faithful description of large-scale ocean circulation, temperature and salinity fields of the world's oceans.

However, he notes one caveat, that this model is very sensitive to dynamics created when the gyre circulation is not taken into account.

When these gyre-scale processes are absent from the model, a 200-300 year mode of ocean variability is clearly defined, taken by Dr. Mann to be the linear mode of the variability in the meridional overturning circulation.

Dr. Mann estimates the effects of the ocean circulation on the atmosphere by parameterizing the modeled response of the atmosphere to sea surface temperature variations.

5. “what he believes to be” weakening.

The extra ocean is a Trivial Change of zero value in that sentence.

6.. <ECB> “this model...” the WR has conjoined widely-separated paragraphs of Mann (1998) and completely changed to meaning to weaken the credibility of Mann's modeling. Mann says that gyre effects dampen swings, so his model includes (parameterized) gyre effects, without which modeled variability is higher than actually seen. In effect, a strength of the approach now sounds like a problem. A related issue appears on the next page.

Mann (1998) p.146

We now introduce a number of approximations and simplifications ...components derived ..., and allowing for the coupling of the two components.

Mann (1998) p.198

...a reasonably faithful description of the large-scale circulation, temperature, and salinity fields of the world oceans as modeled by more complex OGCM models ...the variability of the coupled ocean-atmosphere system can be quite sensitive to the richer dynamics that are impossible with gyre contributions.

Mann (1998) p.261

When gyre-scale processes are absent, a 200-300 year timescale mode of ocean variability is isolated that we interpret as a damped linear mode of variability in the meridional overturning circulation of the Atlantic basin which is excited by stochastic forcing. The passive response of the atmosphere as estimated by a simple parameterization of the response of the overlying atmosphere to SST variations...

Words: (127, 127, 54, 100%, 43%), Issues: 2.

“Summary of Michael E. Mann's Ph.D. Dissertation, *A Study of Ocean-Atmosphere Interaction and Low-Frequency Variability of the Climate System (1998)*” [pp.67-68, 55].

WR, p.68, Paragraph 1 (cont)

When gyre-scale processes are accounted for a 70-to-100 year instability is present.

Dr. Mann interprets this variation as an oceanic delayed oscillator mechanism caused by changes in the meridional overturning, which subsequently causes

changes in the near-surface salinity and heat advection.

These effects in turn dampen the meridional overturning circulation before it can become large-scale.

Dr. Mann contends that the results of the model study underscore possible interactions between these two major circulation processes and the nature of decadal to century scale variability.

7. <ec> “Gyre-scale” ← “gyre-advective,” not a synonym. The trivial change lessened precision for no good reason.
8. <EC> “dampen the meridional” ← “dampen the strengthening...”
The WR phrase has a completely different meaning than Mann’s. Turning off the Gulf Stream would be an extreme dampening of the circulation. Mann is talking about the important dampening effects on the variability of the circulation. It is difficult to believe the writer of this summary was doing much more than selecting and rearranging words to create a false impression of expertise, while simply not understanding the major points.
9. “contends”
10. <ec> “decadal” ← “interdecadal” Trivial Change actually changes meaning, if in a minor way, for no obvious purpose.

Mann (1998) p.261

When gyre-advective processes are accounted for ... a 70-100 year instability is isolated, in addition to the lower frequency variability. This is interpreted as an oceanic delayed oscillator mechanism in which changes in the meridional overturning ocean circulation lead to

Mann (1998) p.262

changes in the near-surface salinity and heat advection...that oppose those due to the meridional overturning circulation, and dampen the strengthening or weakening of the meridional overturning circulation before it has a chance to grow to large scale.... Although this faster, gyre-advective mechanism is quite different from gyre-scale mechanisms... It nonetheless underscores the importance of possible interactions between gyre-scale and meridional overturning circulations in governing the nature of interdecadal-to-century scale variability.

*Decadal is 10-20 years, second page of Abstract.
Interdecadal is 15-30 years, first page of Abstract.*

Words: (84, 84, 48, 100%, 57%), Issues: 4.

“Summary of Michael E. Mann's Ph.D. Dissertation, *A Study of Ocean-Atmosphere Interaction and Low-Frequency Variability of the Climate System (1998)*” [pp.67-68, 55].

WR, p.68, Paragraph 2

Dr. Mann contends that his work shows strong evidence for the existence of 50-100 year scale oscillations centered in the North Atlantic, persistent over several centuries.

This is suggestive (but not conclusive) of damped oscillation in the climate system.

Dr. Mann also contends that an atmospheric response to both of these major circulation processes is identified, corresponding closely to sea level pressure variations.

Lastly, Dr. Mann defends the robustness of this simplified model, stating

that the observed climate variability is

consistent with many complex climatic mechanisms not included in this study.

However, with more long-term proxy data and more large-scale climate reconstructions,

the application of the signal detection methods described here will provide further insight into the nature of these decadal to century scale climate signals.

11., 12. “contends”

13. “defends” “here” seems merely odd.

The repeated use of “attempts,” “contends,” “defends,” often connote defense of a weak position. Does the writer of this Summary show the slightest competence of opinion on these topics?

Mann (1998) p.263

Both instrumental and proxy climate data also support the existence of a 50-100 year multidecadal or “century-scale” oscillations centered in the North Atlantic... The analyses of proxy data suggest the persistence of these signals over several centuries. The frequency-domain characteristics of these oscillatory signals are suggestive, but not conclusive evidence of, stochastically-excited damped oscillations in the climate system.

Mann (1998) p.264

A passive response of the atmosphere is identified, corresponding to a sea level pressure variation

It is apparent then that the paradigm of a climate that exhibits stochastically excited stable oscillatory modes of variation is useful for understanding at least some of the characteristics underlying observed interdecadal and century-scale climatic variability. It is also true that a variety of mechanisms, many of which are far more complex than those studied here, are also consistent with many of the salient features of observed patterns of climatic variability.

Mann (1998) p.265

With more expansive networks of long-term proxy climate data and large-scale climate reconstructions based on these data, a longer term dataset of observed climate variations may soon be available for analysis. The application of the signal detection and reconstruction methodologies described in this study to such long term climate reconstructions should provide important new details about the nature and patterns of decadal-to century timescale climate signals.

Words: (126, 126, 63, 100%, 50%), Issues: 3. Someone scanned a (mostly irrelevant) dissertation, then wrote issue-laden Summary.

Words: (833, 782, 351, 94%, 42%), Issues: 13. Theme-E. **Misplaced (since (n) not very relevant, very odd to devote Summary to this).**

#37 “Summary of Global-scale temperature Patterns and Climate Forcing Over the Past Six Centuries by Michael Mann, Raymond Bradley, and Malcolm Hughes (1998)” (MBH98) [pp.69-70, 55].vs www.elmhurst.edu/~richs/EC/FYS/Mannetal.OriginalPaper.pdf

WR, p.69, Paragraph 1

In an attempt to understand long-term global climate variation, Mann et al. use a widely distributed set of proxy and instrumental climate indicators to reconstruct global temperature patterns over the past 500 years.

Using this data, they attempt to estimate the relationship between global temperature changes and variations in volcanic aerosols, solar irradiance, and greenhouse gas concentrations.

WR, p.69, Paragraph 2

Starting here, same text is in Mann, et al (2000) Summary, except 3 words. The data consisted of a multiproxy network. In this case proxy is a time series constructed using data from various sources, such as tree ring measurements, ice cores, ice melts, and historical records.

Overall the network includes 112 proxies, and each series has been formatted into annual mean anomalies relative to the reference period used for this data, 1902-1980.

Certain tree-ring datasets have been represented by a small number of leading principal components.

The dendroclimatic data has also been carefully reviewed to ensure standardization and sizeable segment lengths.

Although the data network covers large portions of the globe, there is only enough reliable information to conduct a spatial analysis of the Northern Hemisphere.

1. “attempt” is a weakening: MBH did estimate.
2. <e> “formatted” ← “formed” seems odd word choice from poor editing.
3. “only enough” rewording seems to have a weaker connotation.

MBH98, p.779

... studies have sought ... to understand long-term climate variations, by analysing a widely distributed set of proxy and instrumental climate indicators to yield insights into long-term global climate variations... we analyse the spatiotemporal patterns of climate change over the past 500 years, and then take an empirical approach to estimating the relationship between global temperature changes, variations in volcanic aerosols, solar irradiance and greenhouse-gas concentrations...

We use a multiproxy network consisting of widely distributed high-quality annual-resolution proxy climate indicators...The network includes (Fig. 1a) the collection of annual resolution dendroclimatic, ice core, ice melt, and long historical records...

The long instrumental records have been formed into annual mean anomalies relative to the 1902–80 reference period...

Certain densely sampled regional dendroclimatic data sets have been represented in the network by a smaller number of leading principal components...

However, the dendroclimatic data used were carefully screened for conservative standardization and sizeable segment lengths. Although there are notable spatial gaps, this network covers significant enough portions of the globe to form reliable estimates of Northern Hemisphere mean temperature,

Words: (170, 161, 88, 95%, 52%), Issues: 3.

“Summary of Global-scale temperature Patterns and Climate Forcing Over the Past Six Centuries by Michael Mann, Raymond Bradley, and Malcolm Hughes (1998) (MBH98)” [pp.69-70, 55]vs www.elmhurst.edu/~richs/EC/FYS/Mannetal.OriginalPaper.pdf

WR, p.69, Paragraph 3

Because of the heterogeneity of the information available, Mann et al. calibrated the datasets by first decomposing the 20th century instrumental data into its dominant patterns of variability using principal component analysis, and subsequently calibrate the individual climate proxy indicators against the time histories of these distinct patterns during their mutual interval of overlap.

Included in this calibration approach are three assumptions:

- 1) the indicators in our network are linearly related to one or more of the instrumental training patterns,
- 2) a relatively sparse, but widely distributed sampling of long proxy and instrumental records may measure the small number of degrees of freedom in climate patterns at interannual and longer timescales, and
- 3) patterns of variability captured by the multiproxy network have analogs in the patterns they find in the shorter instrumental data.

In their principal component analysis (PCA), Mann et al isolated a small number of dominant patterns of variability, otherwise labeled ‘empirical eigenvectors’.

Each of these patterns or eigenvectors has a characteristic spatial pattern and a pattern evolving over time (also referred to as the ‘principal component’). These eigenvectors are ranked according to the percentage of variance they describe.

The first five eigenvectors describe 93% of the total variance.

Each of the indicators in this study was calibrated using these five eigenvectors.

4. <E> “our” was not edited, MGB00 and MBH98 Summaries now diverge.

MBH98, p.780

... because of the inhomogeneity of the information represented by...

We first decompose the twentieth-century instrumental data into its dominant patterns of variability, and subsequently calibrate the individual climate proxy indicators against the time histories of these distinct patterns during their mutual interval of overlap.

Implicit in our approach are at least three fundamental assumptions.

- (1) The indicators in our multiproxy training network are linearly related to one or more of the instrumental training patterns...
- (2) A relatively sparse but widely distributed sampling of long proxy and instrumental records may nonetheless sample most of the relatively small number of degrees of freedom in climate patterns at interannual and longer timescales. ...

(3) Patterns of variability captured by the multiproxy network have analogues in the patterns we resolve in the shorter instrumental data. We isolate the dominant patterns of the instrumental surface temperature data through principal component analysis (PCA)—PCA provides a natural smoothing of the temperature field in terms of a small number of dominant patterns of variability or ‘empirical eigenvectors’.

MBH98, p.781

Each of these eigenvectors is associated with a characteristic spatial pattern or ‘empirical orthogonal function’ (EOF) and its characteristic evolution in time or ‘principal component’ (PC).

The ranking of the eigenvectors orders the fraction of variance they describe in the (standardized) multivariate data during the calibration period. The first five of these eigenvectors describe a fraction $b = 0.93$ (that is, 93%) of the global-mean (GLB) temperature variations,...

We calibrate each of the indicators in the multiproxy data network against these empirical eigenvectors at annual-mean resolution during the 1902–80 training interval.

Words: (214, 208, 156, 97%, 73%), Issues: 1.

“Summary of Global-scale temperature Patterns and Climate Forcing Over the Past Six Centuries by Michael Mann, Raymond Bradley, and Malcolm Hughes (1998)” (MBH98) [pp.69-70, 55] vs www.elmhurst.edu/~richs/EC/FYS/Mannetal.OriginalPaper.pdf

WR, p.69, Paragraph 3

The temperature reconstructions derived using all indicators and the most optimal eigenvector subsets show long term trends including pronounced cold periods during the mid-seventeenth and mid nineteenth centuries and warmer intervals during the mid-sixteenth and late eighteenth centuries.

Based on their methods, almost all of the years before the twentieth century exhibit temperatures well below the twentieth century mean temperature.

Taking into account the uncertainties in their reconstruction, they find that the years 1990, 1995, and

1997

Each show anomalies that are greater than any other year back to 1400, with roughly a 99.7% level of certainty.

WR, p.70, Paragraph 1

Other general circulation and energy-balance model experiments, including some statistical comparisons of twentieth century global temperatures with forcing series, suggest that although both solar and greenhouse gas forcings play some role in explaining twentieth century climate trends, greenhouse gases appear to play an increasingly dominant role during this century.

Additionally, it is hoped that as larger numbers of high quality proxy reconstructions become available it may be possible to create a more globally representative multiproxy data network for further study.

5. *<c>* “In their (NH) reconstruction”: they sometimes seem to lose the “NH” qualifier. It may be just carelessness here, but the NH/SH/global differentiation does actually matter. Theme-F₄.

MBH98, p.783

The reconstructions discussed here are derived using all indicators available, and using the optimal eigenvector subsets pronounced cold periods during the mid-seventeenth and nineteenth centuries, and somewhat warmer intervals during the mid-sixteenth and late eighteenth centuries,

with almost all years before the twentieth century well below the twentieth-century climatological mean....

MBH98, p. 784

Taking into account the uncertainties in our NH reconstruction (see Methods), it appears that the years 1990, 1995 and

MBH98, p.785

now 1997 (this value recently calculated and not shown)

each show anomalies that are greater than any other year back to 1400 at 3 standard errors, or roughly a 99.7% level of certainty.

A variety of general circulation and energy-balance model experiments as well as statistical comparisons of twentieth century global temperatures with forcing series, suggest that, although both solar and greenhouse-gas forcings play some role in explaining twentieth-century climate trends, greenhouse gases appear to play an increasingly dominant role during this century....

As larger numbers of high-quality proxy reconstructions become available in diverse regions of the globe, it may be possible to assimilate a more globally representative multiproxy data network.

Note loss of “somewhat”. This was a slight weakening, but not counted as , given that one would expect summaries to trim words.

