

VIRGINIA:

IN THE CIRCUIT COURT FOR THE 31ST JUDICIAL CIRCUIT

THE AMERICAN TRADITION INSTITUTE,
and
THE HONORABLE DELEGATE ROBERT
MARSHALL

Petitioners,

v.

RECTOR AND VISITORS OF THE
UNIVERSITY OF VIRGINIA,

Respondents; and,

MICHAEL E. MANN,

Intervenor.

Civil Docket No. CL 11-3236

*Petitioners' Response to Respondent
and Intervenor Memo in Opp. To
Petition for Mandamus and Relief*

**PETITIONERS' RESPONSE TO RESPONDENT AND INTERVENOR
MEMORANDUM IN OPPOSITION TO PETITIONERS'
VERIFIED PETITION FOR MANDAMUS AND INJUNCTIVE RELIEF**

CONTENTS

I. Introduction.....	1
II. Standard of Evidence.....	2
III. There is no First Amendment public speech at issue	3
IV. There is no Academic Freedom “Scholars Privilege” nor is one desirable	9
A. There is no “Scholars Privilege”	11
B. The creative period is protected under Exemption 4.....	14
C. Confidential information is protected under Exemption 6.....	14
D. The marketplace of ideas is not fragile and has not been chilled.....	14
E. Confidential peer review does not eliminate the value of public reviews.....	19
F. Mann has never been exonerated	25
V. All the Withheld Documents are Records	29
VI. VFOIA requires the adjective “Proprietary” be defined narrowly	31
A. Petitioners’ Definition of Proprietary	32
B. The Commercially Competitive Interest Period	33
C. Abandonment of Research.....	36
D. There is no Research Period at issue in this case.....	37
E. Respondents Improperly Seek to Define Proprietary Broadly	37
VII. There are no student records among the exemplars.....	41
VIII. There are no personal or personnel records among the exemplars	43
IX. Broadening Exemptions through redaction	45
X. Conclusion	46

Cases

<i>Adams v. Trs. of the Univ. of N. Carolina-Wilmington</i> , 640 F.3d 550, 565 (4th Cir. N.C. 2011) ..	6
<i>Bevis v. Bethune</i> , 232 Fed. Appx. 212 (4 th Cir. 2007)	5
<i>Branzburg v. Hayes</i> , 408 U.S. 665 (1972).....	12, 36
<i>Bunch v. Artz</i> , 71 Va. Cir. 358, 364 (Va. Cir. Ct. 2006).....	11
<i>Connick v. Myers</i> , 461 U.S. 138 (1983).....	4
<i>DiMeglio v. Haines</i> , 45 F.3d 790 (4th Cir. 1995).....	4
<i>Garcetti v. Ceballos</i> , 547 U.S. 410 (U.S. 2006)	4, 5
<i>German v. Fox</i> , 2007 U.S. Dist. LEXIS 30895 (W.D. Va. Apr. 26, 2007)	4
<i>German v. Fox</i> , 267 Fed. Appx. 231 (4th Cir. Va. 2008).....	5, 7
<i>Holland v. Rimmer</i> , 25 F.3d 1251 (4th Cir. 1994).....	5
<i>Howard v. County of Durham</i> , 2011 U.S. Dist. LEXIS 62957 (E.D.N.C. June 14, 2011).....	6
<i>In re Philip Morris</i> , 706 So. 2d 665 (La.App. 4 Cir. Jan. 28, 1998).....	13, 36
<i>In re R.J. Reynolds Tobacco Co.</i> , 136 Misc. 2d 282 (N.Y. Sup. Ct. 1987).....	13, 36
<i>Markos v. City of Atlanta</i> , 364 F.3d 567 (5th Cir. Tex. 2004).....	7
<i>McLean v. Town of Carolina Beach</i> , 5 Fed. Appx. 304 (4th Cir. N.C. 2001).....	5
<i>Minnesota State Bd. for Community Colleges v. Knight</i> , 465 U.S. 271 (1984).....	10
<i>Pickering v. Bd. of Educ.</i> , 391 U.S. 563, (1968).....	3
<i>Robinson v. Indiana Univ.</i> , 659 N.E. 2d 153 (Ind. Ct. App. 1995)	35
<i>State ex rel. Physicians Comm. for Responsible Med. v. Bd. of Trs. of Ohio State Univ.</i> , 108 Ohio St. 3d 288 (Ohio 2006).....	35
<i>Terrell v. University of Texas Sys. Police</i> , 792 F.2d 1360 (5th Cir. 1986), cert. denied, 479 U.S. 1064 (1987).....	5
<i>University of Pa. v. EEOC</i> , 493 U.S. 182 (1990)	12
<i>Urofsky v. Gilmore</i> , 216 F.3d 401 (4 th Cir. 2000).....	passim
<i>Walker v. Ohio St. Univ. Bd. of Trs.</i> , 2010 Ohio App. LEXIS 297 (2010).....	36
<i>Washington Research Project, Inc. v. Department of Health, Education & Welfare</i> , 504 F.2d 238 (D.C. Cir. 1974)	14
<i>Wilson v. Univ. of Tex. Health Ctr.</i> , 973 F.2d 1263 (5th Cir. Tex. 1992)	7
<i>Zedner v. United States</i> 547 U.S. 489 (2006)	34

Statutes

Va. Code § 2.2-3700(B).....	2, 27
Va. Code § 2.2-3701	1, 25, 36
Va. Code § 2.2-3705.6(17)	28
Va. Code § 2.2-3705.6(3)	13
Va. Code § 2.2-3713(E).....	1, 28, 35
Va. Code § 2.2-3801	1, 39, 40
Va. Code § 2.2-3705.6	1, 35

Virginia Rules of Evidence

Rule 2.602.....	2
Rule 2:1102.....	2
Rule 2:702.....	2

Treatises

J. Peter Byrne, *Academic Freedom: A "Special Concern of the First Amendment"*, 99 Yale L.J. 251, 269 (1989)..... 9

Richard Hofstadter & Walter P. Metzger, *The Development of Academic Freedom in the United States*, 386-87 (1955)..... 9

Sinclair & Middleditch, § 12.3 11

W. Stuart Stuller, *High School Academic Freedom: The Evolution of a Fish Out of Water*, 77 Neb. L. Rev. 301, 302 (1998) 9

I. Introduction

In compliance with the Court's Order of May 11, 2012, Petitioners submit this memorandum in response to Respondent and Intervenor memorandum in opposition to Petitioners' verified petition for mandamus and injunctive relief, wherein Petitioners sought *in camera* review to determine whether the Respondents have properly withheld emails sought under the Virginia Freedom of Information Act and our plea for release of improperly withheld emails. The Court ordered cross briefs, cross responses and cross replies in this matter. Herein we cite to the initial cross briefs as follows: to Petitioners' Memorandum of Facts and Law as "Pet. Memo"; to Respondents' Joint Memorandum in Opposition to Petitioners' Verified Petition for Mandamus and Injunctive Relief as "Res. Memo"; and to the American Association of University Professors ("AAUP") *amicus* brief as "AAUP Memo."

The instruction of the Court to offer a full and complete record for potential appeal compelled the parties to submit lengthy initial memoranda and we acknowledge that to adequately respond to the University's arguments this Response must be so as well. We open by giving brief attention to the standard of evidence the Rules and the Virginia Freedom of Information Act (VFOIA) impose on the Court and parties, specific standards to which we did not fully attend to in our initial brief. Thereafter we take up the First Amendment issues raised by the University, by Michael Mann and by AAUP. The latter half of the memorandum offers our response to the University's arguments regarding application of VFOIA to the exemplars before the Court.

II. Standard of Evidence

Under VFOIA, the records custodian seeking to withhold documents has the burden to justify that withholding. Va. Code § 2.2-3713(E). In carrying that burden, “[a]ny exemption from public access to records or meetings shall be narrowly construed.” Va. Code § 2.2-3700(B). We also ask the Court to give careful attention to three rules of evidence which, when applied to the instant case, eliminate from the Court’s consideration the speculation and hearsay upon which the University and AAUP base the gravamen of their arguments.

Specifically, under Rule 2:1102, “evidentiary rules apply generally to (1) all civil actions,” including this one. Under Rule 2.602, “a witness may not testify to a matter unless evidence is introduced sufficient to support a finding that the witness has personal knowledge of the matter.” And under Rule 2:702 (b) “Expert testimony may include opinions of the witness established with a reasonable degree of probability, or it may address empirical data from which such probability may be established in the mind of the finder of fact. *Testimony that is speculative is not admissible*” (*emphasis added*).

Petitioners repeatedly cite, *infra*, to allegations and speculation offered by the University unsupported by admissible evidence. Among the most significant unsupported allegations of the University is that release of the emails would cause a chilling effect. In fact, the only admissible evidence shows that similar releases have not done so, indicating to the Court that release of the emails at issue here also would not.

The University parades eminences before the Court who in turn speculate as to a Parade of Horribles likely to destroy science, the University and its standing. Amongst these eminences is but one single person who offers first-hand information on the nature of the research process, and after careful scrutiny, he shows himself to be a commercial competitor who places his own

self-interest ahead of the expansion of knowledge. Thus, recognizing also that affidavits are for attestation not speculation, we ask the Court to recognize the overwhelming content of mere supposition in both the UVA and AAUP memoranda and we point to much of it herein.

III. There is no First Amendment public speech at issue

The University and AAUP argue that the public's right to know must be balanced against the obligation to protect and preserve a scientist's ability to freely conduct research and correspond with other researchers. AAUP Memo at 10. Neither the Respondent nor the *amicus*, however, offer any authority for this statement and controlling authority from *Pickering* holds the opposite. The University and AAUP base their entire argument on the presumption that speech about research is protected by academic freedom, which in turn they argue is protected by the First Amendment, thus begging this Court to conduct a balancing test of the respective, allegedly constitutionally protected interests. But, where the research speech is government speech, no balancing is appropriate.

We discussed this issue regarding the First Amendment and government speech in our opening memorandum (Pet. Memo at 34) but further refine the issue here, beginning with the controlling Fourth Circuit and Supreme Court decisions.

In *Urofsky*, the Fourth Circuit examined whether Virginia could restrict a faculty member's access to internet pornography. Urofsky asked that court to balance a government restriction against a faculty member's research speech; specifically, speech asking students to examine pornography for purposes of their course of study. Before conducting this balancing, the Court held that it must first determine "whether the speech at issue is that of a private citizen speaking on a matter of public concern." *Urofsky v. Gilmore*, 216 F.3d 401,406 (4th Cir. 2000).

The *Urofsky* Court held that if no public speech is before the court, there is no First Amendment issue before the court.

Urofsky, follows *Pickering v. Bd. of Educ.*, 391 U.S. 563, (1968), as commonly cited in government speech cases, including routine citation by Virginia federal courts:

In emphasizing the difference between speech made as a public citizen versus speech made as a public employee, the Court . . . when an employee is “simply performing his or her job duties,” courts need not perform “the delicate balancing of the competing interests surrounding the speech and its consequences.”

See, German v. Fox, 2007 U.S. Dist. LEXIS 30895 at *16-17 (W.D. Va. Apr. 26, 2007) (*quoting Pickering*). Because there is no public speech in the emails now before this Court, the *Pickering/Urofsky* precedent thrusts a stake through the heart of the University’s and AAUP’s academic freedom demand for balancing; and thus, this precedent deserves, and we offer, additional clear exposition.

“Speech by a public employee made in the employee’s official capacity is government speech, not public speech, and thus not accorded First Amendment protections.” *Urofsky v. Gilmore*, 216 F.3d at 407. In making this holding, the *Urofsky* court cites to *DiMeglio*, “the [Supreme] Court [has] distinguished between speaking as a citizen and as an employee, and [has] focused on speech as a citizen as that for which constitutional protection is afforded.” *DiMeglio v. Haines*, 45 F.3d 790, 805 (4th Cir. 1995). For authority on this controlling Fourth Circuit precedent, *DiMeglio* cites to *Connick v. Myers*, 461 U.S. 138, 143, (1983), and *Pickering*. . The Supreme Court has more recently reiterated this rule of law. *See, Garcetti v. Ceballos*, 547 U.S. 410, 418 (U.S. 2006) (“*Pickering* and the cases decided in its wake identify two inquiries to guide interpretation of the constitutional protections accorded to public employee speech. The first requires determining whether the employee spoke as a citizen on a matter of public concern.

If the answer is no, the employee has no First Amendment cause of action.”(internal citations omitted) (emphasis added).

The Fifth Circuit has examined this issue at length and offers additional helpful analysis on application of the *Connick* rule.

Because almost anything that occurs within a public agency could be of concern to the public, we do not focus on the inherent interest or importance of the matters discussed by the employee. Rather, our task is to decide whether the speech at issue in a particular case ***was made primarily in the plaintiff's role as citizen or primarily in his role as employee.*** In making this determination, the mere fact that the topic of the employee's speech was one in which the public might or would have had a great interest is of little moment.