Theme-J₆: confidence intervals matter. This and MBH99 were influential because they were early in computing confidence intervals.

Words: (177, 177, 145, 100%, 82%), Issues: 1. Theme-F₄, Theme-H₄, Theme-J₄

Words: (561, 546, 389, 97%, 69%), Issues: 5.

④ #38 “Summary of Northern Hemisphere Temperatures During the Past Millennium: Inferences, Uncertainties, and Limitations Mann et al. (1999)” (MBH99) [pp.71, 56] vs www.ltrr.arizona.edu/webhome/aprilc/data/my%20stuff/MBH1999.pdf , www.ncdc.noaa.gov/paleo/pubs/mann_99.html

WR, p.71, Paragraph 1, 1st part

Estimates of climate variability during past centuries rely upon indirect “proxy” indicators – natural archives that record past climate variations: tree rings, sediments, ice cores and corals.

MBH98 used these temperature proxies to reconstruct yearly global surface temperature patterns back to CE 1400.

In this article, Mann et al. attempt to reconstruct global surface temperature patterns prior to 1400 because it is surmised that temperatures were warmer even before the period reconstructed in MBH98.

However, in order to recreate these temperature patterns, the same methodology employed in MBH98 was applied to an even sparser proxy data network available prior to CE 1400.

Only 12 viable indicators are available for this time period.

Because only a small number of indicators are available in regions where the primary pattern of hemispheric mean temperature variation has significant amplitude, these indicators have a particularly important role.

Just as in MBH98, the calibration procedure for these 12 indicators invokes two assumptions: first, that a linear relationship exists between proxy climate indicators and some combination of large scale temperature patterns and second, that patterns of surface temperature in the past can be suitably described in terms of some linear combinations of the dominant present-day surface temperature patterns.

The calibration/verification statistics for reconstructions based on the 12 indicators are somewhat degraded compared to those for the post CE 1400 period.

The explained variance in the MBH98 data (post-1400 AD) was between 42% and 51%, whereas the explained variance among these 12 indicators is between 34% and 39%.

1. <e>The 2nd and 3rd sentences are almost repeats, poor editing.
2. “attempt” is weakening
3. <e> “warmer even before” is odd English, likely bad editing.

MBH99, p.759

Estimates of climate variability during past centuries must rely upon indirect “proxy” indicators - natural archives that record past climate variations...tree rings, varved sediments, ice cores and corals....

MBH98 reconstructed yearly global surface temperature patterns back in time...to AD 1400. .

It has been speculated that temperatures were warmer even further back, 1000 years ago...

We here apply the methodology detailed by MBH98 to the sparser proxy data network available prior to AD 1400...

Before AD 1400, only 12 indicators ...are available.

MBH99, p.760

...only a small number of indicators are available in regions where the primary pattern of hemispheric mean temperature variation has significant amplitude... These few indicators thus take on a particularly important role.

Just as in MBH98, the calibration procedure for these 12 indicators invokes two assumptions: first, that a linear relationship exists between proxy climate indicators and some combination of large-scale temperature patterns and second, that patterns of surface temperature in the past can be suitably described in terms of some linear combinations of the dominant present-day surface temperature patterns.

The calibration/verification statistics for reconstructions based on the 12 indicators are somewhat degraded compared to those for the post CE 1400 period...

The calibration and verification resolved variance (39% and 34% respectively) are consistent with each other, but lower than for reconstructions back to AD 1400 (42% and 51% respectively){see MBH98).

CE ← AD twice, WR uses AD 11 times. Google “sparser proxy data network available prior to AD 1400” gets many hits. Change to CE, only hits WR.

Words: (249, 222, 168, 89%, 67%), Issues: 4. 4. <ec> “The explained variance...” misstates the antecedent.

“Summary of Northern Hemisphere Temperatures During the Past Millennium: Inferences, Uncertainties, and Limitations Mann et al. (1999)” (MBH99) [pp.71, 56] vs www.ltrr.arizona.edu/webhome/aprilc/data/my%20stuff/MBH1999.pdf , www.ncdc.noaa.gov/paleo/pubs/mann_99.html

WR, p.71, Paragraph 1, 2nd part, Paragraph 2
 Furthermore, the first principal component of the ITRDB (International Tree Ring Data Bank) data in this analysis is the only one of these series that exhibits a significant correlation with time history of the dominant temperature pattern of the 1902-1980-calibration period.
 If this indicator is omitted, positive calibration/variance scores cannot be obtained for the Northern Hemisphere (NH) series.
 Thus, ITRDB PC1 is the most meaningful component i resolving hemispheric temperature trends. The assumption that this relationship is consistent with time requires closer study and as such a more widespread network of proxy indicators will be required for more confident inferences.
 The reconstructed NH series indicates a cooling period prior to industrialization, possibly driven by astronomical forcing, which is thought to have driven long term temperatures down since the mid-Holocene period.
 In addition, significant long-term climate variability may be associated with solar irradiance variations.
 Our reconstruction supports the notion that warmer hemispheric conditions took place early in the millennium followed by a long period of cooling beginning in the 14th century, which can be viewed as the initial onset of the Little Ice Age.
 However, even the warmer intervals in our reconstruction pale in comparison with modern (mid-to-late 20th century) temperatures. The data still upholds the conclusion that the 1990s was likely the hottest decade and that 1998 was likely the hottest year of the millennium. However, without more widespread high-resolution data, further conclusions cannot be drawn in regard to the spatial and temporal details of climate change in the past millennium and beyond.
 5. <e> “is consistent with time” is strange wording. 6. <E> “Our” unchanged.
 7. <ECb> “beginning in the 14th century” ← “following the 14th century”

MBH99, p.761
 It is furthermore found that only one of these series {PC #1 of the ITRDB data} exhibits a significant correlation with the time history of the dominant temperature pattern of the 1902-1980 calibration period. Positive calibration/variance scores for the NH series cannot be obtained if this indicator is removed from the network of 12 ...
 Though, as discussed earlier, ITRDB PC#1 represents a vital region for resolving hemispheric temperature trends, the assumption that this relationship holds up over time nonetheless demands circumspection. Clearly, a more widespread network of quality millennial proxy climate indicators will be required for more confident inferences.
 The reconstructed NH series and estimated uncertainties are shown... associated with a long-term cooling trend in the NH series prior to industrialization... is possibly related to astronomical forcing, which is thought to have driven long-term temperatures downward since the mid-Holocene ...
 In addition, significant century-scale variability may be associated with solar irradiance variations...
 Our reconstruction thus supports the notion of relatively warm hemispheric conditions earlier in the millennium, while cooling following the 14th century could be viewed as the initial onset of the Little Ice Age....
MBH99, p.762
 Even the warmer intervals in our reconstruction pale, however, in comparison with modern (mid-to-late 20th century) temperatures...
 ...the 1990s are likely the warmest decade, and 1998 the warmest year, in at least a millennium.
 More widespread high-resolution data which can resolve millennial-scale variability are needed before more confident conclusions can be reached with regard to the spatial and temporal details of climate change in the past millennium and beyond...

Words: (253, 253, 165, 100%, 65%), Issues: 4. 8. “further...drawn” ← “more confident”. Weak. Theme-F₄, Theme-H₄, Theme-J₄, as in MBH98.
 Words: (502, 502, 333, 100%, 66%), Issues: 8. Issue 7 gives subtle support to Meme-56₀..

#41 “Summary of Global Temperature Patterns in Past Centuries: An Interactive Presentation by Michael Mann, Ed Gille, Raymond Bradley et al. (2000)” [pp.72-73, 56] vs www.ncdc.noaa.gov/paleo/ei/eint_vol4_0004_1_29_2.pdf

WR, p.72, Paragraph 1

This paper expands on earlier work by the same authors (Global-scale temperature Patterns and Climate Forcing Over the Past Six Centuries by Mann et al.), which used multiproxy networks of indirect climate variability indicators, such as tree-ring measurements, ice cores, coral growth, etc., to reconstruct climate variability and temperatures over the past centuries.

In subsequent papers, Mann et al. expanded the reconstruction to Northern Hemisphere (NH) temperature variations over the past millennium, to examine ENSO-scale patterns of climate variability during past centuries, to compare observed patterns of climate variability in the Atlantic, and to assess the relationship between global patterns of climate variation and particular regional patterns.

Most recently, Mann et al. have made available for the first time seasonally resolved versions of global temperature surface patterns in an interactive format, allowing users to select specific spatial regions or time periods of interest. Details of the data and methods involved are discussed in Mann et al. (1998) and (1999).

WR, p.72, Paragraph 2

Note: starting here, several paragraphs are ~identical to MBH98 Summary.

The data consisted of a multiproxy network.

In this case proxy is a time series constructed using data from various sources, such as tree ring measurements, ice cores, ice melts, and historical records.

Overall the network includes 112 proxies, and each series has been formatted into annual mean anomalies relative to the reference period used for this data, 1902-1980.

Certain tree-ring datasets have been represented by a small number of leading principal components.

1. <E> Copying the MBH98 summary is rather careless.
2. <e> “formatted” ← “formed” is odd wording.

Mann, et al (2000), p.1-2

The text at left seems like a normal summary.

These reconstructions have since been extended to estimate Northern Hemisphere (NH) temperature variations over the past millennium... to examine ENSO-scale patterns of climate variability during past centuries ... to compare observed patterns of variability in the Atlantic ... and to assess the relationship between global patterns of climate variation and particular regional patterns...

For the first time seasonally resolved versions of the proxy-reconstructed surface temperature patterns are presented, ...

The reader is enabled to interactively examine spatial as well as temporal details (and their uncertainties) of yearly temperatures back in time for both annual-mean and seasonal windows.

(Source shifts, copied from MBH98 Summary, start p.69, Paragraph 2)

MBH98, p.779

We use a multiproxy network ...

The network includes (Fig. 1a) the collection of annual resolution dendroclimatic, ice core, ice melt, and long historical records...

The long instrumental records have been formed into annual mean anomalies relative to the 1902–80 reference period...

Certain densely sampled regional dendroclimatic data sets have been represented in the network by a smaller number of leading principal components...

Words: (232, 135, 83, 58%, 36%). Issues: 2

“Summary of Global Temperature Patterns in Past Centuries: An Interactive Presentation by Michael Mann, Ed Gille, Raymond Bradley et al. (2000)” (pp.72-73, 56] vs www.ncdc.noaa.gov/paleo/ei/eint_vol4_0004_1_29_2.pdf

§, p.72, Paragraph 2 (cont)

The dendroclimatic data has also been carefully reviewed to ensure standardization and sizeable segment lengths.

Although the data network cover large portions of the globe, there is only enough reliable information to conduct a spatial analysis of the Northern Hemisphere.

WR, p.72, Paragraph 3

Because of the heterogeneity of the information available, Mann et al. calibrated the datasets by first decomposing the 20th century instrumental data into its dominant patterns of variability using principal component analysis, and subsequently calibrate the individual climate proxy indicators against the time histories of these distinct patterns during their mutual interval of overlap.

Included in this calibration approach are three assumptions:

- 1) the indicators in our network are linearly related to one or more of the instrumental training patterns,
- 2) a relatively sparse, but widely distributed sampling of long proxy and instrumental records may measure the small number of degrees of freedom in climate patterns at interannual and longer timescales, and

3. <E> “our” was not edited out here or in the MBH98 Summary.

MBH98, p.779

However, the dendroclimatic data used were carefully screened for conservative standardization and sizeable segment lengths.

Although there are notable spatial gaps, this network covers significant enough portions of the globe to form reliable estimates of Northern Hemisphere mean temperature,

MBH98, p.780

... because of the inhomogeneity of the information represented by...

We first decompose the twentieth-century instrumental data into its dominant patterns of variability, and subsequently calibrate the individual climate proxy indicators against the time histories of these distinct patterns during their mutual interval of overlap.

Implicit in our approach are at least three fundamental assumptions.

- (1) The indicators in our multiproxy trainee network are linearly related to one or more of the instrumental training patterns...
- (2) A relatively sparse but widely distributed sampling of long proxy and instrumental records may nonetheless sample most of the relatively small number of degrees of freedom in climate patterns at interannual and longer timescales. ...

Words: (152, 146, 111, 96%, 73%), Issues:1.

“Summary of Global Temperature Patterns in Past Centuries: An Interactive Presentation by Michael Mann, Ed Gille, Raymond Bradley et al. (2000)” (pp.72-73, 56] vs www.ncdc.noaa.gov/paleo/ei/eint_vol4_0004_1_29_2.pdf

WR, p.72, Paragraph 3 (cont)

3) patterns of variability captured by the multiproxy network have analogues in the patterns we resolve in the shorter instrumental data. In their principal component analysis (PCA), Mann et al isolated a small number of dominant patterns of variability, otherwise labeled ‘empirical eigenvectors’.

At this point, Mann, et al (2000) and MBH98 Summaries diverge.

What could have happened here?

The corresponding text of MBH98 Summary was:

3) patterns of variability captured by the multiproxy network have analogs in the patterns they find in the shorter instrumental data

4. <E> WR used the same text for the MBH98 Summary, where it belonged, and here, where it did not. It seems that it was copied both places, but then someone edited the MBH98 Summary.

“analogs” ← “analogues” (“analogues” is fine, but shows origin)

“they find” ← “we resolve” (Error)

But forgot to edit them here, leaving “we resolve”

MBH98, p.780

(3) Patterns of variability captured by the multiproxy network have analogues in the patterns we resolve in the shorter instrumental data. We isolate the dominant patterns of the instrumental surface temperature data through principal component analysis (PCA). PCA provides a natural smoothing of the temperature field in terms of a small number of dominant patterns of variability or ‘empirical eigenvectors’.

Note: At this point, the source returns to Mann, et al (2000).

Words: (43, 43, 35, 100%, 81%), Issues: 1.

“Summary of Global Temperature Patterns in Past Centuries: An Interactive Presentation by Michael Mann, Ed Gille, Raymond Bradley et al. (2000)” (pp.72-73, 56] vs www.ncdc.noaa.gov/paleo/ei/eint_vol4_0004_1_29_2.pdf

WR, p.73, Paragraph 1

The most recent temperature reconstructions indicate that 1998 (as opposed to 1990, 1995 and 1997 as previously proposed in Mann et al. 1998, 1999) was most likely the warmest year of at least the past millennium.

There are also distinct temperature trends for the Northern and Southern hemispheres.

While both hemispheres have similar trends, the coldness of the 19th century appears to be somewhat more pronounced in the Northern hemisphere.