Terrell v. University of Texas Sys. Police, 792 F.2d 1360 (5th Cir. 1986), cert. denied, 479 U.S. 1064, 93 L. Ed. 2d 997, 107 S. Ct. 948 (1987) (emphasis added).

The *DiMeglio* citizen-government employee distinction is a firm Fourth Circuit precedent. *See, Holland v. Rimmer*, 25 F.3d 1251, 1255-56 (4th Cir. 1994) (finding communications made "in the course of carrying out [plaintiff's] legitimate job duties" and "between employees speaking as employees" were not of public concern); *German v. Fox*, 267 Fed. Appx. 231, 232 (4th Cir. Va. 2008) (“Relying on *Garcetti v. Ceballos*, 547 U.S. 410, 126 S. Ct. 1951, 164 L. Ed. 2d 689 (2006), the district court concluded that German's **emails** were not protected by the First Amendment since the emails were sent pursuant to his official duties as the Director of Public Relations and Membership for the SVTA, and not in his capacity as a private citizen. This appeal followed and we affirm.”); *Bevis v. Bethune*, 232 Fed. Appx. 212 (4th Cir. 2007) (“To determine whether a public employee's speech is constitutionally protected, we must determine, as a threshold matter, whether the expressions in question were made by the speaker ‘as a citizen upon matters of public concern.’ *Garcetti v. Ceballos*, 126 S. Ct. 1951, 1956, 164 L. Ed. 2d 689 (2006) (internal quotation marks omitted) (holding that public employees speaking

pursuant to their official duties are not speaking as citizens for First Amendment purposes)” and “If, however, the employee is not speaking as a citizen for First Amendment purposes or comments ‘upon matters only of personal interest,’ *DiMeglio*, 45 F.3d at 805 (internal quotation marks omitted), the Constitution does not insulate his remarks from employer discipline, and our inquiry ends.”); and *McLean v. Town of Carolina Beach*, 5 Fed. Appx. 304, 305 (4th Cir. N.C. 2001) (speech that was not that of a private citizen speaking on a matter of public concern, but that of an employee, is not entitled to First Amendment protection.).

What, then, is protected “speech of a private citizen speaking on a matter of public concern,” as opposed to an academic’s government speech which is not accorded First Amendment protection? In *Adams* the Fourth Circuit notes that this query:

“permits a nuanced consideration of the range of issues that arise in the unique genre of academia. Under that analysis, ‘[t]o determine whether speech involves a matter of public concern, we examine **the content, form, and context of the speech** at issue in light of the entire record.’ *Kirby v. City of Elizabeth City*, 388 F.3d 440, 446 (4th Cir. 2004) (citing *Connick*, 461 U.S. at 147-48). “Speech involves a matter of public concern when it involves an issue of social, political, or other interest to a community.” *Id.* (citing *Connick*, 461 U.S. at 146); *see also City of San Diego v. Roe*, 543 U.S. 77, 83-84 (2004); (observing that “public concern is something that is a subject of legitimate news interest; that is, **a subject of general interest** and of value and concern to the public at the time of publication”).

Adams v. Trs. of the Univ. of N. Carolina-Wilmington, 640 F.3d 550, 565 (4th Cir. N.C. 2011) (*emphasis added*).

Adams provides examples of what would qualify as matters of public concern: “in the unique genre of academia.” These include a college professor's speech on “academic freedom, civil rights, campus culture, sex, feminism, abortion, homosexuality, religion and morality” which “plainly touched on issues of public, rather than private, concern”. *Id.* In contrast, speech on “matters more immediately concerned with the self-interest of the speaker as employee” is not public speech. *Howard v. County of Durham*, 2011 U.S. Dist. LEXIS 62957, 12-13 (E.D.N.C.

June 14, 2011) (*citing to Stroman v. Colleton County Sch. Dist.*, 981 F.2d 152, 156 (4th Cir. 1992))).

Wilson places an additional layer of gloss on this distinction between public and government speech. Citing to the *Connick* holding that “when a public employee speaks *not* as a citizen upon matters of public concern, but *instead* as an employee upon matters only of personal interest,” the *Wilson* Court held “The words “not” and “instead” in this key statement can only mean that the Court removed from First Amendment protection only that speech that is made only as an employee, and left intact protection for speech that is made both as an employee and as a citizen.” *Wilson v. Univ. of Tex. Health Ctr.*, 973 F.2d 1263, 1269 (5th Cir. Tex. 1992).

The Fifth Circuit is applying the Fourth Circuit’s “content, form, and context of the speech” test from *Adams*, and would not convert government speech into public speech if the employee did not make it in his role as a citizen.

One measure of a government employee not speaking as a citizen would be where the form of the speech is speech withheld from the public, not unlike the internal academic speech in the emails at issue in this case. Reflecting on *Terrell*, the Fifth circuit noted that “He made no effort to communicate the contents of the notebook to the public, and the evidence does not suggest that he would have had any occasion to do so.” *See Markos v. City of Atlanta*, 364 F.3d 567, 571 (5th Cir. Tex. 2004). In like measure, and of particular applicability to the instant case, in *German* the district court concluded that German's emails were not protected by the First Amendment since the emails were sent pursuant to his official duties *German v. Fox*, 267 Fed. Appx. 231, 232, a point favorably noted on appeal (*German v. Fox*, 267 Fed. Appx. at 232).

What, then, is the “content, form, and context of the speech” at issue in this case? Did the University or Intervenor intend to make it public or have any occasion to do so? Plainly, no.

Do the emails involve matters immediately concerned with Michael Mann's self-interest?

Surely. Are the emails speech made pursuant to his official duties? Yes, as is obvious on their face, as Michael Mann has admitted and as AAUP has previously, strenuously argued along with many others.

In his affidavits, Michael Mann describes the context of the emails as "internal discussion," "communication with our department chair and his staff," "communication with a professional colleagues" and "an email I sent to myself." Conf. Mann Aff. He often describes the content as "frank discussion" and as addressing proposed research, discussion on draft professional articles, internal reports to the Dean and requests for assistance as an academic. Mann Aff. and Conf. Mann Aff. The form of the speech is a matter Petitioners dealt with in our initial memorandum (Pet. Memo at 36), and it consists of emails from the University's email system to which the public had no access and which were not sent to the public, a form of speech the Courts have not considered public speech. *See, German*, discussed *supra*.

We note in passing, the "form" element of the *Adams* formulation draws in the entire question as to whether the speech is given in a public forum, the normal starting point for a discussion on First Amendment freedom of speech and which we addressed at length in our opening memorandum. *See, Pet. Memo at 36, et seq.* If the speech is not within a public forum, then it is difficult to argue that they were intended to be made public, and nowhere do Mann, AAUP, the University or others claim they were, and if they were not, then they cannot be public speech.

Under the formulations of *Adams, DiMeglio, Connick, Pickering, Garcetti, Stroman, Wilson, Terrell, McLean, Bevis, and Holland*, and according to Intervenor Mann himself as well as an entire chorus of supporters led by AAUP, the emails at issue do not constitute a citizen

speaking on a matter of public concern. Thus, there is no First Amendment protection for them, and under the *Pickering* rule courts need not perform “the delicate balancing of the competing interests surrounding the speech and its consequences.” This disposes of the alleged constitutional question, which courts seek to avoid confronting where possible; here this Court need not venture into unplumbed waters and formulate a new privilege and related balancing act, for, in this case, there is no Constitutional right at issue and no need of a balancing.

IV. There is no Academic Freedom “Scholars Privilege” nor is one desirable

Academic freedom is a term that is often used, but little explained, by federal courts. Courts are remarkably consistent in their unwillingness to give analytical shape to the rhetoric of academic freedom. Lacking definition or guiding principle, the doctrine [of academic freedom] floats in the law, picking up decisions as a hull does barnacles. As a result, decisions invoking academic freedom are lacking in consistency and courts invoke the doctrine in circumstances where it arguably has no application.

Urofsky v. Gilmore, 216 F.3d at 410 (4th Cir. 2000) (*internal citations omitted*). The instant case is no exception to the metaphor described by the *Urofsky* court. Although UVA and AAUP copiously apply the rhetoric of “academic freedom” in their memoranda, neither candidly articulate the basic meaning of the term. As Byrne explains, “Academic freedom as [UVA and AAUP use it] simply had no meaning.” J. Peter Byrne, *Academic Freedom: A “Special Concern of the First Amendment”*, 99 *Yale L.J.* 251, 269 (1989). That is to say, the term is not without definition, but that UVA and AAUP do not apply its actual meaning.

Hofstadter & Metzger’s definitive study of the subject explains that academic freedom embodies the notion that professors should be free to conduct research and publish findings without fear of reproof from *the church or state*. Hofstadter & Metzger further denoted the authority to determine the content of courses and lectures. Richard Hofstadter & Walter P. Metzger, *The Development of Academic Freedom in the United States*, 386-87 (1955) (*emphasis added*). In 1940 the AAUP adopted these concepts, but extended the focus of protection from

the church and state to include protection from “lay administrators and trustees.” *Byrne, op cite* at 273-78 and Metzger at 1275-76. AAUP states this as a right to be free of any interference, if “the conclusions [of their research] are unacceptable to some constituted authority within or beyond the institution.” *See, W. Stuart Stuller, High School Academic Freedom: The Evolution of a Fish Out of Water*, 77 Neb. L. Rev. 301, 302 (1998). Freedom from the intrusion of a constituted authority is more than a far cry from public observation of past academic or other work-related activity using public resources.

“Significantly, the AAUP conceived academic freedom as a professional norm, not a legal one: The AAUP justified academic freedom on the basis of its social utility as a means of advancing the search for truth, rather than its status as a manifestation of *First Amendment* rights. *See Hofstadter & Metzger, supra*, at 398-400; *Byrne, supra*, at 277-78.” *Urofsky v. Gilmore*, 216 F.3d at 411. This remains the gravamen of UVA and AAUP’s academic freedom argument in the instant matter. Both, however, “fail to appreciate that the wisdom of a given practice as a matter of policy does not give the practice constitutional status. *See Minnesota State Bd. for Community Colleges v. Knight*, 465 U.S. 271, 288, (1984) (concluding that “faculty involvement in academic governance has much to recommend it as a matter of academic policy, but it finds no basis in the Constitution”).” *Urofsky v. Gilmore*, 216 F.3d at 411, n. 12.

The distinction between an academic norm and a constitutional right is now settled law in the Fourth Circuit: “Appellees’ insistence that the Act violates their rights of academic freedom amounts to a claim that the academic freedom of professors is not only a professional norm, but also a constitutional right. We disagree.” *Urofsky v. Gilmore*, 216 F.3d at 411. The *Urofsky* court continues in a note further explaining:

the argument raises the specter of a constitutional right enjoyed by only a limited class of citizens. *See David M. Rabban, Functional Analysis of "Individual" and "Institutional"*

Academic Freedom Under the First Amendment, 53 Law & Contemp. Probs. 227, 238 (1990). Indeed, the audacity of Appellees' claim is revealed by its potential impact in this litigation. If Appellees are correct that the *First Amendment* provides special protection to academic speakers, then a professor would be constitutionally entitled to conduct a research project on sexual fetishes while a state-employed psychologist could constitutionally be precluded from accessing the very same materials. Such a result is manifestly at odds with a constitutional system premised on equality.

Id at n.13 In the instant case, a scientist within the Virginia Department of Environmental Quality or the Virginia Department of Conservation and Recreation, each of whom also examines climate change questions, would not be entitled to constitutional protection that a scientist at UVA would enjoy, a manifest inequality the Constitution simply does not offer.

The University and AAUP, ignoring the core principles and purpose of “academic freedom” and their own arguments that this “freedom” is intended to support social utility rather than manifest a legal right, now wish the Court to join them in their ignorance and declare a First Amendment right to free speech which includes a subsidiary right, for a limited class of citizens, to academic freedom in the form of a “Scholar’s privilege.” They have jumped from their own ship, with no rescue available to them in the precedential case law, and now turn to this Court to create a lifeline in the form of a new non-statutory, but constitutional privilege.

A. There is no “Scholars Privilege”

As *Bunch* explains, “where an assertion of privilege is made, the Court must first determine whether there is a privilege. Although the Rules of the Supreme Court of Virginia do not define the law of privilege under Virginia law, there are statutory privileges in Virginia, such as physician-patient, husband-wife, and priest-penitent. There are also non-statutory privileges such as those under the First Amendment to the U.S. Constitution [Press privilege and associational privilege] [and common law privileges such as] the attorney-client privilege, and

the work product privilege.” *Bunch v. Artz*, 71 Va. Cir. 358, 364 (Va. Cir. Ct. 2006) (citing to *Sinclair & Middleditch*, § 12.3).