Additionally, evidence suggests that the post-1850 warming was more dramatic at higher latitudes relative to lower latitudes due to larger positive feedbacks at high latitudes. The annual mean temperature trends at higher latitudes are seen to be greater than the hemispheric trends themselves. In contrast, the tropical band shows less change than the entire Northern Hemisphere series.

WR, p.73, Paragraph 2

Mann et al. also provide yearly global temperature maps for annual mean, boreal cold season, and warm season for the reconstructed temperature fields from 1730 to 1980, the raw temperature data from 1902-1993 (used for calibration) and the sparse raw “verification” data from 1854 to 1901 (used for cross-validation).

Users can investigate spatial patterns and time histories of this global temperature data at

www.ngdc.noaa.gov/cgi-bin/paleo/mannplot2.pl.

Mann, et al (2000), p.10

... that the warmth of the 1990s (three years in particular: 1990, 1995, 1997) was unprecedented in at least the past 600 years ... Based on the most recent extensions of this reconstruction (Mann et al., 1999), it furthermore now appears that 1998 was likely to have been the warmest year of at least the past millennium.

Mann, et al (2000), p.12

While the two hemispheres (NH and SH series) show similar temperature trends during the past few centuries (Figure 8), the coldness of the nineteenth century appears to be somewhat more pronounced for the Northern Hemisphere.

It is also instructive to examine the trends in different latitude bands.

Overpeck et al. (Overpeck et al., 1997) suggested that post-1850 warming was more dramatic at high northern latitudes relative to lower latitudes due to larger positive feedbacks at high latitudes.

The annual-mean temperature trends at high latitudes are seen (Figure 9) to be greater than the hemispheric trends themselves.

In contrast, the tropical (30S–30N) band shows less change than the entire Northern Hemisphere series.

Mann, et al (2000), p.15-16

Yearly global temperature maps for annual mean, boreal cold season, and warm season are available below for the reconstructed temperature fields (1730–1980), the raw temperature data (1902–93) used for calibration, and the sparse raw “verification” temperature data (1854–1901) used for cross-validation.

To investigate the spatial patterns and time histories of the global temperature reconstructions, readers can begin by visiting the following site online:

www.ngdc.noaa.gov/cgi-bin/paleo/mannplot2.pl

Words: (191,169, 134, 88%, 70%).

“Summary of *Global Temperature Patterns in Past Centuries: An Interactive Presentation* by Michael Mann, Ed Gille, Raymond Bradley et al. (2000)” (pp.72-73, 56] vs www.ncdc.noaa.gov/paleo/ei/eint_vol4_0004_1_29_2.pdf

WR, p.73, Paragraph 3

The statistical relationship between variations in the NH mean temperature and estimates of the histories of solar, greenhouse gas and volcanic forcings suggest that while the natural forcings play a role, only greenhouse gas forcing alone can explain the unusual warmth of the past few decades.

Mann et al. also examined the sensitivity surrounding these forcings and found that when physically reasonable lags are incorporated into the attribution analysis there is evidence of even greater statistical relationships with particular forcings.

At the physical lag of one year, the relationship between temperature variations and volcanic forcing is slightly more consistent.

At the physical lag of 10-15 years the relationship between greenhouse gas increases and increasing temperatures is considerably more significant, while the relationship with solar irradiation is less significant.

Thus, there is significant evidence that recent anthropogenic activities are contributing to the recent warming.

WR, p.73, Paragraph 4

It is clear that the primary limitations of large-scale proxy-based reconstruction in past centuries, both temporally and spatially, reside in the increasingly sparse nature of available proxy networks available to provide reliable, past climate information.

Arduous efforts are needed to extend such networks in space and time to the point where significant improvements will be possible in order to gain a more

empirical understanding of climate variations during the past millennium.

5. <ECB> “only greenhouse” ← “only human greenhouse”.
6. <ec> “irradiation” ← “irradiance,” Trivial Change yields error
7. <cB> “less significant” ← “considerably less significant” Meme-01

Mann, et al (2000), p.22

The statistical relationship between variations in NH mean temperature and estimates of the histories (see Mann et al., 1998) of solar, greenhouse gas, and volcanic forcings ..., only human greenhouse gas forcing alone, as noted by Mann et al. (Mann et al., 1998), can statistically explain the unusual warmth of the past few decades.

Mann, et al (2000), p.24-25

From the above analysis it is clear that when physically reasonable lags are incorporated into the attribution analysis, there is evidence of even greater statistical relationships with particular forcings.

At the physically expected lag of 1 yr, the relationship between temperature variations and volcanic forcing is slightly more consistent ...

For lags of 10–15 yr the relationship between greenhouse gas (GHG) increases in recent decades and increasing temperatures is considerably more significant, while the relationship with solar irradiance is considerably less significant.

Mann, et al (2000), p.27

It is clear that the primary limitations of large-scale proxy-based reconstruction in past centuries, both temporally and spatially, reside in the increasingly sparse nature of available proxy networks available to provide reliable climate information back in time.

Only through the arduous efforts of large numbers of paleoclimate researchers can such networks be extended in space and time to the point where significant improvements will be possible in proxy-based reconstruction of the global climate. Such improvements will lead to further advances in our

empirical understanding of climate variations during the past millennium.

Read this page carefully. The WP weakens clear statements of anthropogenic effect, and weakened the clear limitation of solar irradiance changes. Changing “irradiation” ← “irradiance” is ... very odd terminology.

Words: (214, 187, 161, 87%, 75%), **Issues:** 4. 8.<cB> “contributing to recent warming” is Weak as well. MBH had a clear statement: “only human.” **Words:** (832, 680, 524, 82%, 63%), **issues:** 8. Theme-F₄, Theme-H₄, Theme-J₄ as in MBH99.

#42 Summary of Global Surface Temperature over the Past Two Millennia by Michael Mann and Philip Jones (2003) [pp.77, 56] vs www.meteo.psu.edu/~mann/shared/articles/mannjones03.pdf

<p>WR, p.77, Paragraph 1 Mann and Jones present their reconstructions of Northern and Southern Hemisphere mean surface temperature over the past two millennia based on high-resolution (annually or decadal scaled) proxies.</p> <p>For the Northern Hemisphere, they use previous temperature reconstructions from eight distinct regions based on 23 individual proxy records</p> <p>and for the Southern Hemisphere, they use temperature reconstructions from five distinct regions.</p> <p>Composites were performed separately for each hemisphere, based on available regional temperature records. Each regional temperature record was standardized by removal of the long-term mean and division by the standard deviation, after decadal smoothing.</p> <p>The composites were weighted combinations of the standardized proxy series, weighted by size of region and estimated reliability of the climate signal in the proxy. Proxy records exhibiting negative or approximately zero local correlations were eliminated from the study.</p> <p>Each composite was also standardized to have the same mean and decadal standard deviation as the target instrumental series over the period of common overlap.</p> <p><i>Note: "climate" ← "temperature" converts a precise term to an ambiguous, one, as proxies may contain other climate signals, like rainfall. Temperature is a climate signal, so this is not counted as an error. It certainly does seem like change for the sake of change.</i></p>	<p>Mann and Jones (2003), CM-1 We present reconstructions of Northern and Southern Hemisphere mean surface temperature over the past two millennia based on high-resolution 'proxy' temperature data which retain millennial-scale variability.</p> <p>For the Northern Hemisphere (NH), we make use of temperature reconstructions from 8 distinct regions (based on 23 individual proxy records).</p> <p>Mann and Jones (2003), CM-2 Fewer long series are available for the Southern Hemisphere (SH), where we make use of temperature reconstructions from 5 distinct regions.</p> <p>Composites were performed separately for both hemispheres, based on the available regional temperature records... Each regional temperature record was standardized by removal of the long-term mean and division by the standard deviation after decadal smoothing...</p> <p>Composite series were formed from weighted combinations of the individual standardized proxy series, employing weights on the individual records that account for the size of the region sampled, and the estimated reliability of the temperature signal...</p> <p>Proxy records exhibiting negative or approximately zero local correlations (SH record #2 and #3) were eliminated from further consideration in the study.</p> <p>The hemispheric and global composites were standardized to have the same mean and (decadal) standard deviation as the target instrumental hemispheric mean series over the period of common overlap (1856–1980).</p>
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Words: (157, 157, 132, 100%, 84%).

Summary of Global Surface Temperature over the Past Two Millennia by Michael Mann and Philip Jones (2003) [pp.77, 56] vs www.meteo.psu.edu/~mann/shared/articles/mannjones03.pdf

WR, p.77, Paragraph 2

The Northern Hemisphere reconstruction was observed to be largely insensitive to elimination of shorter proxy records or to the weighting of the proxy series, suggesting a significant robustness.

The reconstruction is consistent with previous Mann reconstructions,

in that the warmth in the late 20th century is unprecedented.

Larger uncertainties in the Southern Hemisphere reconstruction preclude a similar conclusion for this.

Increased quality and quantity of Southern Hemisphere proxy records are needed to decrease the current uncertainties

surrounding the reconstruction and definitively make conclusions about the climate variability.

1. “previous Mann reconstructions” has 2 issues, as it changes the meaning from the clear discussion of Section 1. The only Mann reference was to model simulations, and that important context disappeared. See right.
2. “needed to decrease” ← “should help to decrease”. Weaker. The Mann/Jones paper is fairly clear about the gradation of uncertainty, and precise that the last sentence applies to SH.
3. “definitely” ← “less definite”. Weak.

Mann and Jones (2003) CLM 5-3

The NH reconstruction (Figure 2a) is observed to be largely insensitive to the elimination of the shorter proxy records, or to the precise nature of the weighting of records, suggesting that the result shown back to AD-200 is fairly robust.

The reconstruction is consistent with previous reconstructions (and model simulations [e.g., Mann, 2002]) of NH mean temperatures over the past millennium within estimated uncertainties...

This warmth is, however, dwarfed by late 20th century warmth which is observed to be unprecedented...

Mann and Jones (2003) CLM 5-4

Larger uncertainties in the SH reconstruction (Figure 2b) preclude such a conclusion for the SH series and suggest a similar, but less definitive conclusion, for the global mean (average of NH and SH; Figure 2c).

A greater number of high-quality Southern Hemisphere proxy records should help decrease the uncertainties.....

The sparseness of the available proxy data in the Southern Hemisphere lead to less definitive conclusions for the SH or global mean temperature at present.

A cursory scan of Section 1 of this paper lists 17 different reconstructions by various combinations of authors, of which 6 included Mann. That Section discussed similarities yielded by relatively different approaches “The reconstruction is consistent...” is found in Section 4, in context that ought obviously to refer to various reconstructions, not just previous ones by Mann. The result reads as though the sole responsibility for unprecedented 20th century warmth lies with Mann reconstructions, ignoring coauthors and 2/3 of the other papers that (mostly) agree within usual uncertainties.

Words: (87, 76, 50, 87%, 57%), Issues: 3. Words: (244, 233, 182, 95%, 75%), Issues:3. Theme-F₄, Theme-H₄, Theme-J₄

Words: (244, 233, 182, 95%, 75%), Issues:3. Theme-F₄, Theme-H₄, Theme-J₄

Ⓔ #43 “Summary of Testing the Fidelity of Methods Used in Proxy-Based Reconstructions of Past Climate by Michael Mann, Scott Rutherford, Eugene Wahl and Caspar Ammann (2005)” [pp.84-85, 56] vs www.meteo.psu.edu/~mann/shared/articles/MRWA-JClimate05.pdf

WR, p.84, Paragraph 1

In this article Mann et al. examine two prominent methods in historical climate reconstruction,

Climate Field Reconstruction (CFR) and the Composite-Plus Scale (CPS). The former combines several different proxy records in order to reconstruct underlying patterns of past climate change.

The latter combines many different proxy series (such as tree ring series, ice core series, etc.) and scales the resulting composite against a target series (i.e. the Northern Hemisphere) that is measured instrumentally.

In order to assess both methods, Mann et al. used climate simulation to create a known climate record.

They then layered the model with the typical noise associated with real-world uncertainties found in actual proxies.

Thus, Mann et al. created pseudo proxies that they

could use to test the two methods of climate reconstruction.

They constructed three distinct networks of pseudo proxies, each with attributes similar to actual proxy networks used in past CFR and CPS studies.

WR, p.84, Paragraph 2, 1st part

Following the standard CPS procedure, each pseudo proxy was smoothed by decade and standardized.

The weighted composite of these proxies was then scaled to have the same mean and standard deviation as the actual Northern Hemisphere series.

Using different levels of the signal-to-noise ratio (SNR) (relative amplitudes of noise variance), Mann et al. evaluated the effectiveness of each method.

In CPS experiments, the results most closely resembled those obtained from actual proxies for SNR=1.0.

The lower the SNR level (.25 and .5 were also measured), the lower the skill of reconstruction.

Mann et al (2005), p.4097

Two distinct types of methods have primarily been used to reconstruct past large-scale climate histories from proxy data.

One group, so-called climate field reconstruction (CFR) methods, assimilates proxy records into a

reconstruction of the underlying patterns of past climate change... composite-plus-scale (CPS) methods ... composites a number

of proxy series and scales the resulting composite against a target (e.g., Northern Hemisphere temperature) instrumental series...

Climate model simulations can, however, be used to provide...

Mann et al (2005), p.4098

We thus add a noise component, of appropriate amplitude, to represent the real-world uncertainties that exist in the relationship between proxies and regional climate.

We investigate here both the CFR and CPS approaches, using networks of synthetic pseudoproxy data ... constructed to have attributes similar to actual proxy networks used in past CFR and CPS studies...

Mann et al (2005), p.4100

Following the typical CPS procedure ... each pseudoproxy series was decadal smoothed ... and standardized.

A weighted composite ... was then scaled to have the same mean and decadal standard deviation as the actual NH series...

Mann et al (2005), p.4102

In the CPS experiments, results for SNR = 1.0 most closely resembled those obtained for actual proxy reconstructions...

...lower SNR values (i.e., 0.25) yielded significantly lower estimates of reconstruction skill...

Words: (241, 201, 86, 83%, 36%).

“Summary of Testing the Fidelity of Methods Used in Proxy-Based Reconstructions of Past Climate by Michael Mann, Scott Rutherford, Eugene Wahl and Caspar Ammann (2005)” [pp.84-85, 56] vs www.meteo.psu.edu/~mann/shared/articles/MRWA-JClimate05.pdf

WR, p.84, Paragraph 2, 2nd part

Additionally, when SNR=1.0, the CPS method was found to be relatively insensitive to the length of the calibration interval.

Mann et al. found that in general, CPS or regression based methods employing a short calibration period are likely to underestimate long-term variability.

WR, p.84, Paragraph 3

Mann et al.’s implementation of the CFR approach makes use of the regularized expectation maximization (RegEM), which is similar to Principal Component Analysis (PCA), but it employs estimates of data covariances in iterations.

Mann et al. tested three types of this method:

the straight application of RegEM, a “hybrid frequency-domain calibration” approach and a stepwise version of RegEM.

All three of these methods yielded similar results in the study.

Similar to CPS, Mann et al. found that when SNR=1.0, this method yielded a similar resolved variance and it was relatively insensitive to the calibration period.

However, this method yielded a moderately more skillful reconstruction with a long calibration period. Additionally, the CFR method appears to systematically underestimate the amplitude of the larger volcanic cooling events, most likely because of the small number of volcanic events present in the calibration interval.

WR, p.84-85, Paragraph 3

In general, Mann et al. found no evidence that real-world proxy-based temperature reconstructions are likely to suffer from any systematic underestimate of low-frequency variability.

Their findings also suggest that both of these standard methods, CPS and CFR, are likely to provide a faithful estimate of actual long-term hemispheric temperature histories, within estimated uncertainties.

Mann et al (2005), p.4102

For SNR = 1.0, the reconstructions (Fig. 3a) are observed to be relatively insensitive to whether the short ... or long ... calibration interval is use... ...CPS, or regression-based methods employing a short calibration period, are likely to underestimate long-term variability

Mann et al (2005), p.4100

Our implementation of the CFR approach makes use of the regularized expectation maximization (RegEM) method of Schneider. The method is similar to principal component analysis (PCA) ... but employs an iterative estimate of data covariances...

... we tested (i) straight application of RegEM, (ii) a “hybrid frequency-domain calibration” approach ... (iii) a “stepwise” version of RegEM...

Mann et al (2005), p.4105

As with the CPS experiments, SNR = 1.0 ... yielded a similar verification resolved variance ... As in the CPS experiments, there is a slight sensitivity to which ... calibration period is used, with a long calibration period ... yielding a moderately more skillful reconstruction...

The CFR approach does appear to systematically underestimate the amplitude of the larger volcanic cooling events... small number of moderate volcanic forcing events are contained within

Mann et al (2005), p.4106

the calibration interval... We find no evidence for the suggestion (e.g., VS04) that real-world proxy-based temperature reconstructions are likely to suffer from any systematic underestimate of low-frequency variability. Our findings suggest that both standard methods that have been used in proxy-based reconstruction (CPS and CFR) are likely to provide a faithful estimate of actual long-term hemispheric temperature histories, within estimated uncertainties.

Note: VS04 is the Von Storch, et al (2004) paper listed earlier.

Words: (235, 205, 138, 87%, 59%).

Words: (476, 406, 225, 85%, 47%).

● #44 “Summary of Corrections to the Mann et al. (1998) Proxy Database and Northern Hemispheric Average Temperature Series by Stephen McIntyre and Ross McKittrick (MM03) (2003)” [pp.75-76, 56] vs www.uoguelph.ca/~rmckitri/research/MM03.pdf

WR, p.75, Paragraph 1

In their paper, Corrections to the Mann et al. (1998) Proxy Database and Northern Hemispheric Average Temperature Series, (hereafter referred to as MM03), McIntyre and McKittrick assess the methodology and results of the widely referenced Mann, Bradley, and Hughes paper, Global Scale Temperature Patterns and Climate Forcing Over the Past Six Centuries (hereafter referred to as MBH98).

In MBH98 the authors attempted to reconstruct a temperature history of the Northern Hemisphere for the period 1400-1980. Their result

was a “hockey stick” shaped graph, from which they concluded that the temperatures of the late 20th century were unprecedented and that 1990-2000 was likely the hottest decade in the millennium, and 1998 was likely the hottest year in the millennium. These findings have been prominent in the discussion on global climate change and in subsequent policy discussions. MM03 attempts to recreate the research in MBH98 in order to prove or disprove their findings.

WR, p.75, Paragraph 2

In the course of the research reproduction, McIntyre and McKittrick found errors in the statistical methodology of MBH98.

Primarily, MM03 found that the creation of the proxy database itself held serious errors.

In this context proxy denotes one of the 112 physical measurements used that can serve as an indicator of climatic conditions, including temperature.

Examples of proxies include tree measurements, ice cores, and coral calcification rates.

The time series created from these measurements form the basis of the MBH98 study.

1. <c> MM03 mostly references MBH98, and this summary sometimes confuses it with MBH99 and the IPCC.

2. “attempted to construct” ← “constructed” Weakened.

Seems like reasonable summary.

MM03, p.751.

In a widely cited paper, Mann, Bradley and Hughes (1998, hereafter MBH98) constructed a temperature history of the Northern Hemisphere for the period 1400-1980. The result

MM03, p.752

was the well-known “hockey stick”-shaped graph suggesting that the climate of the late 20th century... (1999), who claimed that “temperatures in the latter half of the 20th century were unprecedented” ... “that the 1990s has been the warmest decade and 1998 the warmest year of the millennium”

Reasonable summary.

MM03, p.753

The term “proxy” denotes some physical data or measurement that can potentially serve as an indirect record of local temperature conditions, including tree ring widths and densities, coral d18O, d13C and calcification rates, ice core d18O, melt percentages...

Words: (231, 80, 50, 35%, 22%), Issues: 2.

Summary of Corrections to the Mann et al. (1998) Proxy Database and Northern Hemispheric Average Temperature Series by Stephen McIntyre and Ross McKittrick (MM03) (2003) [pp.75-76, 56] vs www.uoguelph.ca/~rmckitri/research/MM03.pdf

WR, p.75, Paragraph 3

MM03 claimed the following errors in the MBH98 proxy database:

1. unjustified truncation of three time series
2. copying 1980 values from one series onto another
3. displacement of 18 series to one year earlier than apparently intended
4. Statistically unjustified extrapolations or interpolations to cover missing entries in 19 series
5. geographical mislocations and missing identifiers of location
6. inconsistent use of seasonal temperature data where annual data is available
7. obsolete data in at least 24 series, some of which may have been obsolete at the time of the MBH98 study
8. listing of unused proxies
9. incorrect calculation on all 28 tree ring principal components.

WR, p.75, Paragraph 4

Having accounted for the major errors, MM03 reconstructed the temperature history.

Using the MBH98 methodology, they were able to accurately reproduce the “hockey stick” shaped graph in the MBH98 findings.

Still using the same basic methodology, MM03 prepared the data with improved quality control, including using the most recent data and collating it correctly.

The result was a northern hemisphere temperature reconstruction that takes on a different shape in which the temperature index peaks at

WR, p.76, (cont from previous page)

around 1450 AD, near the earliest measured point on the graph.

MM03 concluded that the errors in MBH98 make the data unreliable and obsolete such that it does not support their end conclusions.

Overall, this is more like a normal summary than most, but it confuses basics, like MBH98 vs MBH99.

3. “Having accounted for the major errors” Strong wording.

4 “data unreliable and obsolete” Strong wording

5. <EB> MM03 put 1450AD absurdly high.. See W.4.4. Meme-59.

MM03, p.753

- (a) unjustified truncation of 3 series;
- (b) copying 1980 values from one series onto other series, resulting...
- (c) displacement of 18 series to one year earlier than apparently intended;
- (d) unjustified extrapolations or interpolations to cover missing entries in 19 series;
- (e) geographical mislocations and missing identifiers of location;
- (f) inconsistent use of seasonal temperature data where annual data are available;
- (g) obsolete data in at least 24 series, some of which may have been already obsolete at the time of the MBH98 calculations;
- (h) listing of unused proxies;
- (i) incorrect calculation of all 28 tree ring principal components.

Note: seems like reasonable summary.

MM03, p.766

it was possible to prepare a data base with substantially improved quality control, by using the most recent data and collating it correctly.

landshape.org/public/FOIA/documents/MannHouseReply.pdf quotes

E&E Editor Sonja Boehmer-Christiansen:

*“I’m following my political agenda – a bit, anyway. *** But isn’t that the right of an editor? “As to “peer review,” Ms. Boehmer-Christiansen has acknowledged in an email to Dr. Tim Osborn of the Climatic Research Unit at the University of East Anglia (U.K.), that in her rush to get the McIntyre and McKittrick piece into print for political reasons Energy & Environment dispensed with what scientists consider peer review (“I was rushing you to get this paper out for policy impact reasons, e.g. publication well before COP9”). As Ms. Boehmer-Christiansen added, the “paper was amended until the very last moment. There was a trade off in favour of policy.””*

Words: (216, 126, 99, 58%, 46%), Issues: 3. Meme-56

(Words: (447, 206, 148, 46%, 33%), Issues: 5.

● #45 “Summary of The MM Critique of the MBH98 Northern Hemisphere Climate Index: Update and Implications by Stephen McIntyre and Ross McKittrick (MM05a) (2005a)” [pp.79-80, 56] www.uoguelph.ca/~rmckitri/research/MM.EE2005.pdf

WR, p.79, Paragraph 1

In an extension of their 2003 paper (Corrections to the Mann et. al. (1998) Proxy Database and Northern Hemispheric Average Temperature Series), McIntyre and McKittrick further detail their critique of Mann et. al. (1998) and respond to its subsequent update Mann et. al. (1999).

In response to McIntyre and McKittrick (2003), Mann et. al. published new information regarding their original research that MM03 attempted to replicate.

While the new information did not include the source code used to generate the original results, it did include an extensive archive of data and supplementary information on the methods at the University of Virginia FTP site.

WR, p.79, Paragraph 2

In their article, MM indicate that the individual data series (proxies) used to reconstruct the temperature index are important, and that errors *within these series do not get washed out in a multi-proxy study.*

Specifically, MM05a found that the differences in MBH98 and MM03 can be almost fully reconciled through the variations in handling of two distinct series, the Gaspé “northern treeline” series and the first principal component (PC1) from the North American proxy roster (NOAMER).

In MBH98, the first four years of both of these series were extrapolated. The extrapolation has the effect of depressing early 15th century results, and was not disclosed by Mann et al. until a later paper, Mann et al. (2004).

The underlying dataset that was subject to extrapolation also fails to meet the data quality standards described by Mann et al. elsewhere in the paper.

MM05a, p.70

In response to MM03 and subsequent submissions and correspondence to *Nature*, Mann et al. have provided new information about MBH98, including

an extensive archive of data and methods at the Supplementary Information ...an extensive archive of data and methods at a University of Virginia FTP site...

One of the points of view advocated in this article is that individual data series matter in the MBH98 results.

We disagree with the view that problems with individual series simply get washed out in a multiproxy study.

MM05a, p.71

Differences between MM03 results and MBH98 type results can be reconciled through variations in the handling of only two series, the Gaspé “northern treeline” series and the first principal component (PC1) from the North American proxy roster (NOAMER).

The only difference between the two series is the extrapolation of the first four years in MBH98... with the convenient result of depressing early 15th century results. This extrapolation was not disclosed in MBH98, although it is now acknowledged in the Corrigendum.

MM05a, p.72

The underlying dataset... fails the data quality standards Mann et. al. themselves listed elsewhere.

Words: (243, 195, 87, 80%, 36%).

“Summary of The MM Critique of the MBH98 Northern Hemisphere Climate Index: Update and Implications by Stephen McIntyre and Ross McKittrick (MM05a) (2005a)” [pp.79-80, 56] www.uoguelph.ca/~rmckitri/research/MM.EE2005.pdf

WR, p.79, Paragraph 3

In the MBH98 methodology, they used a principal component analysis, which they reported to be conventional or centered.

However, in further disclosure of information on the UVA FTP site, it has been determined that the principal component analysis was not actually centered.

In fact the mean used in their calculations is the 1902-1980 mean, but it was applied to the period 1400-1980.

The effect of de-centering the mean is a persistent “hockey stick” shaped PC1, even when layered with persistent red noise.

It follows from this shape that the climate of the late 20th century was unprecedented.

Because the original code is in FORTRAN, which takes much more programming to run statistical processes than modern software such as R, it is very possible that this is due to a programming error, although Mann et al. have not admitted to any such error.

WR, p.79, Paragraph 3

In the MBH98 de-centered principal component calculation, a group of twenty primarily bristlecone pine sites govern the first principal component.

Fourteen of these chronologies account for over 93% variance in the PC1 and 38% of the total variance.

The effect is that it omits the influence of the other 56 proxies in the network.

In a centered version of the data, the influence of the bristlecone pine drops to the fourth principal component, where it accounts for 8% of the total variance.

MM05a, p.72

MBH98 had stated that they used “conventional” principal components (PC) calculations. . . .

After the University of Virginia FTP site was made publicly available following MM03, by examining PC series archived there and, by examining source code for PC calculations, we were able to determine that MBH98 had not carried out a “conventional” PC calculation but had modified the PC algorithm, by, among other things, subtracting the 1902-1980 mean, rather than the 1400-1980 column mean. . . .

We have shown elsewhere . . . so efficient in mining a hockey stick shape that it nearly always produces a hockey-stick shaped PC1 even from persistent red noise

MM05a, p.74

. . . no advantage to the MBH98 approach of using hundreds of lines of Fortran text to carry out the above functions, thereby opening up the possibility of error, since it can be easily done in a few lines of high-level programming languages. . . . may have originated as a programming error, the Corrigendum did not admit any error. . . .

MM05a, p.75

In the MBH98 de-centered PC calculation, a small group of 20 primarily bristlecone pine sites . . . dominate the PC1.

Only 14 such chronologies account for over 93% of the variance in the PC1,

effectively omitting the influence of the other 56 proxies in the network. The PC1 in turn accounts for 38% of the total variance.

In a centered calculation on the same data, the influence of the bristlecone pines drops to the PC4 The PC4 in a centered calculation only accounts for only about 8% of the total variance

Words: (230, 133, 82, 58%, 36%).

“Summary of The MM Critique of the MBH98 Northern Hemisphere Climate Index: Update and Implications by Stephen McIntyre and Ross McKittrick (MM05a) (2005a)” [pp.79-80, 56] www.uoguelph.ca/~rmckitri/research/MM.EE2005.pdf

WR, p.79, Paragraph 3

The MM03 results are obtained if the first two NOAMER principal components are used.

The MBH98 results can be obtained if the NOAMER network is expanded to five principal components.

Subsequently, their conclusion about the climate of the late 20th century is contingent upon including low-order principal components that only account for 8% of the variance of one proxy roster. Furthermore, the MM03 results occur even in a de-

WR, p.80, Paragraph 3 (cont)

centered PC calculation, regardless of the presence of PC4, if the bristlecone pine sites are excluded.

WR, p.80, Paragraph 1

In the Gaspé “northern treeline” series, MM05a found that the MBH98 results occur under three conditions:

- 1) the series must be used as an individual proxy;
- 2) the series must contain the portion of the series that relies only on one or two trees for data; and
- 3) it must contain the ad-hoc extrapolation of the first four years of the chronology.