The Virginia Rules of Evidence, Article V, identifies the privileges recognized at law within the Commonwealth. There are seven. None involve any form of a Scholar’s privilege. The irreducible fact is that there is no Scholars privilege and never has been one. A careful review of the case law on this subject led Judge Wilkens to conclude that the Supreme Court had many opportunities to find or otherwise establish a Scholar’s privilege and in every instance they refused to do so. *Urofsky v. Gilmore*, 216 F.3d at 414.¹ And see *University of Pa. v. EEOC*, 493 U.S. 182, 201 (1990), citing to *Branzburg v. Hayes*, 408 U.S. 665 (1972) and *Herbert v. Lando*, 441 U.S. 153, 174 (1979), where the court “indicated a reluctance to recognize a constitutional privilege” because they “were unwilling then, as we are today, ’to embark the judiciary on a long and difficult journey to . . . an uncertain destination.”

Despite the Court’s refusal to recognize a new Scholar’s privilege, AAUP directs the Court to *U. Penn* for that purpose. This case is one of several cases upon which AAUP relies involving the confidentiality of faculty appraisals and whether those appraisals are protected under a Scholar’s privilege.² AAUP argues that this case established a privilege if disclosure of

¹ *Urofsky* disposes of each of the cases cited by UVA and AAUP in support of their prayer for creation of a new privilege, recognizing that they address different kinds of rights. *And see*, *Urofsky* at 412,

“Moreover, a close examination of the cases indicates that the right praised by the Court is not the right Appellees seek to establish here *See, e.g.*, *Regents of the Univ. of Mich. v. Ewing*, 474 U.S. 214, 226 & n.12, 88 L. Ed. 2d 523, 106 S. Ct. 507 (1985); *Regents of the Univ. of Cal. v. Bakke*, 438 U.S. 265, 312-13, 57 L. Ed. 2d 750, 98 S. Ct. 2733 (1978) (opinion of Powell, J.); *Keyishian v. Board of Regents*, 385 U.S. 589, 603, 17 L. Ed. 2d 629, 87 S. Ct. 675 (1967); *Sweezy v. New Hampshire*, 354 U.S. 234, 250, 1 L. Ed. 2d 1311, 77 S. Ct. 1203 (1957) (plurality opinion); *id. at 261-63* (Frankfurter, J., concurring in the result). Despite these accolades, the Supreme Court has never set aside a state regulation on the basis that it infringed a *First Amendment* right to academic freedom. *Cf. Minnesota State Bd. for Community Colleges v. Knight*, 465 U.S. 271, 287, 79 L. Ed. 2d 299, 104 S. Ct. 1058 (1984).”

² AAUP refers to several other cases that deal with the confidentiality of faculty appraisals, sometimes called “peer review” and which are unrelated to a Scholar’s privilege. Citing to *Corr v. Mazur*, 15 Va. Cir. 184, 188 (Va. Cir. Ct. 1988) and *Adams v. Trs. of the Univ. of N. Carolina-Wilmington*, 640 F.3d 550 (4th Cir. N.C. 2011), AAUP argues the Commonwealth’s courts have recognized the importance of academic freedom. That case made no such

information will direct the content of university discourse toward or away from particular subjects or points of view. AAUP Memo at 2. The case makes no such holding, specifically stating that it “cannot accept the University’s invitation to create a new privilege.” *U. Penn* 493 U.S. at 189 & 198 n.6. AAUP’s reference to this case is to mere dicta and is particularly inappropriate for consideration in the instant case as VFOIA is content neutral and, as discussed below in the section on “chilling” effects, neither UVA nor AAUP offer any evidence that VFOIA has directed any professor to or away from any kind of research. Further, the records at issue in this case do not reveal any confidential information of the kind discussed in *U. Penn*.

Particularly egregious is the claim that “the disclosure of [Mann’s] email correspondence would violate his individual liberty interest under the First Amendment and the principles of academic freedom.” Res. Memo at 44. *Urofsky* simply closed the door on the notion that a faculty member, rather than the university, has such right. The University suggests that *Urofsky* “is entitled to great deference” on this matter. If the University means *Urofsky* is controlling law, they are correct. If they mean anything else, they are simply wrong. Mann has no individual rights greater than any citizen. This is the central holding of *Urofsky* and it controls the deliberations of this and every other Commonwealth court.

Having no constitutional, common law or legislative authority for a Scholar’s privilege, UVA and AAUP ask this Court to recognize a privilege on the basis that it is a good (institutional and/or social) policy. The entirety of their argument is grounded on speculation. They offer no admissible evidence to support their proposal and the only evidence relevant to such a policy cuts the other way.

statement and its decision did not rest on any concern with chilling academic freedom through release of documents under FOIA.

B. The creative period is protected under Exemption 4.

Citing to their affidavits and case law such as *In re R.J. Reynolds Tobacco Co.*, 136 Misc. 2d 282, 286-87 (N.Y. Sup. Ct. 1987), the University and AAUP misdirect the court to the Research Period as being unprotected. As discussed, *infra*, Petitioners believe this period reflects the only time when faculty records constitute competitive commercial interests which Exemption 4 protects.

C. Confidential information is protected under Exemption 6

Throughout their memoranda AAUP and the University mix in concerns about confidential information that the University would be forced to release if this Court held for the Petitioners. These concerns are without foundation. VFOIA protects confidential information provided “pursuant to a promise of confidentiality.” *See*, Va. Code § 2.2-3705.6(3). AAUP cites to *In re Philip Morris*, 706 So. 2d 665 (La.App. 4 Cir. Jan. 28, 1998) as one example of this concern, showing the court there refused to release confidential information. Because VFOIA Exemption 6 protects such information, AAUP offers up an empty concern unrelated to the instant case. More significantly, the University has offered no admissible evidence of any confidentiality agreements covering any of the exemplars, including the peer reviews.

D. The marketplace of ideas is not fragile and has not been chilled

In making their pitch for a Scholar’s privilege, the University and AAUP argue that the research process is fragile and in need of protection. They offer repeated concerns that public exposure of some portion of this process will chill it and reduce its quality. They offer no evidence of this, and what evidence there is suggests that the industry of academic research is marked as much by concern for career advancement and attendant professional prestige and material rewards of the academic and scientific market than in the advancement of science or the

creation of new knowledge. *See, Washington Research Project, Inc. v. Department of Health, Education & Welfare*, 504 F.2d 238, 244-245 (D.C. Cir. 1974) (Petitioners note the expressed view of the U.S. government that “biomedical researchers are really a mean-spirited lot who pursue self-interest as ruthlessly as the Barbary pirates did in their own chosen field.”).

The parade of eminences’ affidavits, constituted of impermissible speculation as opposed to the acceptable attestation, contains no specific examples of a chilling effect from transparency and in particular that the transparency which occurs after completion and publication of research. They offer only speculation and opinion based on speculation, neither of which is admissible evidence in Virginia. The University offers no specific evidence that anyone has ever experienced a chilling effect from a Freedom of Information Request or other mandated transparency to which they agreed as a condition of their comfortable public employment.

AAUP offers two specific claims of a chilling effect, neither of which actually supports their claim. Citing to written testimony submitted to Congress, but not offered under oath, AAUP suggests that Benjamin Santer, an employee at a Federal laboratory has “experienced interference” with his ability to perform scientific research due, in part, to Freedom of Information Act requests. AAUP Memo at 17. Left unstated were Santer’s public claims indicating this scrutiny is a good thing and the best defense is exposure of the work³ Also left unmentioned was Santer’s improprieties that prompted scrutiny⁴

³ *See*, “Lab News”, Anne M. Stark, “DDLS on climate science’s charged atmosphere”, Lawrence Livermore National Laboratory Community News, Vol. 3, No. 2, January 15, 2010, <https://newsline.llnl.gov/rev02/articles/2010/jan/01.15.10-ddls.php>(emphases added).

⁴ Santer was a principal in the Climategate emails and his actions had been the subject of scrutiny since 1996 when the past president of the National Academy of Sciences, Dr. Frederick Seitz, detailed certain of Santer’s behavior that was uncovered, as a lead author of the United Nations Intergovernmental Panel on Climate Change (IPCC). *See*, Frederick Seitz, “A Major Deception on Global Warming,” *Wall Street Journal*, June 12, 1996, available at, e.g., <http://thedgw.org/definitionsOut/..%5Cdocs%5CSeitz%20-%20A%20Major%20Deception%20on%20Global%20Warming.pdf>.

Examination of the Santer testimony tells a different story than that portrayed by AAUP. Before addressing the content of the testimony, we note that it is hearsay and is not admissible as evidence in this case. Because Petitioners had no opportunity for discovery in this matter, we could not cross-examine Santer and could not elicit testimony under oath as to his allegations not made under oath.

With regard to the content of the testimony, the text quoted from his statement comes under a section Santer titles “Personal Thoughts on Harassment of Climate Scientists.” Santer Testimony p. 18, available at

<http://globalwarming.markey.house.gov/files/HRG/052010SciencePolicy/santer.pdf>.

This section follows the “Conclusion” to his formal testimony and represents nothing more than a bald statement without any supporting testimony on who interfered, how they interfered and what work could not be done because of the interference.

Santer claims the interference began “immediately after publication of the second [IPCC] assessment report in 1996.” *Id.* It is unclear what Santer was unable to do because of the alleged interference, but his curriculum vitae demonstrates that he had a continuing high-volume stream of work after publication of the 1996 IPCC report, including 30 papers, reports and books published between 1996 and 2004. *See*, Santer vita.⁵ Considering his additional work as a journal editor and the many committees, councils and commissions on which he served, this level of workload does not suggest any “chilling” interference whatever.

The second piece of evidence AAUP offers is the Kempner paper.⁶ AAUP Memo at 17.

⁵ Available at:

<http://www.google.com/url?sa=t&rct=j&q=&esrc=s&frm=1&source=web&cd=3&ved=0CGgQFjAC&url=http%3A%2F%2Fstat-www.berkeley.edu%2Fusers%2Fbickel%2FNeyman%2Fsantercv.doc&ei=OxocUKumH6eR6wH-ooGoAQ&usg=AFQjCNHUK3Lc1rhYCKceaTHhUblaed3Gg&sig2=ZlhIR8CrxvIrh4pHFDCV7Q> (accessed 8/9/2012).

⁶ AAUP also mentions the Levinson-Waldman paper (AAUP Memo at 17), but review of that paper does no more than refer back to Kempner.

The Kempner paper offers two pieces of information, one speculation and the other admissible evidence. AAUP offers the Court the speculation. We now offer the evidence.

The Kempner paper reports on a survey of scientists engaged on HIV or sex-related research, at the time a subject of high interest to Congress and on which several of the scientists were required to testify. Kempner, J, “*The Chilling Effect: How Do Researchers React to Controversy?*” PLoS Med. 2008 November; 5(11) 1571- 1577: e222, (2008) (available at <http://www.ncbi.nlm.nih.gov/pmc/articles/PMC2586361/>). Even though a peer-reviewed paper, it suffers from major problems and is not sufficient to pass muster under the Federal Data Quality Act. *See*, Section 515 of the Consolidated Appropriations Act, 2001 (Pub.L. 106-554).

Among its failings are lack of any testable hypotheses; a complete absence of confidence intervals for the outcomes, as needed to determine whether, at a 95 percent confidence interval, the study conclusions are expected to be true or false; a low response rate to the survey; and, perhaps most worrisome, that the survey responses came only from a self-selected sample. This last bias, alone, completely impeaches the study because Kempner made no effort to determine what biases self-selection introduced. Indeed, Kempner admits to this problem. Kempner at 1576.

AAUP latches onto the Kempner results showing 71 percent of respondents agreed with the statement that “this political controversy created a ‘chilling effect’ in research, dissuading scientists from studying controversial research.” *Id* at 1574. These responses, of course, are no more than “attitudes” and constitute nothing other than speculation. More significant is the non-speculative finding that 79 percent of the respondents agreed with the statement “No amount of political controversy could dissuade me from conducting HIV or sex-related research.” *Id*.

In simpler terms, Kempner shows that academics will claim transparency will chill the

research of “the other guy” but will not affect themselves. This is consistent with Santer’s public statements cited, *supra*, that, at least to him, scrutiny is vindicating. Notably, the former is speculation, the latter hearsay, even if less speculative, and none of it reflects statistically proven scholarship.

Petitioners have already offered evidence that releases of Intervenor Mann’s emails have had no chilling effect on his research or his use of email in that research. *See*, Pet. Memo at 40. Nor has Michael Mann offered any such evidence of a chilling impact on himself. His affidavit makes general, unsupported non-specific allegations of a chilling effect, but gives no actual evidence whatever. At most, he alleges “several scientists” did not wish to be named in his affidavit for fear that they will be harassed. Mann Aff. at ¶ 49. Because the Court did not permit Petitioners the opportunity to depose Michael Mann, we have no means to test these kinds of allegations, no way to elicit the specifics of any chilling effect he alleges. Because Mann himself refuses to offer specifics, and because these allegations by others are mere hearsay, they do not constitute admissible evidence and certainly do not meet the burden of showing by a fair preponderance of the evidence that there is any chilling effect whatever.