Under all other conditions, including using an archived version of the series without extrapolation, MM03 type results occur.

MM05a, p.75

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...

Hence their conclusion about the uniqueness of the late 20th century climate hinges on the inclusion of a low-order PC series that only accounts for 8 percent of the variance of one proxy roster. If de-

centered PC calculation is carried out (as in MBH98), then MM-type results still occur regardless of the presence or absence of the PC4 if the bristlecone pine sites are excluded...

MM05a, p.76

MBH-type results occur only if a duplicate version of the Gaspé series is used as an individual proxy and

the portion of the site chronology with 1–2 trees is used and if

the first four years of the chronology are extrapolated under an ad hoc procedure...

If representation is achieved by use of the updated version of the Sheenjek River series (which meets replication standards in the 15th century), then MM-type results occur.

Words: (165, 131, 73, 79%, 45%).

“Summary of The MM Critique of the MBH98 Northern Hemisphere Climate Index: Update and Implications by Stephen McIntyre and Ross McKittrick (MM05a) (2005a)” [pp.79-80, 56] www.uoguelph.ca/~rmckitri/research/MM.EE2005.pdf

WR, p.80, Paragraph 2 (cont)

MM05a also addresses the MBH98 claims of robustness in their findings.

The sensitivity of the 15th century results to slight variations in the data and method of two individual series show a fundamental instability of the results that

flatly contradicts the language used in MBH98 and in

Mann et al. (2000) where it states “...whether we use all data, exclude tree rings, or base a reconstruction only on tree rings, has no significant effect on the form of the reconstruction for the period in question...”

Additionally, MM05a notes much of

the specialist literature raises questions about these indicators and at the least these questions should be resolved before using these two series as

temperature proxies, much less as uniquely accurate stenographs of the world’s temperature history.

WR, p.80, Paragraph 3

In response to MM03, Mann et al. wrote several critiques that appeared in Nature magazine as letters and as separate articles.

The Mann et al. (2004) paper argued that the MM03 use of centered principal components calculations amounted to an “effective omission” of the 70 sites of the North American network.

However, the methodology used omits only one of the 22 series.

A calculation like this should be robust enough that it is relatively insensitive to the removal of one series.

Also, “effective omission” is more descriptive of the MBH98 de-centering method, which uses 14 bristlecone sites to account for over 99% of explained variance.

MM05a, p.78

The sensitivity of 15th century results to such slight variations of method and data show a fundamental instability in MBH98

results, related especially to the presence or absence of bristlecone pines and Gaspé cedars. This flatly contradicts some claims by Mann et al. about the robustness of MBH98 results....

Mann et al. [2000] as follows: Whether we use all data, exclude tree rings, or base a reconstruction only on tree rings, has no significant effect on the form of the reconstruction for the period in question.

MM05a, p.81

Instead the specialist literature only raises questions about each indicator which need to be resolved prior to using them as

MM05a, p.82

temperature proxies at all, let alone considering them as uniquely accurate stenographs of the world’s temperature history.

MM05a, p.89

Mann et al. [2004a, 2004b] argued that our use of centered principal components calculations amounted to an “effective omission” of the 70 sites of the North American network.

First, the North American PC1 is only one of 22 series in the AD1400 step.

A robust statistical method should be relatively insensitive to the presence or absence of one of 22 series... using the MBH98

decentered method, 14 bristlecone sites account for over 99% of the explained variance... more accurate to characterize MBH98 de-centering methods as “effectively omitting” the majority of tree ring sites.

Words: (220, 161, 87, 73%, 40%).

“Summary of The MM Critique of the MBH98 Northern Hemisphere Climate Index: Update and Implications by Stephen McIntyre and Ross McKittrick (MM05a) (2005a)” [pp.79-80, 56] www.uoguelph.ca/~rmckitri/research/MM.EE2005.pdf

WR, p.80, Paragraph 4

In another response, Mann et al. claim that the PC series are linear combinations of the proxies and as such cannot produce a trend that is not already present in the underlying data.

However, the effect of de-centering the mean in PC analysis is that it preferentially selects series with hockey-stick shapes and it is this over weighting that yields a pattern not representative of the underlying data.

Additionally, Mann et al. responded to the MM03 critique of the bristlecone pine,

which pointed out that the bristlecone pine had no established linear response to temperature and as such was not a reliable temperature indicator.

Mann et al. responded by stating that their indicators were linearly related to one or more instrumental training patterns, not local temperatures.

Thus, the use of the bristlecone pine series as a temperature indicator may not be valid.

The authors of MM05 concluded that the various errors and adverse calculations that were not disclosed exhibit the limitations of the peer review process.

They also note the limited due diligence of paleoclimate journal peer review and that it would have been prudent to have checked the MBH98 data and methods against original data before accepting the findings as the main endorsement of the Intergovernmental Panel on Climate Change.

1. “the limited due diligence...” the WR simply copies the sweeping MM assertion published in a poorly-regarded, journal, about a field in which the WR showed lack of expertise. Meme-b.

2.<CB> MBH98 as the main endorsement of the IPCC?

Even MM did not say that. Meme-a

Of course, Meme-18 is the main topic of MM05b.

MM05a, p.90

...Mann et al. [2004a, 2004b] argued that their PC series were simply linear combinations of the underlying proxies and that no pattern could be produced in the PC1 which was not in the underlying data.

...but it is evident that the de-centering process preferentially selects series with hockey-stick shapes and this over-weighting is what yields a pattern that is not representative of the underlying data.

... in our criticism of bristlecone pines as an arbiter of world climate, we pointed out (as above) that a linear response to temperature had not been established for these sites... In fact we specified (MBH98) that indicators should be “linearly related to one or more of the instrumental training patterns,” not local temperatures....”

In MBH98, there are a number of examples, where results adverse to their claims were not reported...

recognizing the limited due diligence of paleoclimate journal peer review, it would have been prudent for someone to have actually checked MBH98 data and methods against original data before adopting MBH98 results in the main IPCC promotional graphics.

The IPCC used the MBH99 graph, not the MBH98 graph, but to be fair, sometimes MM use MBH98 to mean MBH98 →MHB99, I think. But the confusion propagates.

MM actually do use quotes when quoting other papers. The “results in the main IPCC promotional graphics” phrase seems fair.

Words: (211, 131, 68, 62%, 32%), Issues: 1. Meme-18, Meme-a, Meme-b

Words: (1069, 751, 397, 70%, 37%), Issues: 1. Dubious Source (E&E –see comments on Summary of MM03.)

#46 “Summary of Hockey sticks, principal components, and spurious significance by Stephen McIntyre and Ross McKittrick (2005b)” (MM05b) [p.81, 56] climateaudit.files.wordpress.com/2009/12/mcintyre-gr1-2005.pdf

WR, p.81, Paragraph 1

In their critique of Global-scale temperature Patterns and Climate Forcing Over the Past Six Centuries (MBH98) by Mann et al., McIntyre and McKittrick (MM) note several errors in the methodology and subsequent conclusions made by Mann et al.

First, MM discuss the incorrect usage of principal component analysis (PCA) in MBH98.

A conventional PC algorithm centers the data by subtracting the column means of the underlying series.

For the 1400 to 1450 data series, the FORTRAN code contains an unusual data transformation prior to the PC calculation, which was never reported in print.

Each tree ring series was transformed by subtracting the 1902-1980 mean and then dividing by the 1902-1980 standard deviation and dividing again by the standard deviation of the residuals from fitting a linear trend in the 1902-1980 period.

For PCA, if the 1902-1980 mean is close to the 1400-1980 mean, then there will be very little impact from this linear transformation.

However, if the means differ, then the explained series variance is inflated.

Since PCA gives more weight to series that have more explained variance, the effect is preference for the ‘hockey stick’ shape seen in Mann et al.

This ‘hockey stick’ shape supports the conclusions that climatic conditions in the late twentieth century are anomalies.

MM05b, p.1

Opinion: reasonable summary.

A conventional PC algorithm centers the data by subtracting the column means of the underlying series.

Instead, MBH98 Fortran code ... contains an unusual data transformation prior to PC calculation that has never been reported in print.

Each tree ring series was transformed by subtracting the 1902-1980 mean, then dividing by the 1902-1980 standard deviation and dividing again by the standard deviation of the residuals from fitting a linear trend in the 1902–1980 period.

... for those series in which the 1902–1980 mean is close to the 1400–1980 mean, subtraction of the 1902–1980 mean has little impact on weightings for the PC1.

But if the 1902–1980 mean is different than the 1400–1980 mean...series variance, which grows with the square of each residual, gets inflated. Since PC algorithms choose weights that maximize variance, the method reallocates variance so that hockey stick shaped series get overweighted.

Words: (209, 96, 80, 46%, 39%).

“Summary of Hockey sticks, principal components, and spurious significance by Stephen McIntyre and Ross McKittrick (2005b)” (MM05b) [p.81, 56] climateaudit.files.wordpress.com/2009/12/mcintyre-grl-2005.pdf

WR, p.81, Paragraph 2

MM also ran a Monte Carlo Simulation on 70 of the stationary proxy data series.

When applying the linear transformation described above that was found in MBH98, nearly every simulation yielded first principal components (PC1) with a ‘hockey stick’ shape. Without this transformation, the ‘hockey stick’ shape appeared in the PC1 only 15.3% of the time.

Additionally, the MBH98 method creates a PC1 that is dominated by bristlecone pine and foxtail pine tree ring series (both closely related species).

Out of the 70 sites in the network, 93% of the variance in the MBH98 PC1 is accounted for by only 15 bristlecone and foxtail pine sites, all with data collected by one man, Donald Graybill.

Without the transformation, these sites have an explained variance of less than 8%.

The substantially reduced share of explained variance coupled with the omission of virtually every species other than bristlecone and foxtail pine,

argues strongly against interpreting it as the dominant component of variance in the North American network.

There is also evidence present in other articles calling the reliability of bristlecone pines as an effective temperature proxy into question.

1. Was “one man” added to strengthen the argument? That seems a gratuitous interjection amidst block of copied text (not, not counted) “There is also evidence...” is written as a claim by WP, not as a comment on what MM claim. Did the WP actually research this topic enough to have credible opinions? Deep Climate showed the Striking Similarity of WR tree-ring text to Bradley (1999), but with injected errors.

MM05b, p.2

We carried out 10,000 simulations, in each case obtaining 70 stationary series of length 581 (corresponding to the 1400–1980 period). By the very nature of the simulation, there were no 20th century trends, other than spurious “trends” from persistence.

We applied the MBH98 data transformation to each series in the network:… The simulations nearly always yielded PC1s with a hockey stick shape…

Without the MBH98 transformation (top panel), a 1-s hockey stick occurs in the PC1 only 15.3% of the time.

MM05b, p.3

The MBH98 method creates a PC1 which is dominated by bristlecone pines and closely related foxtail pines....

Out of 70 sites in the network, 93% of the variance in the MBH98 PC1 is accounted for by only 15 bristlecone and foxtail pine sites collected by Donald Graybill....

Without the data transformation, the distinctive contribution of the bristlecones only appears in the PC4, which accounts for less than 8% of the total explained variance.

This substantially reduced share of explained variance, together with the fact that species other than bristlecone/foxtail pines are effectively omitted from the MBH98 PC1, argues strongly against interpreting it as the “dominant component of variance” in the North American network...

...problems relating to the interpretation of bristlecone/foxtail pine growth as a temperature proxy...

Words: (186, 150, 100, 81%, 54%), **Issues:** 1 There have been arguments about pines, but people explained how they coped..

“Summary of Hockey sticks, principal components, and spurious significance by Stephen McIntyre and Ross McKittrick (2005b)” (MM05b) [p.81, 56] climateaudit.files.wordpress.com/2009/12/mcintyre-grl-2005.pdf

WR, p.81, Paragraph 3

MM also evaluated the MBH98 usage of the Reduction of Error statistic in place of the more reliable and widely used Monte Carlo Model to establish significant benchmarks.

By using the Monte Carlo Model, MM found that a more accurate significance level for the MBH98 procedures is .59, as opposed to the level of 0.0 reported in the original study.

A guard against spurious RE significance is to examine other statistics, such as the R2 and CE statistics.

However, MBH98 did not report any additional statistics for the controversial 15th century period.

The MM calculations indicate that these values for the 15th century section of the temperature reconstruction are not significant, thereby refuting the conclusions made by MBH98.

2. “thereby refuting the conclusions made by MBH98” is a rather sweeping conclusion, especially considering the WR was finalized in 2006, 8 years after MBH98, that MBH99 and other studies had superceded MBH98, that MM05b (Figure 3) managed to make 20th century warming, well-established by instrumental records, seem to disappear entirely. MM did not make such a vague, broad claim. Overly strong. Meme-18.

MM05b, p.xxx

... a Monte Carlo model more accurately representing actual MBH98 procedures is 0.59, as compared to the level of 0.0 reported in the original study

An obvious guard against spurious RE significance is to examine other cross-validation statistics, such as the R2 and CE statistics

In the case of MBH98, unfortunately, neither the R2 and other cross-validation statistics nor the underlying construction step have ever been reported for the controversial 15th century period.

Our calculations have indicated that they are statistically insignificant.

Words: (118, 75,41, 64%, 35%), Issues:1. Meme-18

Words: (513, 321, 222, 63%, 43%), Issues: 2.

Ⓞ#47 “Summary of Highly Variable Northern Hemisphere Temperatures Reconstructed from Low- and High-Resolution Proxy Data by Anders Moberg et al. (2005) [pp.82-83, 56] vs www.nature.com/nature/journal/v433/n7026/pdf/nature03265.pdf stephenschneider.stanford.edu/Publications/PDF_Papers/MobergEtAl2005.pdf

WR, p.82, Paragraph 1

In their study, Moberg et al. reconstruct a climate history for the past 2,000 years using low resolution proxies (proxies that provide climate information at multi-centennial timescales, such as ocean sediment cores) and high resolution proxies (proxies that provide climate information on a decadal scale, such as tree rings).

Due to the high profile of high-resolution proxies in reconstructions, mostly from Mann et al. 1998,

views have been expressed that only tree ring and other high resolution data are useful for quantitative large scale temperature reconstructions. However, tree ring data has a well documented unreliability in reproducing multi-centennial temperature variability.

By using low-resolution data for multi-centennial information combined with high-resolution data for decadal information, the most unreliable timescales for each proxy can be avoided.

WR, p.82, Paragraph 2

The dataset used for this study was limited since proxies were required that dated back 2,000 years.