The same is true for Mann’s allegations that he has to use the telephone instead of email, or that there has been adverse impact on obtaining grants. Indeed, Mann continues to be a grant-magnet and supports not only himself but many others at his institution. Indeed, since the November 2009 ClimateGate email release that painted him as repeatedly failing to meet the norms of academia, he has received six major grants totaling \$3,055,959 – a massive level of support and surely not one reflective of a chilling effect. *See*, Michael Mann *Curriculum Vitae* 2012 at <http://www.meteo.psu.edu/~mann/Mann/about/cv.php> (accessed 8.9.2012).

Petitioners offer additional evidence that UVA's concerns are illusory. George Mason University had no qualms about releasing emails of one of its professors, after publication of research. In fact, ATI has received emails in response to similar and related freedom of information requests from Texas A&M, Texas Tech and the University of Arizona over the past year. None have refused to provide emails on the basis that this would chill their research or otherwise compromise any academic freedom.

UVA, even with assistance from AAUP, has not carried its burden of proof. Without more, the preponderance of admissible evidence suggests no chilling effect has ever occurred from the transparency offered under freedom of information act requests.

E. Confidential peer review does not eliminate the value of public reviews

The University alleges that when a faculty member is comfortable enough with his or her research to compile it in a publishable format, the work is put through the rigorous peer review process, during which the scholar's research, data, assumptions, and conclusions are subject to scrutiny and criticism. The scientific community collectively assesses whether the scholar's conclusions rest on solid ground. (Res. Memo at 21.) This, the University claims, is sufficient for scientific and policy purposes and thus there is no need for inquiry into the scientific process by the public. Dean Gittleman goes further, suggesting that "No scientist would claim a right to withhold data or research results described in a published article. We expect to be questioned on and to be accountable for what we elect to publish and present as truth to the broader community." Res. Memo at 30. Putting aside for the moment the infamous February 21, 2005 email to researcher Warwick Hughes by Phil Jones, the star of Climategate -- "Why should I give information to you when all you want to do is find something wrong with it" -- Petitioners

respectfully suggest Dean Gittleman wake up and smell the coffee. This “norm” is not normal and in the instant case the University’s allegation is completely false.

The true “norm” is to keep data and methods hidden from the public and especially those who seek to critically evaluate submitted or published work. In an email Mann sent while at UVA he specifically stated:

I know I probably don't need to mention this, but just to insure absolutely clarify (sic) on this, I'm providing these for your own personal use, since you're a trusted colleague. So please don't pass this along to others without checking w/ me first. This is the sort of "dirty laundry" one doesn't want to fall into the hands of those who might potentially try to distort things.

See Pet. Response Exhibit 1. This private stance hardly resembles the public posture of Dean Gittleman or, *e.g.*, Ben Santer cited *supra*.

The information Mann wished to keep secret are statistical data developed during preparation of papers published in 1998 and 1999. *See* Pet. Response Exhibit 2. Independent scientists sought the information to evaluate Mann’s statistical approach – an approach later found to be inappropriate for use in the manner Mann used it, thus impeaching the infamous hockey stick graph used to force significant legislative and regulatory programs in the United States and throughout the world.⁷

The secrecy “norm” extended beyond UVA to the National Science Foundation who also refused to produce the information they had paid Mann to create, using taxpayer funds. *See*, Pet. Response Exhibit 3.

⁷ The significance of the independent scientific analysis that Mann did not want to allow was made manifest when the U.S. Congress commissioned a prominent statistical expert, former chairman of the Committee on Applied and Theoretical Statistics of the National of Sciences, Dr. Edward Wegman of George Mason University, to review Mann’s work in light of the independent analysis. His report excoriated the Mann statistical approach and validated the independent analysis that was possible only because of FOIA. Wegman also examined and excoriated what is known as “pal review,” something Mann routinely used, despite its violation of academic “norms.” Notably, as discussed herein, Wegman himself had to perform under VFOIA, something he did fully and expeditiously. We also note that when the VFOIA request for Wegman’s emails was made public, there was a deafening silence from the current anti-FOI chorus who, saw no harassment or violating academic freedom in that request and production.

Indeed, the peer review process does not live up to this alleged openness “norm”, much less the allegation that the scholar's research, data, assumptions, and conclusions are subject to scrutiny and criticism as part of a “rigorous” peer review. Petitioners’ Exemplar 9 gives a clear picture of how Intervenor Mann conducted his peer review, stating his review is “based on admittedly only skimming the paper.” *See* Pet. Exemplar PE-9. Nor is Mann alone in this peer review minimalism.

In 2004, Mann had submitted a manuscript to *Climate Change*. A reviewer, questioning the statistical analysis used in preparation of the manuscript, asked the journal editor for the computer code Mann used so that he could determine the assumptions applied in Mann’s analysis. The Journal Editor-in-Chief responded, writing “I have run the issue by the full Climatic (sic) Change Editorial Board since a source code request by a reviewer is unprecedented in the 28 years since I founded the journal.” *See*, Pet. Response Ex. 4.⁸ The journal refused to grant the reviewer’s entirely reasonable request.

Nor is *Climate Change* apparently the only journal that does not routinely provide the information necessary to duplicate exactly the science they publish.

I checked with other editors as well, and in main the atmospheric sciences and climatology journals (and Science, Nature and PNAS), to the extent I can glean, there are no policies requiring source codes to be made available--except it happens sometimes by the voluntary action of authors.

⁸ Petitioners’ Response Exhibit 4 is a very lengthy correspondence chain. We include it in full as it shows not only the superficial correspondence, but the internal journal correspondence of editors who refuse to release to reviewers the information necessary to conduct a proper peer review. It is a complete case study in how journals routinely violate the “norm” and protect their pals (hence the term “pal review” in place of “peer review”). Within the exhibit, for example, is this “inside the hockey stick team” correspondence from Phil Jones at the Climate Research Unit in East Anglia University to Michael Mann:

“Mike, This is for YOURS EYES ONLY. Delete after reading - please ! I’m trying to redress the balance. One reply from Pfister said you should make all available !! Pot calling the kettle black - Christian doesn't make his methods available. I replied to the wrong Christian message so you don't get to see what he said. Probably best. Told Steve separately and to get more advice from a few others as well as Kluwer and legal.

PLEASE DELETE - just for you, not even Ray and Malcolm

Pet. Response Ex. 4 at p. 24. Indeed, if openness were the norm, Professor Boulton would not have needed to prepare a lengthy report to the Royal Society explaining the pathologies of current scientific secrecy and the “need to be open towards fellow citizens.” Pet. Memo at 47.

Nor can the Court expect peer review to routinely function well enough to prevent publication of spurious work, especially in the nascent field of climate change science. Alternatively, public review of all basic data and assumptions has been shown to offer significant utility, making transparency of the research process essential and arguing heavily against any form of Scholar’s privilege. Petitioners offer two examples of how transparency and public review can better serve than, or at minimum complement an admittedly flawed practice of peer-review.

This month, with great fanfare, NASA scientist James Hansen and associates released a paper "The Perception of Climate Change" that claims that recent heat waves and droughts were caused by human-induced climate change. To quote their abstract:

It follows that we can state, with a high degree of confidence, that extreme anomalies such as those in Texas and Oklahoma in 2011 and Moscow in 2010 were a consequence of global warming because their likelihood in the absence of global warming was exceedingly small.

Hansen, J., Sato, M and Ruedy R., “Perception of climate change” *Proceedings of the National Academy of Sciences*, at <http://www.pnas.org/content/early/2012/07/30/1205276109>. The NAS Proceedings conducted some version of peer-review prior to publication of the Hansen paper and is the house journal of the most prestigious scientific organization in the United States.

In a single week this paper has been quoted in hundreds of media outlets and newspapers and has garnered the praise of many environmental advocacy groups. In that same week, Clifford F. Mass, Professor of Atmospheric Sciences at the University of Washington, wrote in his public blog that Hansen’s “conclusions are demonstrably false and their characterization of the science

and statistics are deceptive at best,” citing to information publicly available. *See*, Mass, C. “Climate Distortion” at <http://cliffmass.blogspot.com/2012/08/climate-distortion.html>. Professor Mass, we note for whatever it is worth in these times, confesses he falls into the “warmist”, as opposed to “skeptic”, camp, and describes himself as believing “that human-induced global warming is both observed, real, and a serious problem for mankind.” *Id.* His deconstruction of Hansen’s paper is not alone. A platoon of scientists and engineers have identified fatal flaws in the paper, something that should have been accomplished during the peer review. *See*, Watts, A. “NASA’s James Hansen’s big cherry pick” at <http://wattsupwiththat.com/2012/08/06/nasas-james-hansens-big-cherry-pick/>. ; *and see*, NOAA’s Martin Hoerling, “‘This isn’t a serious science paper,’ Dr. Hoerling said. ‘It’s mainly about perception, as indicated by the paper’s title. Perception is not a science’”, Gillis, J., “Study Finds More of Earth Is Hotter and Says Global Warming Is at Work”, *New York Times*, August 6, 2012, <http://www.nytimes.com/2012/08/07/science/earth/extreme-heat-is-covering-more-of-the-earth-a-study-says.html> (Hoerling, for whatever it is worth, also states he is not a “skeptic” but merely, like Mass, highly critical of such publications written “from passion and not reason” posing and treated as “science”). Indeed it is now more often educated members of the public who puncture peer-reviewed science, than it is members of the class dependent on publicly funded science, and often this occurs with the assistance of FOI laws, including the United Kingdom’s. England’s A.W. Montford is a chemist by training and currently serves as a science writer and editorial assistant to science authors.⁹ He authored *The Hockey Stick Illusion* (Stacey International 2010), a book-length demolition of the veracity of Michael Mann’s research, uncovering the misrepresentations of data behind Mann’s temperature reconstruction

⁹ *See*, http://www.amazon.com/Hockey-Stick-Illusion-Climategate-Independent/dp/1906768358/ref=sr_1_1?s=books&ie=UTF8&qid=1344541865&sr=1-1.

and the tireless scrutiny by scientists and engineers who forced Mann to disgorge his data and research methods – work that reflected the exact time period over which the emails in the instant case were written. Only through information requests outside the peer review process was it possible to force release of the data needed to fully examine Mann’s early and influential work. Without access to that information Mann’s reconstructions would never have been challenged. In this case, **a Scholar’s privilege would have completely frustrated the only serious efforts ever made to conduct an honest, thorough evaluation of Mann’s temperature reconstruction work.** Because there was no such privilege it was possible for a non-academic to publish peer-reviewed papers that significantly contributed to this scientific issue. See, McIntyre, Stephen; McKittrick, Ross (2003). “*Corrections to the Mann et. al. (1998) Proxy Data Base and Northern Hemispheric Average Temperature Series,*” Energy & Environment 14 (6) and see, McIntyre, S.; McKittrick, R. (2005). “*Hockey sticks, principal components, and spurious significance*”. Geophysical Research Letters 32 (3).

In Petitioners’ opening memorandum we discussed two additional elements of peer review that impeach the University’s allegations, and so only mention them briefly here in response to the University’s initial argument. UVA alleges that “The confidentiality expectation of peer reviewers is considered a key element of the peer review process's ethical code.” Res. at 23. As we explained, it is neither necessary nor a universal norm. Pet. Memo at 47. Finally, neither the University nor Michael Mann has offered any evidence showing that the email exemplars that include peer reviews were covered by a confidentiality agreement, much less providing such an agreement itself. Absent that, there can be no presumption of confidentiality.

F. Mann has never been exonerated

The University and Michael Mann make an additional claim that the public does not need to see behind the ivy covered walls as each time Mann has been attacked he has been exonerated. *See*, Mann Aff. at ¶ 38. This is a regular claim of Mann, his lawyers and his supporters. Although this Court has made clear that this allegation is irrelevant to applying VFOIA, Mann and the University, among others, continue to rely upon it as somehow undermining the applicability of the law to him. It is also, on its face, untrue. Exoneration requires investigation; investigation requires pursuit aimed at discovering material facts. Mann's employer since 2005, Penn State University, has conducted no such thing. Neither has the University of Virginia.

To claim that either the Muir Russell or Oxburgh inquiries into UK taxpayer-funded operations at the University of East Anglia's Climatic Research Unit (CRU) were empowered to investigate U.S. citizen Mann is without support. They respectively inquired into "aspects of the **behaviour** of the CRU scientists" [emphasis in original], "allegations about CRU's impact on climate science" and "to understand the significance of the roles played by those involved from CRU" (*see*, "The Independent Climate Change E-mails Review", <http://www.cce-review.org/pdf/FINAL%20REPORT.pdf>); and "to assess the integrity of the research published by the Climatic Research Unit in the light of various external assertions" *See*, "Report of the International Panel set up by the University of East Anglia to examine the research of the Climatic Research Unit", <http://www.uea.ac.uk/mac/comm/media/press/CRUstatements/SAP>). Mann is not and was not with CRU, was not party to those investigations, and his role in Muir Russell was limited to submitting comments, like 110 other individuals seeking to influence matters, despite, according to Muir Russell, authoring the second-greatest number of relevant

emails. As regards Oxburgh, Mann's name does not even appear in that report purportedly "exonerating" him.

These facts nicely frame the weight to be granted the blurred rhetorical hand-waving of Mann's supposed exoneration by myriad inquiries, which nonetheless remains irrelevant to the instant matter. However, further irony is found in the fact that a UK FOI request helped uncover how the Oxburgh panel operated to cover over dissenting opinion in the ranks. *See, e.g.,* Delingpole, J. "How Lord Oxburgh of Persil washed the Climategate team whiter than white (pt 2)", *Telegraph* (UK), June 24, 2010, <http://blogs.telegraph.co.uk/news/jamesdelingpole/100044687/how-lord-oxburgh-of-persil-washed-the-climategate-team-whiter-than-white-pt-2/>.

Inquiries of Mann could be performed by either Penn. State, where he worked when the Climategate leaks occurred, or by the University of Virginia where he worked when first organizing against researchers who were undermining his claims. Otherwise-sympathetic Clive Crook in *The Atlantic* elegantly devastated PSU's contortions, a demonstration piquantly summarized with the conclusion they "would be difficult to parody". *See, "Closeout Memorandum"*, <http://www.theatlantic.com/politics/archive/2010/07/climategate-and-the-big-green-lie/59709/>.¹⁰ In addition, documents exist in which a principal in PSU's effort

¹⁰ In the same analysis Crook styles the Muir Russell effort as being "equally probing" as Penn State's. "Further 'vindication' of the Climategate emailers was to follow, of course, in [Muir Russell's equally probing investigation](#). To be fair, Russell manages to issue a criticism or two. He says the scientists were sometimes "misleading" -- but without meaning to be (a plea which, in the case of the 'trick to hide the decline', is an insult to one's intelligence). On the apparent conspiracy to subvert peer review, it found that the 'allegations cannot be upheld' -- but, as the impressively even-handed [Fred Pearce](#) of the *Guardian* notes, this was partly on the grounds that 'the roles of CRU scientists and others could not be distinguished from those of colleagues. There was "team responsibility".' Edward Acton, vice-chancellor of the university which houses CRU, calls this 'exoneration'."

acknowledges it was orchestrated from behind the scenes to avoid certain people being asked certain things, presumably because that would make the desired outcome impossible.¹¹

Further, Petitioners note that, subsequent to Penn State's report, a U.S. Department of Commerce Inspector General happened to interview Eugene Wahl in the context of federal government involvement in Climategate.¹² Wahl was one of Mann's correspondents on the subject of hiding or destroying records, to whom Mann forwarded Phil Jones's (UEA) request that Wahl do so. The IG report confirming this latter fact affirms that Penn State's unexplained decision to not interview Wahl further made a mockery of its supposed inquiry into whether Mann "engage[d] in, or participate[d] in, directly or indirectly, any actions with the intent to delete, conceal or otherwise destroy emails, information and/or data, related to [IPCC] AR4, as suggested by Phil Jones". *See*, "RA-1O Final Investigation Report Involving Dr. Michael E, Mann", http://live.psu.edu/pdf/Final_Investigation_Report.pdf.

Because Petitioners were not given the opportunity to depose Michael Mann, we are unaware what if any knowledge of any of this Mann had at the time or since, though we do know that PSU's effort oddly did not meet the same uproar organized against other efforts to scrutinize the record, for example our VFOIA request. Unlike PSU's proclaimed instigative tribunal, a simple VFOIA request presents no ability to sanction Mann, but only threatens the transparency Mann agreed to as a condition of his employment at UVa. Yet announcement of what proved to be a risibly inept PSU effort if one nominally with teeth was met with silence and, we are told,

¹¹ See McIntyre, S., "New Information on the Penn State Inquiry Committee", ClimateAudit.org, November 15, 2011, <http://climateaudit.org/2011/11/15/new-information-at-penn-state/>; Also described in: Horner, C., "The Liberal War on Transparency: Confessions of a Freedom of Information "Criminal", Threshold Editions (Release date October 2, 2012).

¹² *See*, Department of Commerce Inspector General, "[Examination of issues related to internet posting of emails from Climatic Research Unit](http://www.oig.doc.gov/Pages/Response-to-Sen.-James-Inhofe's-Request-to-OIG-to-Examine-Issues-Related-to-Internet-Posting-of-Email-Exchanges-Taken-from-.aspx)," alternately styled "Response to Sen. James Inhofe's Request to OIG to Examine Issues Related to Internet Posting of Email Exchanges Taken from the Climatic Research Unit of the University of East Anglia, UK," February 18, 2011, p. 5, available from <http://www.oig.doc.gov/Pages/Response-to-Sen.-James-Inhofe's-Request-to-OIG-to-Examine-Issues-Related-to-Internet-Posting-of-Email-Exchanges-Taken-from-.aspx>.

complete cooperation including turning over all requested records. It is somewhat understated to note this is inconsistent behavior.¹³

The National Science Foundation purported to inquire, as well, but worked from what PSU provided it. See <http://www.nsf.gov/oig/search/A09120086.pdf>. This undermines Mann's claims in this regard *ab initio*.

Beyond these two lay a special silence. The dog not barking in the rhetorical hand-waving about supposed exoneration is the University of Virginia. Not once has UVA argued that it looked into Mann's activities occurring on UVA's watch. In fact, the University apparently was quite deliberate to not conduct an inquiry. Petitioners are informed that UVA's Board of Visitors suggested the school get to the bottom of what transpired on Grounds, only to be rebuffed. The argument, we have been told, is that the school could not guarantee that the findings would not be made public and as such it could not risk an investigation. Petitioners wished to depose the University on this matter but were denied the opportunity to confirm this. Having faith in our information, we are confident that the University cannot dispute this Board of Visitors discussion.

Regardless, to the extent the University and Mann continue to push this as somehow relevant to application of VFOIA, what we know is that Mann has never been seriously investigated. By definition he has therefore not been exonerated. In fact, he and his allies furiously oppose the sole possible independent inquiries -- scrutiny, by either the taxpayer or Virginia's attorney general, of public records providing what they all swear is the missing context that would explain Climategate away as a big misinterpretation. As Petitioners have explained repeatedly, release of the emails at issue in this matter has the potential to actually

¹³ *Id.*

exonerate Michael Mann. Because the University has refused to make an honest inquiry, release of the emails is all Mann has left to clear his name.

V. All the Withheld Documents are Records

Having addressed the issues associated with the First Amendment, we now turn to those related to enforcement of the Virginia Freedom of Information Act. The University argues a record must be one that was “prepared for or used in the transaction of public business”, citing to Va. Code § 2.2-3701, but nowhere describes what constitutes Mann’s “public business”. The University also appears to suggest that an email containing both personal information and public business is not a public record. Had ATI been given the opportunity to depose a University representative, we could have offered detailed information on the nature of an academic’s “public business.” In the absence of that discovery, we must rely on what Dr. Mann described as his faculty activities in his report to his own management chain, for purposes of the University department’s annual report. To that we can now add Dr. Mann’s affidavits in this matter. Comparing the content of the exemplars the University claims are not records with Mann’s own statements demonstrates clearly that every exemplar is a public record.

The University claims that all but seven of the exemplars are public records – that PE-2, 13, 14, and RE-10 through 12 are not. Inspection of them and their context as explained in Michael Mann’s affidavits show each is.

PE-2 deals specifically with what a Mann-correspondent thinks ought to be in a technical report Mann is authoring, and what data he used for one of his academic papers. This is a professional communication, not a personal one and Mann lists this kind of activity on this work as part of his job (See RE-4.) Petitioners accept this acknowledgement as true.

PE-13 addresses specific research and academic writing on which Mann is engaged in his academic role.

PE-14 is a discussion on how a group of academics intend to address a challenge to their own scientific views, with Mann specifically debating whether or not to prepare another academic contribution.

RE-10 specifically addresses what to put into an academic paper Mann is preparing.

RE-11, according to Mann, involves his sabbatical plans, including what library materials to which he will need access, and what electronic connections he will need, both elements of academic research he intended to do during that period. See, Confidential Mann Aff. at 2. Sabbaticals are academic activities paid for out of University funds and thus are public business.¹⁴

RE-12, according to Mann, concerns his upcoming academic sabbatical. Because Petitioners had no opportunity to question Mann on this email, its content seemed ambiguous, and potentially not a public record. Mann now admits it concerns his upcoming academic sabbatical (Conf. Mann Aff. at 2) thus admitting the email is a public record as it addresses public business.

¹⁴ University of Virginia, Executive Vice President & Provost, “Leaves of Absence” at http://www.virginia.edu/provost/docs_policies/leaves.html (accessed 8/6/2012) (*emphasis added*).

“The University does not have a standard system of sabbatical leaves, but does provide a more restricted program of research assignments and leave for educational purposes as described below. ‘Educational purposes’ are defined as activities that have as their primary purpose the acquisition of new knowledge, techniques, and experiences that are judged by the relevant department chair, dean, and academic vice president to facilitate or enhance an ability to contribute to the University.”

Research is a primary mission of the University. In order to sustain it at a high level, the vice president and provost permits research assignments for full-time, resident teaching faculty of the schools that report to him. The assignment to research is granted with relief from other duties for one semester, or with half-time relief for one academic year. The assignment may be carried out at any location approved by the appropriate dean.

Research assignments are made possible by the normally budgeted resources of the University, school, and department and are not supported by large endowments.” (emphases ADDED/OR IN ORIGINAL?)

RE-13 is a note to himself that Mann admits in his affidavit is related to his academic activities, and consists of little more than reference to previously published papers whose conclusions are in conflict.

All seven of these emails are clearly public records according to Mann's own representations, as well as taking into account University documents, policies and practice. Thus all of the exemplars are public records, and hence all of the withheld emails are records.

VI. VFOIA requires the adjective "Proprietary" be defined narrowly

We begin our response to the University's discussion of Exemption 4 by directing the Court's attention to the opening section of the Virginia Freedom of Information Act. There the legislature firmly establishes principles that guide the interpretation of exemptions. Specifically, the General Assembly explains that "The affairs of government are not intended to be conducted in an atmosphere of secrecy since at all times the public is to be the beneficiary of any action taken at any level of government." Va. Code § 2.2-3700(B). Further, the General Assembly directs that "The provisions of this chapter shall be liberally construed to promote an increased awareness by all persons of governmental activities and afford every opportunity to citizens to witness the operations of government." *Id.* In addition, they require that "Any exemption from public access to records or meetings shall be narrowly construed." *Id.* Finally, the burden is on the University to "establish an exemption by a preponderance of the evidence." Va. Code § 2.2-3713(E).

Thus, by legislative mandate, application of the term proprietary *must* be "narrowly construed."

A. Petitioners' Definition of Proprietary

Petitioners offer a definition that is narrowly construed and in harmony with other parts of the VFOIA. The University does not and the AAUP specifically argues that the definition should be “broadly” applied. AAUP at 14. Similarly, and as previously noted to this Court, the University made numerous representations to Petitioners’ counsel that it was applying the Act’s exemptions “broadly” when reviewing for release or withholding, before it decided to stop producing altogether.

Like the Petitioners, the University recognizes that VFOIA does not include the term “proprietary” in its section on definitions, and that there is no Virginia case law defining the term.¹⁵ VFOIA does, however give meaning to the word in a section specifically addressing proprietary records and trade secrets. There the legislature provides an exemption for proprietary research-related information produced or collected as a result of study or research on scholarly issues “if the disclosure of such information would be harmful to the competitive position of the applicant.” Va. Code § 2.2-3705.6(17). On this basis, Petitioners argue that where there is a *commercial competitive interest* at stake, the University could properly exempt the information under Exemption 4.

Petitioners then offered the thoughtful decision of the D.C. Circuit in *Washington Research Project*, holding that “a non-commercial scientist's research design is not literally a trade secret or item of commercial information, for it defies common sense to pretend that the scientist is engaged in trade or commerce.” *Washington Research Project, Inc. v. Department of Health, Education & Welfare*, 504 F.2d 238, 244-245 (D.C. Cir. 1974).

¹⁵ The University does cite to *Green v. Lewis*, 221 Va. 547,555,271 S.E.2d 181, 186 (1980), arguing that a proprietary interest need only involve dominion over a thing or property. This case involves clearing title to a church and offers no insight as to academic activities or the VFOIA. It is a bald attempt to obtain judicial agreement with its unilaterally assumed right to construe Exemption 4 broadly, ignoring VFOIA text and case law specifically addressing research data.

On the basis of VFOIA and *Washington Research Project*, Petitioners can identify no commercial competitive interest at stake by release of the emails, as, at most, they involve non-commercial scientists' research designs (if that), none of which involve trade or commerce, with all such work more than a decade old, long after publication of the research.

B. The Commercially Competitive Interest Period

Petitioners suggest that there is in fact a period of time during the pendency of research in which the University and its faculty have a commercially competitive interest. We believe that the beginning of this period is marked by the recordings in UVA's required Research Log that memorialize the start of a creative period. This may be as simple as recording a research idea that is subsequently carried out in a timely manner. This would not include a bright idea never further addressed in a research proposal or in actual research. It would be the initiation of a research effort.