Seven tree-ring series and eleven low-resolution proxy series were used. To obtain a reconstruction covering the complete range of timescales Moberg et al. created a wavelet transform to ensure tree-ring records contribute only to timescales less than 80 years and all low-resolution proxies contribute only to longer timescales.

To calibrate the reconstruction, its mean value and variance were adjusted to agree with the instrumental record of Northern Hemisphere annual mean temperatures in the overlapping period 1856-1979.

Moberg, et al (2005) p.613

These reconstructions have mainly used tree-ring data and other data sets of annual to decadal resolution. Lake and ocean sediments have a lower time resolution, but provide climate information at multicentennial timescales... Here we reconstruct Northern Hemisphere temperatures for the past 2,000 years by combining low-resolution proxies with tree ring data...

Moberg, et al (2005) p.614

... prominent role that the multi-proxy reconstruction by Mann et al. had in the latest IPCC report and in public media...

A view has been expressed that only tree-ring and other high resolution data are useful for quantitative large-scale temperature reconstructions. Tree-ring data, however, have a well-documented difficulty in reliably reproducing multicentennial temperature variability.

Our aim is to combine low-frequency climate information (contained in low-resolution proxy data) with high-frequency information (from tree-ring data) ...

avoid using each proxy type at timescales where it is most unreliable.

The number of available 2,000-yr-long local-to-regional scale temperature proxy series is very limited.

We found seven tree-ring series and eleven low-resolution proxy series.

Given the small number of tree-ring series, their contribution is best thought of as an approximation of the statistical character of variability at <80-yr scales. A sample size of eleven low-resolution series is reasonable for reconstructing >80-yr variability.

Moberg, et al (2005) p.615

To calibrate the reconstruction, its mean value and variance were adjusted to agree with the instrumental record of Northern Hemisphere annual mean temperatures in the overlapping period AD 1856–1979...

Words: (217, 130, 74, 60%, 34%).

“Summary of Highly Variable Northern Hemisphere Temperatures Reconstructed from Low- and High-Resolution Proxy Data by Anders Moberg et al. (2005) [pp.82-83, 56] vs www.nature.com/nature/journal/v433/n7026/pdf/nature03265.pdf stephenschneider.stanford.edu/Publications/PDF_Papers/MobergEtAl2005.pdf

WR, p.82, Paragraph 3

The reconstruction indicates two warm peaks around A.D. 1000 and 1100 and pronounced cold periods in the 16th and 17th centuries. The peaks in medieval times are comparable to those of the 20th century, although warmth seen in post-1990 seems to be unprecedented.

Reconstructions of the temporal evolution of warming variables (volcanic aerosols, solar irradiance and greenhouse gases) have been used to drive simple energy balance climate models as well as fully coupled atmosphere-ocean general circulation models.

Moberg et al. note that the Northern Hemispheric temperature series obtained from such an experiment with the coupled model ECHO-G bears a strong qualitative similarity to their reconstruction.

This supports the case of a pronounced hemispheric low-frequency temperature variability resulting from the climate’s response to natural changes in radioactive forcing.

WR, p.82, Paragraph 4

There are notable differences in the Moberg et al. reconstruction and that of Mann et al. 1998.

While there is a large amount of data in common between the two reconstructions, Mann et al. combined tree-ring data with decadal resolved proxies without any separate treatment at different timescales. Additionally, this study’s dataset contains centennially resolved data from the oceans while Mann et al. used only annually or decadal resolved data from continents or locations near the coast.

Mann et al. also used a different calibration method (regression versus variance scaling as in this study).

1. <e>“Mann et al 1998” ← “Mann and Jones” (2003). Confusion.

Moberg, et al (2005), p.615

The reconstruction depicts two warm peaks around AD 1000 and 1100 and pronounced coolness in the sixteenth and seventeenth centuries...

The peaks in medieval times are at the same level as much of the twentieth century, although the post-1990 warmth seen in the instrumental data (green curve in Fig. 2b) appears to be unprecedented. Changes in radiative forcing due to variability in solar irradiance, the amount of aerosols from volcanic eruptions and greenhouse gas concentrations...

Reconstructions of the temporal evolution of these variables have been used to drive climate models, ranging from simple energy balance models to fully coupled atmosphere-ocean general circulation models.

The Northern Hemisphere temperature series obtained from such an experiment with the coupled model ECHO-G for the AD 1000–1990 period (Fig. 2e) is qualitatively remarkably similar to our multi-proxy reconstruction...

...supports the case of a rather pronounced hemispheric low-frequency temperature variability resulting from the climate system’s response to natural changes in radiative forcing.

Moberg, et al (2005), p.616

There are several reasons for the notable differences between our and previous multi-proxy reconstructions. The reconstruction of Mann and Jones has a large amount of data in common with ours, but these workers combined tree-ring data with decadal resolved proxies without any separate treatment at different timescales. Furthermore, our data set contains some centennially resolved data from the oceans, while Mann and Jones used only annually to decadal resolved data from continents or from locations very near the coast.

Different calibration methods (regression in the work of Mann and Jones versus variance scaling in this study)...

Words: (220, 220, 145, 100%, 65%), Issues:1. Theme-H Late 20th-century warming.

“Summary of Highly Variable Northern Hemisphere Temperatures Reconstructed from Low- and High-Resolution Proxy Data by Anders Moberg et al. (2005) [pp.82-83, 56] vs www.nature.com/nature/journal/v433/n7026/pdf/nature03265.pdf stephenschneider.stanford.edu/Publications/PDF_Papers/MobergEtAl2005.pdf

WR, p.82, Paragraph 5, p.83 (cont)

Further study in the process of weighting different timescales and spatial representation of the data should be conducted to see which method most accurately depicts past climate variability.

This study finds no evidence for any earlier periods in the past two millennia with warmer conditions than the post-1990 period. However, natural multi-centennial climate variability, especially as a response to solar irradiance, may be larger than previously thought.

This does not imply that global warming has been caused by natural factors alone, but that there is

a need to improve scenarios for future climate change by also including forced natural variability.

2.<CB> ”especially as a response to solar irradiance” is odd,

Moberg, et al actually wrote:

“Changes in radiative forcing due to variability in solar irradiance, the amount of aerosols from volcanic eruptions and greenhouse gas concentrations have been important agents causing climatic variability in the past millennium .Post-1990 != last millennium..

Climate scientists certainly believe that part of the warming of the 1850-1950 period was increase in irradiance, but not since ~1980.

(Meme-1? “It’s the Sun” is one of the most common climate anti-science arguments, and this is not the only case where variations in irradiance get emphasized more than they were in the original.)

Moberg, et al (2005)

A goal for further research could be to determine how such weighting should be undertaken,...

We find no evidence for any earlier periods in the last two millennia with warmer conditions than the post-1990 period natural multicentennial climate variability may be larger than commonly thought, and that much of this variability could result from a response to natural changes in radiative forcings.

This does not imply that the global warming in the last few decades has been caused by natural forcing factors alone, as model experiments that use natural only forcings fail to reproduce this warming. Nevertheless, our findings underscore

a need to improve scenarios for future climate change by also including forced natural variability...

Words: (100, 72, 56, 72%, 56%). Issues: 1. Meme-01

Words: (537, 422, 272, 79%, 51%), Issues: 2.

Ⓧ#48 “Summary of The Spatial Extent of 20th Century Warmth in the Context of the Past 1200 Years by Timothy Osborn and Keith Briffa (2006)” [pp.92, 56] vs www.sciencemag.org/cgi/reprint/sci;311/5762/841 (paywall)

WR, p.92, Paragraph 1, part 1

In this article Osborn and Briffa review past work on proxy-based climate reconstruction in an attempt to assess if the claim that the late 20th century was the warmest period during the past millennium is supported.

Whether or not this claim is supported depends on the comparison of recent instrumental temperature records with the earlier proxy-based temperature reconstructions.

This comparison is only valid if it takes an account of the uncertainties associated with interpreting a specific reconstruction as an estimate of the actual temperature.

Some of the reviewed studies do not provide a reconstruction of the entire millennia and some do not estimate the uncertainty in an appropriate manner assessing the significance of late 20th century warmth.

Osborn and Briffa focus on three studies that meet the criteria of a formal quantitative comparison of late 20th century temperatures against reconstructed temperatures for the past millennia. These studies are Mann et al. 1999, Mann and Jones 2003, and Jones, Osborn and Briffa 2001.

While all of these studies supported the claim of unprecedented temperatures in the 20th century and published uncertainties associated with proxy reconstructions above the 95% uncertainty range, the Intergovernmental Panel on Climate Change (IPCC) concluded that this

claim could only be made with a 66 to 90% confidence because of unquantifiable error that may arise from the proxies in the dataset.

1. <e>“entire millennia” ← “millennium” or “1000 years”. Edit errors.
2. <cb>“While...”: The “recent temperatures exceeding the published 95% uncertainty range” disappeared, and the resulting sentence effectively turned that into a “claim by studies.” Weakening.

Osborn, Briffa (2006), p.841

Assessing whether these recent temperatures are unprecedented depends on comparing the recent instrumental temperature record with the earlier proxy-based temperature reconstructions.

Quantitative calibration of the reconstructions is essential, and the comparison with the instrumental record is only valid if it takes account of the uncertainties associated with interpreting a specific reconstruction as an estimate of the actual temperature.

Of the studies cited above, some do not provide reconstructions that cover the whole of the millennium ... or do not estimate reconstruction uncertainty in a way that is appropriate for assessing the significance of very late 20th century warmth

There are, therefore, currently only three studies (5, 11, 16) that allow a formal quantitative comparison of late 20th-century instrumental temperatures against reconstructed temperatures for the past 1000 years or more.

For these reasons, the Intergovernmental Panel on Climate Change (15) correctly judged that the conclusion that recent warmth is unprecedented in the context of the past 1000 years could be made with only 66 to 90% confidence, despite recent temperatures exceeding the published 95% uncertainty ranges of all earlier reconstructed values (5, 11, 16).

Words: (223, 170, 78, 76%, 35%), Issues: 2.

“Summary of The Spatial Extent of 20th Century Warmth in the Context of the Past 1200 Years by Timothy Osborn and Keith Briffa (2006)” vs www.sciencemag.org/cgi/reprint/sci;311/5762/841 (paywall)

WR, p.92, Paragraph 1, part 1

Osborn and Briffa conduct their own analysis of the proxy data of Mann et al. 1999 by smoothing the data and simply counting the fraction of records with values that exceed one or two standard deviations from the mean.

The differences between pairs of these fractional exceedance time series (or the fraction of records at least one standard deviation above the mean minus the fraction of records with at least one standard deviation below the mean) were also analyzed.

The highest positive deviations occur in the late 20th century, even far exceeding those of the mid-20th century.

WR, p.92, Paragraph 2

The instrumental temperature results show a close correspondence with the proxy records, especially for the early 20th century increase and variations during 1930-1975.

Additionally, the multi-decadal intervals

support the concepts of the medieval warming period and Little Ice Age period.

However, the dates of onset are vague and the analysis geographically restricted.

The most conclusive finding is that the 20th century is the most anomalous interval in the entire period of analysis, including significant positive extremes in the proxy records.

3. <cb> “analysis geographically restricted” and warming/cooling “geographically restricted have (somewhat) different meanings..Meme-59.

Osborn, Briffa (2006), p.841

The proxy records were analyzed simply by counting the fraction of those series that have data in any given year whose smoothed and normalized values exceed certain thresholds. The thresholds used are the series mean and 1 or 2 SD above or below the mean.

The differences between pairs of these fractional exceedance time series were also analyzed (i.e., the fraction of records at least 1 SD above the mean minus the fraction that are at least 1 SD below the mean).

Osborn&Briffa (2006), p.842

...shows only small deviations from zero throughout the analysis period except during the late 20th century, which exceeds all other periods, including the mid-20th century.

The instrumental temperature results show a close correspondence with the proxy records, particularly for the early 20th century increase and the variations during the 1930 to 1975 period...

The multidecadal intervals (Figs. 2 and 3) with significantly widespread positive anomalies between 890 and 1170 and significantly widespread low proxy values between 1200 and 1850 (interspersed by periods with high or near zero anomalies) provide support for the concepts of anomalous medieval (29) and Little Ice Age (30) periods (particularly from the late 1500s to the mid-1800s), although they are clearly discontinuous in time (with consequently ill-defined dates of onset and termination) and geographically restricted.

The 20th century is the most anomalous interval in the entire analysis period, with highly significant occurrences of positive anomalies and positive extremes in the proxy records.

Note: “highly” got deleted.

Theme-G: MWP variability. Theme-H: Late 20th-century warmth.

Words: (177, 177, 102, 100%, 58%), Issues: 1. Theme-G, Theme-H. Meme-56.

Words: (400, 347, 180, 87%, 45%), Issues:3.

49 “Summary of *Proxy-Based Northern Hemisphere Surface Temperature Reconstructions: Sensitivity to Method, Predictor Network, Target Season and Target Domain* by Rutherford et al. (2005)” [pp.88-89] www.meteo.psu.edu/~mann/shared/articles/RuthetalJClimate05.pdf

WR, p.88, Paragraph 1

Rutherford et al. discuss the necessity of climate reconstruction with multi-proxy networks as empirical evidence in documenting past climate variability.

In this case, they note the advantage of using high-resolution proxies (annually or seasonally resolved proxies, such as tree rings, corals and ice cores) because they overlap with instrumental data of the past century, allowing analysis of their climate signal and reliability.

These proxies have been used to reconstruct spatial climate fields which not only provide a climate variability record but which also retain information of the mechanisms or forcing underlying the variability.

Annually resolved proxy networks have also been used to directly reconstruct indices of climate variability, but these methods are somewhat flawed in that they assume a direct relationship between the recorded proxy variables and temperature and precipitation

but large-scale climate influences may change over time.

Rutherford et al. focus specifically on recent constructions of this type of Northern Hemisphere temperatures and the reasons for the differences between reconstructions.

WR, p.88, Paragraph 2

There are four identifiable factors that largely contribute to differences in reconstructions. Those are

- 1) using proxies as calibrators for surface temperature patterns,
- 2) the difference in character of the proxy networks used,
- 3) the target season of reconstruction, and
- 4) the target region of reconstruction.
 1. “flawed” ← “limited,”
 2. <e> “construction” ← “reconstruction” not the same, poor edit
 3. <e> “this type of Northern Hemisphere temperatures” awkward editing.

Rutherford, et al (2005), p.2308.

...via the careful use of long-term empirical evidence... (annually or seasonally resolved) proxies such as tree rings ..., corals ... ice cores

Rutherford, et al (2005), p.2309.

A critical advantage of using such high-resolution proxy data is the possibility of comparing the proxies against long temporally overlapping instrumental records both to estimate the climate signal in the data (calibration) and independently test the reliability of the signal (verification or cross validation).

Annually resolved proxy indicators have been used to reconstruct spatial climate fields...

retain vital information that can provide insight into the mechanisms or forcing underlying observed variability...

Annually resolved proxy networks have also been used to directly reconstruct indices of climate variability...

Such approaches are potentially limited by the assumed relationship between local variables recorded by the proxies (temperature and precipitation) and larger-scale climate patterns, since the relationship between local and large-scale influences may change over time...

Of particular interest in this study are various recent reconstructions of NH temperature... Some differences do exist, however, among hemispheric temperature reconstructions...

...several distinct factors in varying combinations could be responsible for the differences between reconstructions.

One factor is the method...

Rutherford, et al (2005), p.2310

... the potentially different character of the proxy network used...

An additional factor is the target season of the reconstruction...

A final related factor is the target region of the reconstruction...

Words: (207, 175, 79, 85%, 38%), Issues: 3.

“Summary of Proxy-Based Northern Hemisphere Surface Temperature Reconstructions: Sensitivity to Method, Predictor Network, Target Season and Target Domain by Rutherford et al. (2005)” [pp.88-89] www.meteo.psu.edu/~mann/shared/articles/RuthetalJClimate05.pdf

WR, p.88, Paragraph 2 (cont)

The intent of this study is to provide an assessment of the relative impacts of these four factors.

WR, p.88, Paragraph 3

To measure the sensitivity of the proxy network selected, three networks were used: the multiproxy dataset used by Mann et al., the MXD data used by Briffa et al., and a combination of these datasets for the third network.

To perform the reconstruction on these three networks a RegEM approach of climate field reconstruction was used.

The RegEM method is an iterative method for estimating missing data through the estimation of means and covariances from an incomplete data field. The calibration interval for this approach was the time interval that includes overlap of proxy and instrumental data.

Rutherford et al. made two modifications to the RegEM approach. First, they applied the method in a stepwise fashion, performing the reconstruction one step at a time using all available climate information.

Second, they separated the datasets into low and high frequency datasets to create two independent reconstructions, which were then combined at the conclusion of the experiment to create a complete reconstruction.

In the findings, Rutherford et al. stated that using a 20 year boundary for the frequency calibration gave superior results in almost all cases while the stepwise modification of the RegEM method did not produce any different results. Additionally, since the combined network showed only marginal improvement over the other two, it is likely that these reconstructions are relatively insensitive to the proxy network used.

Rutherford, et al (2005), p.2311.

The intent of this study is to provide a systematic assessment of the relative impacts of these four factors

We used two largely independent predictor networks to assess the sensitivity of the temperature reconstructions to the network used. The first of these is a multiproxy dataset used by Mann ... MXD data used by Briffa and coworkers... We also prepared third “combined” network by combining both networks...

Rutherford, et al (2005), p.2313.

...including applications to paleoclimate field reconstruction...

The REGEM method is an iterative method for estimating missing data through the estimation of means and covariances from an incomplete data field... a calibration interval can be defined as the time interval over which the proxy and instrumental data overlap,

Rutherford, et al (2005), p.2314.

We have modified the application of the method in two ways...

The REGEM approach was in all cases applied in a stepwise fashion. The reconstruction is performed one step at a time, using all available climate field information...

...dataset is split into two distinct datasets, through application of a low-pass filter to the data. The low-pass component of the data defines the low-frequency component, while the residual defines the high-frequency component.

Rutherford, et al (2005), p.2319.

...finding the 20-yr period boundary to give superior results in almost all cases...

Rutherford, et al (2005), p.2320.

The fact that the combined network performs, at best, only marginally better than the two independent networks alone...

Words: (243, 185, 106, 76%, 44%).

“Summary of Proxy-Based Northern Hemisphere Surface Temperature Reconstructions: Sensitivity to Method, Predictor Network, Target Season and Target Domain by Rutherford et al. (2005)” [pp.88-89] www.meteo.psu.edu/~mann/shared/articles/RuthetalJClimate05.pdf

WR, p.89, Paragraph 1

To measure the sensitivity of the target season and region on reconstruction, Rutherford et al. performed an array of RegEM climate field reconstructions based on various seasons and regions. These reconstructions were compared with several previous reconstructions based on common predictor datasets.

Rutherford et al. found that the optimal results for the MXD data were produced for the period in which the cold season ends while the optimal results for the multiproxy principal component reconstruction (Mann and coworkers reconstruction) were produced for the period in which the cold season begins.

Additionally, the MXD network was found to outperform the combined network in the warm season.

In terms of region, Rutherford et al. found differences in the target region lead to significant variability in the hemispheric mean estimates.

While they found that reconstructions are sensitive to changes in season and region, Rutherford et al. maintained that the unprecedented temperatures in the late 20th century that are seen in many reconstructions are supported

with respect to all of the factors considered in this study.

4.“maintained” can have a subtle negative connotation, in the sense of supporting against opposition. This is fairly minor, but is consistent with pervasive other subtle word changes.

Rutherford, et al (2005), p.2315.

We performed an array of REGEM CFR experiments based on different target seasons and proxy networks... We compared our results against previous reconstructions based on common predictor datasets...

Rutherford, et al (2005), p.2319

For the MXD network, optimal results were achieved for cold-season reconstructions when predictors were temporally aligned with the predictand during the year in which the cold season ends...

in which predictors are aligned with the predictand during the year in which the cold season begins.

Rutherford, et al (2005), p.2320

...the MXD network outperforms the combined network for the warm season...

Rutherford, et al (2005), p.2326

Differences in target region appear to lead to more substantial differences... yield “hemispheric mean” estimates with increasingly greater variability...

Differences in target seasonal window are also important... Finally, the evidence for exceptional late-twentieth-century warmth in the context of the period since A.D. 1400 (in warm, cold, and annual temperatures) is a robust conclusion with respect to all of the factors considered.

What is the value of twice changing the precise “year” to the vague “period”?

Theme-F₄: Surface temperatures vary geographically

Theme-H₄: Late 20th-century warming.

Theme-J₄: Confidence intervals.

Words: (173, 139, 58, 74%, 34%). Issues: 1. Theme-F₄, Theme-H₄, Theme-J₄

Words: (623, 499, 243, 80%, 39%), issues: 4.

Ⓞ#54 “Summary of Reconstructing Past Climate from Noisy Data by Hans von Storch et al. (2004)” [pp.78, 57] vs www.sciencemag.org/cgi/reprint/306/5696/679.pdf (paywall)

WR, p.78, Paragraph 1

While attempting to measure anthropogenic effects on the earth’s climate, it is necessary to create a reconstruction of past climate variations. Most studies have identified

varying warm values in the 11th and 12th centuries followed by secular cooling periods in the mid- 16th, 17th and early 19th centuries.

These cooler intervals were followed by warming that is still experienced today.

The amplitude of these preindustrial variations is debated, although the most notable study on the subject and the most quoted, Mann et al. 1998 (MBH98), as well as the Intergovernmental Panel on Climate Change (IPCC), report that these variations were of small amplitude.

However, recent studies have suggested that centennial variations may have been larger than previously thought.

This study uses a coupled atmosphere-ocean model simulation of the past millennia as a surrogate climate to test the reconstruction method of MBH98.

WR, p.78, Paragraph 2

Using this model as a virtual world to determine the skill of regression-based reconstruction models like MBH98, von Storch et al. found that the model is reasonably skilled at reproducing short-term variations but substantial underestimation occurs in the long-term estimations.

On an inter-annual scale, the reconstruction has a

calibration reduction-of-error statistic of .7 for perfect pseudo-proxies and .3 for pseudo-proxies with a higher degree of noise.

- 1.<e> “Millennia” ← “1000 years”
- 2. “models like MBH98” ← “methods” not same, confused.
- 3.<e> “estimations” ← “variations”: result makes no sense.

Von Storch, et al (2004), p.679

Reconstruction of past climate from palaeoclimate proxy data is important for the detection of anthropogenic climate change. ...

A number of reconstructions show that the temperatures in the last millennium were characterized by geographically

varying warm values in the 11th and 12th centuries, followed by a secular cooling trend punctuated by decadal-scale colder periods in the mid-16th, early 17th, and early 19th centuries (6).

These cooler intervals were followed by the marked warming experienced until today.

Although the amplitude of these preindustrial variations is still debated, according to the most quoted NH temperature reconstruction Mann, Bradley, Hughes, 1998 (MBH98) (1) and Mann, Bradley, Hughes, 1999 (MBH99) (2) and the most recent Intergovernmental Panel on Climate Change (IPCC) report (7), these variations were of small amplitude.

However, recent studies with general circulation models suggest that these centennial variations may have been larger.

We used a coupled atmosphere-ocean model simulation of the past 1000 years as surrogate climate to test whether the reconstruction method of MBH98....

Von Storch, et al (2004), p.680

... it will be used as a virtual world to determine the skill of regression-based reconstruction methods like MBH98...

The short-term variations are reasonably reproduced...

substantial underestimation of low-frequency temperature variations ...

For instance, on an interannual time scale, the fit between simulated and reconstructed NH temperature is good, with a

calibration reduction-of-error statistics of 0.7 for perfect pseudoproxies and 0.30 for pseudoproxies with $e = 0.5$.

Theme-GⓄ: MWP variability.

Words: (207, 168, 130, 81%, 63%), Issues:3. Theme-GⓄ..

“Summary of Reconstructing Past Climate from Noisy Data by Hans von Storch et al. (2004)” vs
www.sciencemag.org/cgi/reprint/306/5696/679.pdf (paywall)

WR, p.78, Paragraph 2 (cont)

However, only 20% of the 100-year variability is recovered when the noise level is approximately 50%.

Similar results were obtained using the third Hadley Centre coupled model (HadCM3), indicating the results are not dependent on the model used.

WR, p.78, Paragraph 3

Von Storch et al. also tested a number of other hypotheses.

They found that including more instrumental data in the proxies does not improve results, expanding the proxy set in sparse areas improved results marginally, and that expanding the range of temperature variability present in the pseudo-proxies greatly improves the results.

Additionally, von Storch et al. questioned the validity of linear regression models in general in estimating climate.

Using pseudo-proxies to estimate local temperatures which were then spatially

averaged to derive a Northern Hemisphere temperature,

they found similar problems that occur in

MBH98:

underestimation of low-frequency variability for a given amount of noise.

The authors conclude that climate simulations of the past millennium are burdened by model limitations and uncertainties in external forcing and therefore the output must be considered with care.

Note: is it clear to casual reader that this is not a general comment on models? (This relates to other discussions). Strong.

Additionally, the linear regression methods as used in MBH98, suffer from marked losses of centennial and multidecadal variations.

4. <cb> “validity” ← “limitations,” Stronger.

5. “similar problems” lost qualifiers, Stronger.

Von Storch, et al (2004), p.680

For example, only 20% of the 100-year variability is recovered when the noise level is 50%.

Similar results are obtained with a simulation with the third Hadley Centre coupled model (HadCM3), demonstrating that the results obtained here are not dependent on the particular climate characteristics of the ECHO-G simulation

Our setup allowed the test of a number of hypotheses.

The first hypothesis is that the inclusion of more instrumental data would improve the estimate...

Von Storch, et al (2004), p.681

The second hypothesis is related to the sparseness of the proxy locations.. ... we tested whether the range of variability present in the instrumental period is sufficient to reconstruct the climate of past centuries.

A further question is whether the limitations we have found are common to regression methods in general... In the first,

local temperatures were estimated by a linear regression from pseudoproxies, and the local temperature estimations were spatially averaged to derive the NH temperature...

For the first method, we found qualitatively similar but quantitatively even worse problems than with the MBH98 method; that is, the underestimation of low frequency variability for a given amount of noise is greater than for MBH98

Von Storch, et al (2004), p.682

Climate simulations of the past millennium are burdened by model limitations and uncertainties in the external forcing, and therefore their output must be considered with care.

von Storch, et al seem to properly caveat their use of models.

However, they provide a surrogate climate realistic enough to conclude that the use of the regression methods considered here, ...

suffer from marked losses of centennial and multidecadal variations.

Words: (188, 131, 91, 79%, 51%), Issues: 2.

Words: (395, 316, 225, 80%, 57%), Issues: 5.

#58 “Summary of Ocean Observations and the Climate Forecast Problem by Carl Wunsch (2002)” [pp.74, 57] vs ocean.mit.edu/~cwunsch/papersonline/rmsbookpaper.pdf

WR, p.74, Paragraph 1

Due to the recent importance of studying climate change, it has become apparent that there are significant **problems in observing the ocean** and its **climate**.

Much of the problem is technical, but there is also the **matter of culture** and **misapprehension**.

Many in the field of meteorology continue to have an antiquated and misleading perception of the ocean circulation.

In his article, Wunsch outlines the reasons for many of the problems in observing the ocean.

WR, p.74, Paragraph 2

Since **the opacity of the ocean** has made it difficult to observe until recent

technological innovations, and the cost of supporting **oceanographic ships** is prohibitive, **time series of oceanic variables were almost nonexistent**.

*The only variables relatively easy to measure and interpret were **properties such as temperature, salinity and oxygen**.*

Since these properties are particularly stable, the static picture of ocean circulation became the predominant view.

*However, with the advent of modern electronics, obtaining **time series of oceanographic data** became easier.*

After years of **literature and data on the subject**, it became clear **that the ocean is actually quite turbulent** under the surface and

that few, if any, elements of ocean circulation are truly steady.

WR, p.74, Paragraph 3

There **exists a large-scale oceanic circulation that appears to be steady** over decades, but is thought to be slowly changing everywhere **in ways not yet known**.

However, the current understanding of how oceanic circulation will affect the climate is actually very narrow.

Wunsch (2002), p.233

The **problems in observing the ocean** have come in recent years to loom large, often because of the importance of the ocean in **climate** and climate change.

Many of the problems are technical ones, but a number of them might be regarded as being more a **matter of culture**, or of **misapprehension**, than of science....

Many in the highly sophisticated meteorological community continue to have an antiquated and misleading perception of the ocean circulation....

The opacity of the ocean to electromagnetic radiation...

Wunsch (2002), p.234

Consider that **oceanographic ships** are very expensive devices....

Thus **time series of oceanic variables were almost nonexistent**....

...in great contrast to the velocity field, scalar **properties such as temperature, salinity, oxygen** ...

...proved to be apparently stable...

Wunsch (2002), p.238

...modern electronics had evolved...one could (nearly) routinely obtain **time series of oceanographic data**...