The end of the period is marked by publication of the research results or abandonment of the research effort. In some cases, the academic would never publish the research results, perhaps because the research did not produce positive findings, because no journal was interested in the paper and the faculty member offered up no self-published research report, or perhaps because the research simply faltered from lack of necessary support or was overtaken by events. In any case, the University is not without means to identify the endpoint of a research period, regardless of the success of the research effort. Once ended, the commercially competitive interest period has ended.

The commercial competitive interest can take multiple forms. The most obvious form would be development of a patentable pharmaceutical drug or mechanical or electronic device. The competitive interest is inherent in the product being created and the interest accrues to both

the university and the faculty member. Clearly these kinds of products are created within the private, non-academic competitive marketplace, a marketplace in which the academic can also operate.

Computer code may or may not fall within this obvious competitive interest category. Computer code that has commercial value would. Computer code written only for the purpose of analyzing data from a specific experiment would not. The former could be used generally while the latter is idiosyncratic to the research and has no use beyond that research. Only the researcher and those attempting to duplicate the research need or want that code and it has no commercial value.

What, then, of non-commercial research such as climate research? As the D.C. Circuit explained, “a non-commercial scientist's research design is not literally a trade secret or item of commercial information, for it defies common sense to pretend that the scientist is engaged in trade or commerce.” *Op. cit.* Petitioners caution, however, that the D.C. Circuit only addresses the scientist. The university has commercial interests different from and broader than any particular academic.

We credit the argument that the university is in a commercial competitive marketplace for research funds, and might be in such a marketplace for faculty and students. Forced release of research ideas and information during the research period would reduce the competitive advantage created in the university because of a research effort. This is particularly true for research grant competition where both creative ideas and talented faculty increase the likelihood of obtaining grant funds. While some of those funds accrue to the faculty, significant portions also accrue directly to the university, making the grants a competitive interest of a university. It

is commercial to the degree that both public and private universities compete for the same grant funds.

Competition for faculty and students is less obvious and both the University and AAUP offer nothing more than speculation that release of research records during the research process would reduce the university's competitive advantage for faculty and students. Because a release would, however, compromise a grants process, it is not necessary to address the competition for faculty and students and we leave that for another day where grant funds are not involved.

UVA faculty agree with this formulation of a research period. Professor Kubovy suggests this period is the "context of discovery" within which creativity would be undermined if disrupted. He acknowledges that this period ends upon publication of results when a new, open period, the "context of justification" begins and the public participates. (Kubovy Aff. at 2.) Dean Gittleman describes this period as the "research process" consisting of "hypothesis development, data collection, statistical analysis, grant writing, publication in peer-reviewed journals." Gittleman Aff. at ¶ 7 *and see* Broad Aff. at ¶ 2. Professor West acknowledges the same kind of creative period in the arts, one that opens with the "germination of an idea" and ends with a "public performance." West Aff. at ¶¶ 3 & 5.

Beyond the adoption of this research period by scholars put forward by the University, Petitioners point to how another Virginia university has put this approach into practice under VFOIA.

Professor Edward Wegman of George Mason University was asked by the U.S. House of Representatives to produce an analysis of the methods and statistical validity of a 1998 paper published with Michael Mann as the lead author (*see*, <http://www.uoguelph.ca/~rmckitri/research/WegmanReport.pdf>). He received a request for

certain related records under the Freedom of Information Act of Virginia including his e-mails, emails identical in type and kind to those at issue in this matter. The Court may be interested to know this person seeking the emails under VFOIA happened to be an academic, David Ritson of Stanford University, and he was forwarding his correspondence to Michael Mann, as Mann has posted the emails on his PSU website. See <http://www.meteo.psu.edu/~mann/house06/RitsonWegmanRequests.pdf>). On grounds including that material was being prepared for journal publication, Professor Wegman responded, correctly we believe, that "it is not clear to me that before journal peer review process is complete that we have an academic obligation to disclose the details of our methods before publication." He did not release his records.

Later, after all relevant publication had occurred, GMU received a new FOIA request for Professor Wegman's e-mails. GMU responded by providing approximately 3,000 pages of responsive records to a *USA Today* writer, one apparently working closely with Mann's co-author Raymond Bradley, within 14 days; in electronic format without charge, litigation, or other delaying tactics as is appropriate under the statute.

C. Abandonment of Research

It is important to understand the abandonment of research is not only the end of a research period but an important opportunity to advance science through the public release of unpublished and abandoned research data and methods. Provost Simon gives an example of this situation. In his affidavit, the Provost admits that he "dismissed or found uninteresting and elected not to offer for publication" work that might have had a "broader significance within the medical research community" than he was able to offer. Simon Aff. at ¶ 12. Rather than make this information available to others more competent, inquisitive or creative in the subject area, he

did nothing with the research data. Only later did he decide to continue to shield the information from others and then seek the assistance he needed, but others might not have needed to bring the research to fruition. Dr. Simon abandoned the research and then restarted the research. In the interregnum the Provost stopped his particular contribution's advance of scientific knowledge. We cannot know what patients suffered because of his self-interested unwillingness to give other scientists the opportunity to create new knowledge toward advances in patient care, when he could not. Had a VFOIA request come in at a point after which he had initially abandoned the research but before he restarted his work, his commitment to the advancement of knowledge should have compelled him to release the information so that others could succeed where he had failed.

D. There is no Research Period at issue in this case.

Petitioners acknowledge the Research Period as the time when a proprietary interest deserves protection under VFOIA Exemption 4. Notably, that period has long expired for the emails in question. The University has never offered any evidence that any faculty member is using the content of the emails for ongoing research. Thus, the emails no longer can fall within the ambit of "proprietary" information and to withhold them is to stymie others who might learn from them, others who might advance the borders of knowledge. Far from advancing science, the University's stance seeks to sanction impeding it.

E. Respondents Improperly Seek to Define Proprietary Broadly

Ignoring the mandates of VFOIA, the University offers three arguments that seek to define the term "proprietary" broadly: (i) that the emails are copyrightable and thus are by definition proprietary; (ii) that the legislative history suggests the General Assembly did not mean to require a commercial competitive interest be at stake in order for records to be

proprietary; and, (iii) that Virginia should follow the laws of other states that broadly interpret the term ,if ignoring a Golden Rule of FOI laws, that they be broadly applied and their exemptions narrowly interpreted to bias them toward disclosure.¹⁶

i. Copyright interests remain irrelevant

The parties have previously briefed the copyright issue and need not do so again here. Neither the University nor the Intervenor has offered a cogent basis in policy or law for arguing that any copyright interest is at risk by release of the sought emails and even in the instant matter the University has already released not only material subject to copyright, but actual copyrighted material.

ii. The Legislative History creates ambiguity

The University’s legislative history argument offers no means to narrowly construe Exemption 4. They argue that because the legislature dropped the words “when the disclosure of such data or records may result in a substantial loss to the individual or institution” when revising the statute, the General Assembly intended to broaden the definition of the term proprietary to protect something more than a commercial competitive interest. The University’s analysis of this history is incomplete and fatally flawed.

Petitioners suggest this court follow the sensible approach offered in *Zedner*, to wit:

¹⁶ See, e.g., Presidential Memorandum for Heads of Executive Departments and Agencies, 75 F.R. § 4683, 4683 (Jan. 21, 2009), restating this inherent bias in the federal law; see also, *Milner v. Department of the Navy*, 131 S. Ct. 1259, 1262 (2011) (internal citations omitted) (citing *FBI v. Abramson*, 456 U.S. 615, 630 (1982)); and see also, *EPA v. Mink*, 410 U.S. 73, 93 (1973).The University also makes reference to two Virginia Attorney General Opinions. Petitioners suggest that AG opinions arise where there is no settled law by which to direct the activities of the executive and legislative branches of the Commonwealth, but lose all significance thereafter. It is the Court that decides the law, not the Attorney General. Where an AG opinion offers a cogent argument, a Court may wish to credit that logic and apply it at law, but in the instant case the AG opinions simply don’t offer any specific logic regarding the term “proprietary.” Indeed, the University admits that “the Attorney General does not elaborate” his logic or the legal principles upon which he relies, making his opinion of no value to a Court who must declare the law. Further, both opinions follow the requirement to construe Exemption 4 narrowly, whereas the University attempts to use them to do the opposite, to expand the Exemption broadly.

the only language that constitutes “a Law” . . . that entitles it to our attention, is the text of the enacted statute. See, e.g., *Conroy v. Aniskoff*, [507 U. S. 511](#), 518–528 (1993) (Scalia, J., concurring in judgment).” * * * “if legislative history is relevant when it confirms the plain meaning of the statutory text, it should also be relevant when it contradicts the plain meaning, thus rendering what is plain ambiguous. [T]he use of legislative history is illegitimate and ill advised in the interpretation of any statute

Zedner v. United States 547 U.S. 489, 509-11 (2006) (Scalia concurring).

The University offers no reason why the proffered text was dropped. It may well have been that in the mind of the legislators those words were merely redundant, or it may be that the law could not be passed unless the term were forced to be more ambiguous, the legislature having been unable to otherwise agree on specific language, or it may be that the words were too broad themselves and simply did not reflect the right of the public to see the records. No one knows and UVA possesses no special insight. This is why we suggest the Court not rely on such ambiguous history – a history more ambiguous than the meaning of the word itself. Because the burden is on the University to “establish an exemption by a preponderance of the evidence,” they must do more than speculate on why the General Assembly did what it did. Va. Code § 2.2-3713(E). Offering no more than speculation, they fail to carry their burden.

Finally, of course, accepting the University’s call to read entrails of legislative history only carries the day if the Court concludes that such mystic interpolation of General Assembly decision making must trump the plain language of the Act: that is because the legislature did not drop its mandate that the statute’s exemptions be interpreted narrowly. Instead, the General Assembly prominently placed that command right up front in the Act where, presumably, it could not be inadvertently missed amid the linguistic clutter of the Act’s many exemptions, none of which, Petitioners note, includes that which the University asks this Court to create. This fact alone, which the University elides, dooms its argument.

iii. Other States’ law do not apply to Virginia

With regard to the University's citations to other states' laws, we address them serially, showing none are appropriate for use in this case, either because they operate under different statutory language or involve factual situations unlike those in the instant case.

The University cites to *Robinson v. Indiana Univ.*, 659 N.E. 2d 153 (Ind. Ct. App. 1995), a case involving care and use of animals in research. The University itself admits this case is inapposite because "The exemption in the Indiana Public Records Law differs from Virginia's exemption in that it does not refer to "information of a proprietary nature." (Res. Memo at 14); and because Indiana's FOIA exemption is much more specific and there is no evidence that Virginia intended its exemption to be so broad.

As with their copyright argument, the University attempts to conflate the term "proprietary" with "intellectual property records" when citing to *State ex rel. Physicians Comm. for Responsible Med. v. Bd. of Trs. of Ohio State Univ.*, 108 Ohio St. 3d 288 (Ohio 2006). VFOIA clearly distinguishes the two. *See* Va. Code §2.2-3705.6. Further, the case did not address under what conditions research data might be proprietary. Rather, it involved records of the university's use of lab animals, and whether that information had been released by having been shared with other scientists at other institutions. The case has no bearing on the matters before this Court.

Both UVA and AAUP refer to cases where courts refused to force release of information on the basis that it was collected under a confidentiality agreement. *See*, Res. Memo at 16, citing to *Walker v. Ohio St. Univ. Bd. of Trs.*, 2010 Ohio App. LEXIS 297 at **13 (2010); AAUP memo at 11, citing to *Reyniak v Barnstead Intl.*, 27 Misc. 3d 1212A (N.Y. Sup. Ct. 2010) which relies on *In re R.J. Reynolds Tobacco Co.*, 136 Misc. 2d 282, 286 - 87 (N.Y. Sup. Ct. 1987) (involving confidential information on current patients); *In re Philip Morris*, 706 So. 2d 665

(La.App. 4 Cir. Jan. 28, 1998) (Blanket subpoenas of confidential information); and, *Brazburg v. Hayes*, 408 US 665 (1972) (disclosure of confidential sources). Petitioners have never argued that information protected by a confidentiality agreement is subject to release and the University has offered no admissible evidence that any of the emails at issue in this case are covered by such an agreement.

VII. There are no student records among the exemplars

In its brief, the University fails to identify a single exemplar that contains scholastic records. "Scholastic records" means those records containing information directly related to a student or an applicant for admission and maintained by a public body that is an educational agency or institution or by a person acting for such agency or institution. Va. Code § 2.2-3701. Definitions.

The only references to any student are in RE-3 and 10. RE-3 addresses what Mann plans to have done under his direction by a graduate student. There is no indication it is actually a scholastic record.

We further note that this graduate student may also have been a research assistant understood to have been employed by the university. We cannot know for certain as Petitioners were denied the discovery necessary to elicit that fact. Although the traditional academic and scholastic records on a graduate student may be exempted, where those records are nothing more than the public business of a research assistant, they are not.

The email provides evidence that the graduate student was functioning as a research assistant rather than as a student. A graduate student is expected to formulate an original research hypothesis, defend his research proposal, conduct the research and present the findings.

Here Mann specifically states what he, himself, will do (See RE-3 numbered paragraph “1.”) and indicates that this effort was something on which Mann was engaged, with the help of a research assistant. Nothing in the email or Mann affidavit suggests it was anything other than work done by Mann and a research assistant at Mann’s direction. Nothing suggests it was the student’s original work, done on his/her own initiative and not done at Mann’s direction.

Intervenor Mann buttresses this interpretation of the email as not a scholastic record in his confidential affidavit (Conf. Mann Aff. at 1). There he not only claims personal authorship and responsibility for the research, he makes no mention whatever of the involvement of a student.

In order to claim that the email contained scholastic information, the University has the burden to show it was the student’s original work and that Mann has not directed the work to be done. They have not met the burden of showing this is a scholastic record.

RE-10 fails as a scholastic record for much the same reason. That email does no more than contain the name of the graduate student who is co-authoring a professional paper with Mann, presumably to be identified in the paper as such. A student’s name is considered “directory information” and is allowed to be released under the Family Educational Rights and Privacy Act (FERPA).¹⁷ Because the student identified himself as such at a public meeting, and because Mann so identifies him in the email sent to the journal *Science*, even if the email constituted a scholastic record, it had already been released to the public and thus the University waived and scholastic records exemption it might now wish to claim. Intervenor Mann’s

¹⁷ See, 20 U.S.C. 1232g(a)(5) (“the term “directory information” relating to a student includes the following: the student’s name, address, telephone listing, date and place of birth, major field of study, participation in officially recognized activities and sports, weight and height of members of athletic teams, dates of attendance, degrees and awards received, and the most recent previous educational agency or institution attended by the student.”) And see, 20 U.S.C. 1232g(b)(1) “No funds shall be made available under any applicable program to any educational agency or institution which has a policy or practice of permitting the release of education records (or personally identifiable information contained therein other than directory information, as defined in paragraph (5) of subsection (a) of this section).”

affidavit offers additional information showing why it is not a scholastic record, stating that RE-10 is a communication between himself and the journal *Nature*, which includes a brief instruction to his graduate student. Inspection of the email shows the journal is not *Nature*, but is *Science*. Further, the graduate student is also a research assistant. The email contains no information about the research assistant other than that he is also a graduate student and that fact was already made public before the email was written. The article, the abstract of which is included in the email, was presented at an American Geophysical Union (AGU) meeting in San Francisco, as explained in the exemplar, a communication from Mann to *Science*. There is no other information about the research assistant in the email. Thus, the email is not a scholastic record, and if it were, it had already been released to the public, not only by Mann (via the email), but by the student himself in his poster presentation at the AGU meeting.

VIII. There are no personal or personnel records among the exemplars

The University claims RE-14 contains personnel records. It does not. Although the University suggests that the term “personnel record” is commonly understood, they offer no authority or definition of the term. Not only does VFOIA not define the term, it does not appear anywhere in the Virginia Code, Title 2.2 (Administration of Government). The Provost policy publications, however, do define the term:

Faculty personnel files in the offices of department chairs and deans consist of initial letters offering employment, records of professional development revealed by annual reports, evaluations for tenure and promotion, and other matters of concern at the school level. An official personnel file is also kept in the Department of Human Resources and contains material related primarily to state employment matters. Information in these files which is not exempt from disclosure is available to faculty members, who may request the opportunity to review their personnel files from the department chair or dean or from the supervisor of faculty records. By University policy, confidential letters and statements of recommendation and evaluations of qualifications for employment, retention, or promotion are not available for access to faculty members.

The University adheres to the Virginia Freedom of Information Act (FOIA) and the Virginia Privacy Protection Act of 1976. Categories of personnel information

considered public information under the Freedom of Information Act are an individual employee's "position, job classification, official salary or rate of pay [above \$10,000]...and allowances or reimbursements for expenses."

UVA Provost Policy, "Release of Information from Faculty Personnel Records" at

http://www.virginia.edu/provost/docs_policies/records.html (accessed 8/6/2012) (*emphasis added*).

RE-14 is not a record kept in a personnel file and does not contain the kind of information kept in personnel files. To the degree that it contains any personnel information of any kind, it is similar to an individual employee's position or job classification, both of which are considered public information.

The University also claims RE-10, 11, 12 and 13 contain personal information that can be exempted. None do. VFOIA uses the definition of "Personal information" as provided in Va.

Code § 2.2-3801. Definitions. (Government Data Collection and Dissemination Practices Act).

That act offers this definition:

"Personal information" means all information that (i) describes, locates or indexes anything about an individual including, but not limited to, his social security number, driver's license number, agency-issued identification number, student identification number, real or personal property holdings derived from tax returns, and his education, financial transactions, medical history, ancestry, religion, political ideology, criminal or employment record, or (ii) affords a basis for inferring personal characteristics, such as finger and voice prints, photographs, or things done by or to such individual; and the record of his presence, registration, or membership in an organization or activity, or admission to an institution. "Personal information" shall not include routine information maintained for the purpose of internal office administration whose use could not be such as to affect adversely any data subject nor does the term include real estate assessment information.

RE-10 contains the kind of civil exchange among friends that rises to the level of "best wishes" or "sincerely." RE-11 and 12 cannot be withheld on the basis that the email contains travel plans. As discussed *supra*, Mann describes these two emails as relating to his public business. Petitioners cannot determine why the University offered RE-13 as containing personal

information, and we suspect its inclusion is a typing error. RE-13 makes reference to published works, by full citation. It offers nothing else about the Intervenor. RE-14 does contain information about the Intervenor's hopes and dreams, but nothing that falls within the definition under § 2.2-3801.

There is no exemption for "personal communications" such as travel plans, holiday greetings and personal ambitions. The email system user who includes such information in an email and fails to mark the email as private has already stated to the University that he understands he makes the email subject to VFOIA, and it is not subject to exemption simply because an email containing public business also contains a personal communication of this kind.

IX. Broadening Exemptions through redaction

The University cites to case law to argue that if it can find a single word in an email that is subject to exemption, it can withhold the entire email on the basis that VFOIA does not compel it to redact that word. None of the cases offered by the University address the situation where a custodian hides behind this ploy. It is a common if unsupportable ploy specifically targeted by President Obama and Attorney General Eric Holder for federal agencies to avoid when administering the federal law. *See*, Attorney General Eric Holder, Memorandum for the Heads of Executive Departments and Agencies, "The Freedom of Information Act," March 19, 2009, <http://www.justice.gov/ag/foia-memo-march2009.pdf>; *see also*, U.S. Department of Justice OIP Guidance, "President Obama's FOIA Memorandum and Attorney General Holder's FOIA Guidelines, Creating a 'New Era of Open Government,'" <http://www.justice.gov/oip/foiapost/2009foiapost8.htm>.

If this move is given credit, an academic need do no more than include in his email tag-line specifics on his education, perhaps that he earned a doctorate at a named university, and then

could withhold anything he wished, hiding behind this holding much as the University wishes in this case to hide behind the skirts of Michael Mann's graduate student's public poster session. The effect of this ploy is to broaden an exemption, even where the exemption is valid. It could enable the University to broaden an exemption dealing with personnel information into a blanket exemption of every email ever written by an academic. This is facially absurd. Like the Respondent's and the Intervenor's briefs, this one is lengthy and we do not wish to extend it further than we must. If the Court believes that it must address the redaction question in this litigation, we would ask for an opportunity to fully brief that issue.

X. Conclusion

The University argues that the Court must apply a balancing test by creating a Scholar's privilege in order to protect the withheld emails. In the alternative, it asks the Court to broadly apply the "proprietary", "student records" and "personnel/personal records" exemptions under VFOIA, but only to a subset of emails they admit are records. Petitioners have shown that there is no public speech at issue in this case and hence the Court need not reach a balancing test. This saves the Court from having to create from whole cloth a never-before recognized First Amendment privilege. As well, relying on Intervenor Michael Mann's characterization of the exemplars and the content of the exemplars, Petitioners have impeached the University's allegation that some of the exemplars are not public records subject to VFOIA. Petitioners recognize a "research period" during which VFOIA exemption 4 would apply; but, in their initial memorandum and herein, we show that this research period is long past and their release under VFOIA benefits the society and the expansion of knowledge.

Petitioners close with reference to the Founder and Father of the University of Virginia, in arguing that failure to comply with the VFOIA would breach the norms established by

Jefferson himself as to the need for the public to be well informed about the workings of government, norms that have served for over two hundred years:

Wherever the people are well informed they can be trusted with their own government; that whenever things get so far wrong as to attract their notice, they may be relied on to set them to rights.

Thomas Jefferson to Richard Price, Paris Jan. 8. 1789.

Respectfully submitted,

**THE AMERICAN TRADITION
INSTITUTE and THE HONORABLE
ROBERT MARSHALL**

By Counsel:

A handwritten signature in blue ink that reads "David W. Schnare". The signature is written in a cursive style and is positioned above a horizontal line.

David W. Schnare, Va. Bar No. 44522
SchnareATI@gmail.com
9033 Brook Ford Road
Burke, VA 22015

Petitioners' Response Exhibits 1 – 3

From: Tim Osborn <???@uea.ac.uk>
To: "Michael E. Mann" <???@virginia.edu>
Subject: Re: reconstruction errors
Date: Fri Aug 1 14:24:35 2003

Thanks very much for helping me out with this Mike. Rest assured that the data won't be passed on to anyone else. I'll let you know if I use them to compute uncertainties at different time scales.

Cheers

Tim

At 16:18 31/07/2003, you wrote:

Tim,

Attached are the calibration residual series for experiments based on available networks back to:

AD 1000

AD 1400

AD 1600

I can't find the one for the network back to 1820! But basically, you'll see that the residuals are pretty red for the first 2 cases, and then not significantly red for the 3rd case--its even a bit better for the AD 1700 and 1820 cases, but I can't seem to dig them up. In any case, the incremental changes are modest after 1600--its pretty clear that key predictors drop out before AD 1600, hence the redness of the residuals, and the notably larger uncertainties farther back...

You only want to look at the first column (year) and second column (residual) of the files. I can't even remember what the other columns are!

Let me know if that helps. Thanks,

mike

p.s. I know I probably don't need to mention this, but just to insure absolutely clarify on this, I'm providing these for your own personal use, since you're a trusted colleague. So please don't pass this along to others without checking w/ me first. This is the sort of "dirty laundry" one doesn't want to fall into the hands of those who might potentially try to distort things...

Petitioners'
Response
Exhibit 1

December 17, 2003

Dear Professor Mann,

In MBH98 and MBH99, you refer to analyses of residuals carried out in these studies. Could you please provide me with (a) preferably, a FTP location for the residual series, together an FTP reference for the program generating the residuals; or, (b) in the absence of such FTP location, an email enclosing this information. Your analysis of these residuals was used to estimate confidence intervals in an influential scientific paper.

Thank you for your attention.

Yours truly,

Stephen McIntyre

cc: David Verardo (NSF)

Source: <http://www.climateaudit.info/correspondence/mann.031217.htm> (accessed 8/7/2012).

Petitioners'
Response
Exhibit 2

Dec. 17, 2003

Dear Mr. McIntyre,

I apologize if my last electronic message was not clear but let me clarify the US NSF's view in this current message. Dr. Mann and his other US colleagues are under no obligation to provide you with any additional data beyond the extensive data sets they have already made available. He is not required to provide you with computer programs, codes, etc. His research is published in the peer-reviewed literature which has passed muster with the editors of those journals and other scientists who have reviewed his manuscripts. You are free to your analysis of climate data and he is free to his. The passing of time and evolving new knowledge about Earth's climate will eventually tell the full story of changing climate. I would expect that you would respect the views of the US NSF on the issue of data access and intellectual property for US investigators as articulated by me to you in my last message under the advisement of the US NSF's Office of General Counsel.

Respectfully,

David J. Verardo

Director, Paleoclimate Program

Division of Atmospheric Sciences (Room 775)

National Science Foundation

4201 Wilson Blvd.

Arlington, VA 22203

phone: 703-292-8527

fax: 703-292-9023

email: dverardo@nsf.gov

<http://www.nsf.gov>

cc Michael Mann

Petitioners'
Response
Exhibit 3

Petitioners' Response Exhibit 4

**2004 McINTYRE-STEPHEN SCHNEIDER CORRESPONDENCE
re MANN ET AL SUBMISSION**

Dec. 18, 2003 McIntyre to Schneider

From: Steve McIntyre
Sent: December 18, 2003 10:27 AM
To: Stephen Schneider
Cc: Ross McKitrick
Subject: Forthcoming Climatic Change Article
Dear Professor Schneider,

We are writing to you in your capacity as editor of *Climatic Change*. It has come to our attention that you are planning to publish an article by Mann, Bradley and Hughes, criticizing an article which we recently published in *Energy & Environment*. It is our view that Mann, Bradley and Hughes have seriously misrepresented our findings and that their article is deeply flawed.