A very large **literature** has developed **on this subject**. What was reasonably evident **that the ocean is intrinsically turbulent**.

Wunsch (2002), p.239

...it has become clear **that few if any elements of ocean circulation are truly steady**.

There is a large-scale oceanic circulation, which appears to be stable over decades, but **expected** to be slowly changing in ways we do not understand because we do not have adequate measurements of it.

Words: (232, 166, 88, 72%, 38%).

“Summary of Ocean Observations and the Climate Forecast Problem by Carl Wunsch (2002)” [pp.74, 57] vs ocean.mit.edu/~cwunsch/papersonline/rmsbookpaper.pdf

WR, p.74, Paragraph 3 (cont)

Additionally, the problem is further compounded by the fact that models have become so sophisticated and interesting, it is tempting to assume they must be skillful.

Most papers written on the subject of oceanographic models give little or no guidance to the reader as to the actual expected skill of the model.

This type of research begs the question, is it really plausible that a 4° or 1° ocean model can be integrated with skill for 1000 years?

The magnitude of the error in these models is enormous when integrated over such a long time period. The evidence for the skillfulness of similar models is scant.

WR, p.74, Paragraph 4

The assumption that the oceanic system is much simpler than it actually is leads to a corruption of the entire literature.

Readers of paleoclimate papers will notice that extraordinarily complicated and far-reaching changes in the climate system are often reduced to simple assertions about how the “global conveyor” changed.

One might also be suspicious of “concrete” evidence of atmospheric modeling because atmospheric modeling must be equally if not more difficult than modeling the ocean.

In order to begin to make any kind of model, years of observation with oceanographic satellites are needed.

Most of these satellites are not currently regarded as operational.

Of primary concern is to insure that everyone understands the problem and to recognize the great influence past assumptions exercise over future necessity.

1. *Loss of context makes Wunsch’s comments about models of atmospheric flow seem like negatives about models in general. Meme-05?*

Wunsch (2002), p.242

The problem is further compounded by the fact that models have become so sophisticated and interesting, it is tempting to assume they must be skillful. ...

Wunsch (2002), p.243

...the reader is usually given little or no guidance as to the actual expected skill of such models.

Is it plausible that a 4° or even 1° ocean model can be integrated with skill for 1000 years?

If there is real skill, then the modeling community has solved one of the most difficult of all problems in turbulence: that of a rotating, stratified fluid in a complex geometry. What is the evidence for its truth?

At its worst, the assumption that the system is much simpler than it actually is, leads to the corruption of an entire literature.

Readers of paleoclimate papers in particular, will notice that extraordinarily complicated and far-reaching changes in the climate system are often reduced to simple assertions about how the “global conveyor” changed.

One might be suspicious of such claims about the atmospheric circulation. I am unaware of any concrete evidence that modeling the ocean is any simpler than modeling the atmosphere.

Indeed, one might readily infer that it is much more difficult...

Wunsch (2002), p.244

...we need, and will surely continue to need for many years, and adequate set of observations of the ocean...

...none of the oceanographic satellites of the most urgent importance ... is yet regarded as operational....

The first order of business is evidently, to be clear that everyone understands the problem, and to recognize the great influence past assumptions exercise over future necessity.

Words: (231, 185, 135, 80%, 58%). **Issues:**1. This somewhat over-generalizes Wunsch’s comments to cast doubt on models in general.

Words: (463, 351, 223, 76%, 48%), **Issues:**1. **Questionable Relevance.** Theme-E4. Ocean oscillations are not forcings.

#59 “Summary of Abrupt Climate Change: An Alternative View by Carl Wunsch (2006)” [pp.90-91, 57] vs
ocean.mit.edu/~cwunsch/papersonline/abrupt2006.pdf

WR, p.90, Paragraph 1

A Dansgaard-Oeschger (D-O) event is a rapid climate fluctuation, taking place at the end of the Ice Age.

Twenty-three such events have been identified between 110,000 and 23,000 years before present.

A widely held view of abrupt climate change during the last glacial period is that these

D-O events are at least hemispheric, if not global and caused by changes in ocean circulation.

It has been hypothesized that there may be abrupt climate change similar to a D-O event because of ongoing global warming and its oceanic affects.

Underlying the major conclusions about D-O events and abrupt climate change there are several assumptions, including

(1) the ^{18}O variations appearing in ice core are viable as a proxy,

(2) climate fluctuations in Greenland reflect those on a hemispheric or global basis,

(3) the cause of D-O events can be traced to major changes of the North Atlantic meridional overturning circulation and perhaps failure of the Gulf Stream, and

(4) apparent detection of a D-O event at a remote location in a proxy implies local climatic importance.

In this article Wunsch reexamines these assumptions in order to assess their relevance, specifically focusing on (2) and (3).

1. “taking place at the end of the Ice Age” is strange wording, especially given that the 2nd sentence places them during the previous glacial period. D-O events have not occurred during the period studied by MBH99 (or even for thousands of years before). “the Ice Age” is also odd ..

Wikipedia, 29:37 19 February 2006 (but many others)

en.wikipedia.org/w/index.php?title=DansgaardOeschger_event&oldid=40330757

Dansgaard-Oeschger events are rapid climate fluctuations during and at the end of the last ice age.

Twenty-three such events have been identified between 110,000 and 23,000 years BP

Wunsch (2006), p.191

The widely-held view of abrupt climate change during the last glacial period, as manifested, particularly, in the so-called Dansgaard–Oeschger (D–O) events, is that they are at least hemispheric, if not global, ~~in extent,~~ and caused by changes in the ocean circulation.

~~The possibility of abrupt climate change occurring because of the ongoing global warming and its oceanic effects is attracting great attention.~~

Underlying the ~~now very large literature of interpretation~~ are several assumptions, assertions and inferences including:

(1) The ^{18}O variations appearing in the record of Figure 1. are a proxy for local temperature changes.

(2) Fluctuations appearing in Greenland reflect climate changes on a hemispheric, and probably global, basis and of large amplitude.

(3) The cause of the D–O events can be traced back to major changes (extending to “shutdown”) of the North Atlantic meridional overturning circulation and perhaps even failure of the Gulf Stream.

Wunsch (2006), p.192

(4) Apparent detection of a D–O event ~~signature~~ at a remote location in a proxy implies its local climatic importance.

The purpose of this paper is to briefly re-examine these assumptions and assertions, but with emphasis on (2) and (3).

Words: (194, 194, 137, 100%, 71%), .Issues: 1.

“Summary of Abrupt Climate Change: An Alternative View by Carl Wunsch (2006)” [pp.90-91, 57] vs ocean.mit.edu/~cwunsch/papersonline/abrupt2006.pdf

WR, p.90, Paragraph 2

In terms of using 18Oxygen in the Greenland ice cores as a climate proxy, Wunsch found that although it was relatively accurate for central Greenland, when aligned with other locations a visual similarity would appear on the spectral graph, but that there was actually little statistical correlation; this occurred when comparing time periods of less than 900 years.

While this does not disprove the hypothesis of a large impact of the D-O events, it cannot be used to support this assumption.

There are three possible explanations for the disappearance of covariance for these periods less than 900 years.

First, although both records have wide variability, it is primarily regional in character and there is no simple relationship between them.

Second, the age-model (the calibration of age versus depth in the core) error has a larger influence on the short period variations than the long period ones.

Third, different physical processes dominate the proxies at high frequency in the two separate locations, but they have roughly similar low spectral moments.

Any of these factors could affect the lack of covariance between geographical locations.

Subsequently, the assumption that there exist large-scale hemispheric correlations with the D-O events is neither proven nor disproven.

2. *“impact” ← “spatial extent” : a precise term was made ambiguous. Change from “coherence” to “covariance” seems slightly odd, given that paleoclimate uses*

Wunsch (2006), p.193

Seems a reasonable summary, following sentence marginal:

Despite the alignment, there is no statistically significant coherence between the records at periods shorter than about 900 yr.

Such a result does not disprove the hypothesis of large spatial extent of the D-O events, but the record, showing no high frequency covariance, cannot be used to support the inference...

Wunsch (2006), p.194

The disappearance of any coherence at periods shorter than about 900 yr has at least three explanations:

(1) Although both records have a physically rich variability, it is primarily regional in character and there is no simple relationship between them. This interpretation would be similar to that describing, e.g., London-UK and New York City daily temperature variations.

(2) The age-model error has a larger influence on the short period variations than on the long-period ones (consistent e.g., with the analytical results of Moore and Thomson, 1991, and Wunsch, 2000) and destroys what would otherwise be a strong coherence.

(3) Different physical processes dominate the proxies at high frequency in the Cariaco Basin and Greenland, but they have roughly similar low spectral moments.

On the basis of these two records, one cannot distinguish these explanations and all three may well be operating.

The hypothesis that there exist large-scale hemispheric correlations of the D-O events remains neither proven nor (within the age-model errors) disproven.

Changing “inference” to “assumption,” and “hypothesis” to “assumption” seem careless, as they are not the same.

Words: (200, 155, 90, 78%, 45%). Issues: 1.

“Summary of Abrupt Climate Change: An Alternative View by Carl Wunsch (2006)” [pp.90-91, 57] vs
ocean.mit.edu/~cwunsch/papersonline/abrupt2006.pdf

WR, p.90, Paragraph, p.3

The heat flux associated with meridional overturning (the sinking and spreading of cold water and dispersion of heat) of the ocean has the most direct impact on the atmosphere in terms of oceanic circulation patterns.

The contribution of the oceanic Northern Hemisphere to this pole-ward circulation falls very rapidly as heat is transferred to the atmosphere.

At the 40th latitude North, the oceanic contribution is less than 25% of the atmospheric contribution.

Hypothetically, if warming continues, and the Northern Atlantic is injected with fresh water from glacial melting, the meridional overturning circulation would be dramatically reduced, resulting in a D-O-like event.

However, models attempting to construct this theoretical climate change have not been successful, mostly in that they have not taken into account

the overlying wind field response to this event.

Since much of the temperature flux of the North Atlantic is carried in the Gulf Stream, scenarios requiring wind shifts sufficient to shut it down are likely a physical impossibility because of the need to conserve angular momentum in the atmosphere.

WR, p.91, Paragraph, p.1

Coupled models that have been claimed to show an atmospheric response to oceanic flux shift are so simplified and lack adequate resolution

that they cannot be skillfully integrated over the time periods required to describe true climatic time scales.

Again, these models are only indicators of processes that can be operating but with no evidence that they dominate.

3. <ec> “poleward circulation” not equal “poleward of about 25 N,” minor.

4. Model Meme-21? Meme-02? Meme-05?

Wunsch (2006), p.196

First, the oceanic Northern Hemisphere contribution poleward of about 25-N falls very rapidly as heat is transferred to the atmosphere ...

By 40-N, the oceanic contribution is less than 25% of the atmospheric contribution.

Wunsch (2006), p.197

The hypothesis is that an injection of fresh water would dramatically reduce the meridional overturning circulation ...

...models, ..., do not have the resolution, either vertical or horizontal, to properly compute the behavior of fresh water and its interaction with the underlying ocean and overlying atmosphere...

Wunsch (2006), p.197

Scenarios ... important changes in the overlying wind field in response,...

In any event, much of the temperature flux of the modern North Atlantic is carried in the Gulf Stream; scenarios requiring wind shifts sufficient to shut it down are likely a physical impossibility because of the need to conserve angular momentum in the atmosphere.

Coupled models that have been claimed to show atmospheric response to oceanic mass flux shifts do not themselves resolve the major property transport pathways of either ocean or atmosphere.... lack adequate resolution and dynamical and physical components required for true realism. Little evidence exists that such simplified representations of the climate system can be integrated skillfully over the long periods required to describe true climatic time scales...

They remain primarily as indicators of processes that can be operating, but with no evidence that they dominate.

Words: (236, 173, 81, 73%, 34%), Issues: 2.

“Summary of Abrupt Climate Change: An Alternative View by Carl Wunsch (2006)” [pp.90-91, 57] vs ocean.mit.edu/~cwunsch/papersonline/abrupt2006.pdf

WR, p.91, Paragraph, p.2

While the abrupt climate changes in Greenland may not have occurred in other parts of the globe, there still is the question of why it occurred in Greenland.

One apparent observation is that the D-O events ceased in the Holocene and have been remarkably placid since.

As such, the operative mechanism causing the D-O events must have also disappeared.

The answer is the disappearance of the Laurentide and Fennoscandian ice sheets.

Two enormous mountain ranges of high albedo (reflection factor) were removed.

In a study by Jackson (2000), he noted that small, regional changes in the ice sheet elevations had a large effect on the atmospheric stationary wave patterns.

As a standing wave, the wind encountering the ice sheets has more than one equilibrium state.

Major local climate change could appear with a slight shift in the wave pattern of the wind system.

While the model for this hypothesis is rough, other studies have indicated great influence of the ice sheets on atmospheric scales as well.

The body of these theories suggests that the most important and sensitive determinant of oceanic circulation is wind, and not the temperature flux.

Similarly, the widely accepted view that D-O events were of global impact and may occur as a result of recent warming is based on four assumptions, which in turn are based on ambiguous data and a high degree of uncertainty. As such, to make conclusions about such events would be imprudent without first addressing the uncertainties in the age-model as well a cautious reinterpretation of proxy signals.

“D-O events ceased in the Holocene’. MBH99 covers the last millennium (of 12) in the Holocene. Did the WP not notice this?

Wunsch (2006), p.199

Reasonable summary of paragraph starting “An alternative view”

...the disappearance of the Greenland D-O events in the Holocene (approximately the last 10,000 yr) and its remarkable placidity since... If one takes this disappearance as a clue to the operative mechanisms, it leads one to ask what was the major change between the glacial period and the Holocene?

The answer is, of course, immediate it is the disappearance of the Laurentide and Fennoscandian ice sheets.

In effect, two enormous mountain ranges of high albedo, nearly bracketing Greenland, were removed.

Jackson (2000) studied...demonstrated that small regional changes in the ice sheet elevations had a large effect on the atmospheric stationary wave patterns...westerly wind structure, the standing-wave patterns, encountering the massive ice sheets ... and that more than one equilibrium is possible.

Wunsch (2006), p.200

Major local climate change could appear,...

The body of theory suggests that the most important and sensitive determinant of the circulation is the wind field.

No obvious antecedent.

Theme-E₄: *Ocean oscillations are not forcings.*

Words: (255, 126, 79, 49%, 31%). Meme-21₁, Meme-02₁, Meme-05₂ Theme-E₄

Words: (885, 647, 387, 73%, 44%), Issues:4. Questionable relevance. D-O events 10 millennia ago, or longer.

Version History, Reminders of Issues

V1.0 09/26/10 Initial release