We have been informed that the accepted text includes the following statement:

“Journals that receive critical comments on a previously published papers always provide the authors who are being criticized an opportunity to review the study prior to publication, and offer them the chance to respond. This is standard operating procedure in any legitimate peer-reviewed scientific journal. Mann and colleagues were never given this opportunity, nor were any other leading paleoclimate scientists that we're familiar with.”

Applying this to the matter in hand, we note that the Mann, Bradley and Hughes article is a critical comment on a previously published paper, but, contrary to the above policy, *Climatic Change* has not provided us with an “opportunity to review the study prior to publication” nor offered us a “chance to respond”. We hereby formally request the "chance to respond", which we can do on a timely basis

Thank you for your consideration.

Yours truly,

Stephen McIntyre
Ross McKitrick

Dec. 18, 2003 Schneider to McIntyre

From: Stephen H Schneider
Sent: December 18, 2003 9:01 PM
To: Steve McIntyre
Cc: Ross McKitrick; katarina kivel
Subject: Re: Forthcoming Climatic Change Article

Petitioners' Response Exhibit 4

Hello and thanks for telling me your concerns. However, there are several assertions below that are not right. First of all you say I am intending to publish the Mann et al paper in *Climatic Change*, and you are worried

about a passage in it. As I do not publish papers without extensive reviews (unlike some journals)--and controversial ones get an extra reviewer or two--this work is in review, and no decision has remotely been made. Second, the passage you quote was not in the version of the paper we sent out according to my Assistant Editor, Katarina Kivel--she used a find command and found none of the words you mention. In fact, she was surprised you didn't know that as you (SM) were one of the reviewers and the manuscript was sent to you in Toronto on the 10th of December--hopefully the US to Canadian mails handoff will not have failed here--a week already. If in fact the quote is in this version and we missed it, please let me know--but even if it were there it would be up to the reviewers to advise on it and for me to decide if it were a fair statement. I usually give a broad representation of viewpoints the opportunity to be reviewers, and since this paper directly involves you it would have been very unlikely I would not have sent it to one of you--if you want to do a joint review, feel free.

The procedures at CLimatic Change are to be sure, first, technical discussions are state-of-the-art and methods appropriate and accurate. Second, clarity of communications is important, and third, originality counts, but not necessarily in the disciplinary components of submissions but in the integration aspects. After reviews, I direct authors to respond, give them the option to object--but only with detailed arguments--require them to raise paradigmatic disputes directly in the text with references to other viewpoints and sometimes even commission simultaneous editorials to appear that comment on such paradigmatic debates. More than half the time there are second and even third revisions and re-reviews. This does not permit rapid publication, but Climatic Change is more a journal of record than an announcement journal, so we put "quality" ahead of speed of publication. I also never object to authors or others posting in press papers on their websites--or even submitted ones--as long as the status of the article at Climatic Change is clear to users.

I hope this explains how we have been doing things at CLimatic Change for the past 25 years, and I certainly will not change that for this paper.

However, since you sent me an email at the website address, not my e-mail, it occurred to me that you may have gotten your quote or the idea the paper was "in press" from me. So I checked, and apologize that that is what happened. I do post submitted papers if authors don't object, and this should have been labeled "submitted". Unfortunately, in this case, there was a second miscommunication between me and my staff--in part since I was out of town for two weeks and did not check every one of the latest modifications to the 60,000 words and hundreds of links on the site myself last week. I gave to staff an earlier version of a draft Mann et al manuscript that was intended to be submitted, and didn't make clear enough not to post it until the actual submitted version arrived--which it did a week or so ago and it was sent to reviewers including you. That is entirely my responsibility and I am sorry for that error. So the website has two errors--"in press" and the presubmission version. If some revised version of the paper is eventually accepted, and if you offer a comment that is eventually accepted, both will appear on the website in the future. I actually did not intend that version to be posted on my website since I knew the authors were going to modify it--including it appears removing the paragraph you worry about.

I apologize for my communications error with staff due to my overcommittments, and appreciate your pointing this out--after all this massive webssite was just relaased and despite months of external and internal reviews, some mistakes are inevitable and will be corrected as soon as I learn about them. This will be fixed tomorrow as I will forward this e-mail to my staff to correct the error immediately.

I look forward to your review of this paper and the editorial process which will follow it. If you do not get the paper soon let me know and we'll resend it.

Sincerely,
Steephen Schneider

Dec. 19, 2003 McIntyre to Schneider

From: Steve McIntyre [mailto:stephen.mcintyre@utoronto.ca]
Sent: December 19, 2003 10:37 AM
To: Stephen H Schneider
Subject: Re: Forthcoming Climatic Change Article

Thank you for your courteous reply.

As you surmise, the version of the paper which I which saw was the version posted at your website. It was first brought to my attention by an Australian agricultural economist, who in turn had received a printed version circulating by fax machine. He contacted my coauthor to ask if he had seen the paper "about to be published" at CC. Prior to your deletion of the paper from your website, it seems to have been widely disseminated, as it was the top hit under Google for "mcintyre mckitrick". Since this paper contains language you probably do not want people thinking you'd approve for publication, you should perhaps think of straightening out the public record.

I am pleased to accept your invitation to act as a reviewer of the MBH paper. I would prefer an email copy however.

Regards,
Stephen McIntyre

Dec. 19, 2003 Schneider to McIntyre

From: Stephen H Schneider
Sent: December 19, 2003 12:20 PM
To: Steve McIntyre
Cc: Katarina Kivel
Subject: Re: Forthcoming Climatic Change Article

Hello and thanks for the e-mail. Apparently my staff, who removed the paper from my site references section, tell me it is identical to one that was widely distributed by Mann et al, and the only error on our part was the incorrect reference to CLimatic Change in press. SO they thought a widely disseminated webpaper was not inappropriate for inclusion on the reference site, and I agree. THE problem was the CLimatic Change inference that I did not make clear to them in my overcommitted haste. I'm not sure what to do about who got that paper in the few days since the site was open and now when the error is removed, but since it--including the paragraph you don't like--has been available for weeks elsewhere, I doubt

the few days this version sat in my site references constitutes much of an impact. What will really matter is the response of the reviewers, the authors response to the reviewers, any formally submitted comments from you and others--all on the record. That of course will take time, but is how science works when it is functioning well. I can ask my programmers to put a sentence in the site refernces where the version with the wrong attribution to CLimatic Change used to be to put in an errata sentence about the "CLimatic Change in press" error.

As to electronic version, my Assistant editor has gone on vacation and sent you--and all reviewers, as CLimatic CHange usually sends hard copies since we are an international journal and many in 3rd world are not electronically up to date--a hard copy a week or more ago. If it doesn't arrive let me know. Ask Katarina for an electronic copy when she returns on Dec 29--in fact I'll CC this to her so she'll find it when she returns.

With regards,
Steve S.

Dec. 18, 2003 Kivel to McIntyre

From: Katarina Kivel
Sent: December 29, 2003 2:54 PM
To: smcintyre
Subject: Fwd: submission to "Climatic Change"

Dear Dr. McIntyre,

As requested, attached is the submission to Climatic Change by Mann, Bradley, and Hughes. Attached are also the Guidelines for reviewers, Cl Ch Editorial Policy, and review request letter from Dr. Stephen Schneider. By now you should also have received the hard copy sent on 10 December by postal mail.

Please acknowledge receipt of this message.

We look forward to receiving your comments in due course.

Sincerely,
Katarina Kivel

Dec. 18, 2003 McIntyre to Schneider

From: Steve McIntyre
Sent: December 29, 2003 3:18 PM
To: Katarina Kivel
Cc: Ross McKitrick; Stephen H Schneider
Subject: Re: submission to "Climatic Change"

Thanks for this. I've already received and looked at the print version and will send comments in about a week. By the way, I notice that your website stephenschneider.stanford.edu/Publications/PDF_Papers/MannRebuttal.pdf contains a different version of this paper described as being in print. I previously communicated with Prof. Schneider about this and he informed me that it was up on your website in error, that he would remove it and a couple of days later, it was removed. I notice that the paper has now been restored to your website, containing the same presumably erroneous information. I find this rather puzzling.

Yours truly,
Stephen McIntyre

Dec. 29, 2003 Kivel to McIntyre

From: Katarina Kivel
Sent: December 29, 2003 3:26 PM
To: Steve McIntyre
Subject: Re: submission to "Climatic Change"

Dear Dr. McIntyre,

Thank you for confirming receipt of the MBH paper.
As far as Dr. Schneider's web site, I am not involved in this aspect, but I will remind Dr. Schneider about your message so that he can contact his web site assistant.

Thank you for calling this to our attention.

Sincerely,
Katarina Kivel

Dec. 29, 2003 McIntyre to Schneider

From: Stephen H Schneider
Sent: December 29, 2003 5:12 PM
To: Steve McIntyre
Cc: Katarina Kivel; Ross McKitrick
Subject: Re: submission to "Climatic Change"

Hello all. What was removed was the incorrect statement that the Mann et al piece was in press in CLimatic Change. AND the actual submission is not posted either, as it is in review. What was put back was not that paper, but a link to one of their websites--at least that is what I told my staff to do. SO the two are separable, even if the link contains a draft not too different from the submitted version I asked SM to review.

When you submit your review--and if you choose to submit a comment--and all that becomes "in press"--or even if you post your comments on your website--that will likely be linked on my website (your review will be kept confidential, of course, unless you say it can be released to either authors or more generally--that is your call). I feel no constraint, however, in linking to other folks websites or using materials from them they post for general audiences, but that is completely separate from the CLimatic Change version and the erroneous "in press" statement that I removed virtually the moment I was made aware of it by one of you. I hope that clarifies things. Cheers, Steve S.

PS. Since there have been things on my site before I was not aware of given the communications with staff, I checked. THIS is what I find on this issue in the "site references" of my website:

"Mann, M.E., R.S. Bradley, and M.K. Hughes, 2003: "Note on paper by McIntyre and McKitrick in Energy and Environment," University of East Anglia Climatic Research Unit website. Available online at:
<http://holocene.evsc.virginia.edu/Mann/EandEPaperProblem.pdf>

Erratum: for the first few days this site was publicly available we had, in these references only, erroneously identified a similar manuscript as "in press" at Climatic Change. Rather, a revised version was submitted to Climatic Change and is undergoing the normal review process"

Above is a link to Mann's website and his posted piece. So I do not shrink from including that in my site as it is his views and not a mis-statement of the CLimatic Change aspect. I--and most other websites--include dozens of such links and downloads, and I will add many more as they become available and we become aware of them. Of course, my selection of what goes in the site is my view on the debate, and reasonable people can differ--very substantially--on what they think belongs in an informational website. Mine contains my beliefs of what parties interested in this issue should be aware of, not some journalistic notion of equal time and equal space to all claimants of "truth"--but if you saw my "mediarology" section you know my philosophy and I suspect there will be things we will just have to agree to disagree about. But an error, like the "in press" words in the first few days of the website's release, that is not a personal judgment and thus I removed it as fast as I learned it was there.

Jan 6, 2004 McIntyre to Schneider

From: Steve McIntyre

Sent: January 6, 2004 3:30 PM

To: Stephen H Schneider

Cc: Ross McKittrick; Katarina Kivel

Subject: MBH Submission to CC January 4, 2004

Dear Prof. Schneider,

Thank you for the opportunity to review the submitted paper. Some of its key arguments are based on computer calculations which are not spelled out in the paper or in on-line Supplementary Information. In order to assess them, I need access to the computer programs used. The next section lists the particular items I need. I am not asking for anything onerous, just the command files used, which the authors evidently have at hand since they were able to generate the graphs and compute statistics. I would appreciate it if you could arrange for these supporting calculations to be provided to me, whereupon I will have other comments. My comments on the paper itself follow the listing of the needed files.

SUPPORTING CALCULATIONS

Would you please provide me with the supporting calculations for the following. The paper doesn't even state the program used (SAS? SPSS?) let alone give any mathematical details.

Pages 4-5: The program(s) used to compute the series in Figure 2, and the cross-validation experiments.

Page 5, item (a): In order to assess if this is an "Important Technical Error" as the heading suggests, we will need the gridpoint standard deviations used by MBH98 under their CRU version, as well as supporting calculations for temperature eigenvectors, principal components and eigenvalues; and the computer scripts.

Items (b,c): Neither here nor in MBH98 is the “objective selection criterion” spelled out in mathematical detail. Presumably though there is a command file that generates the test statistics: this (and the output file) will suffice.

Page 7, Items (d,f) etc. Calculations showing that the MBH98 reconstruction is “completely insensitive” to the use of summer rather than annual mean CET and Central European temperature data, use of the 20-year fill for series #50, and correcting the misplaced geographical locations.

page 8, last paragraph. The calculation of the uncertainty limits for Figure 1.

COMMENTS

INTRODUCTION

Page 1, sentence 1. MM03 does not make the specific claim that “15th century temperatures were warmer than those of the late 20th century”. No such claim is made in MM03, and where some prominent commentators have interpreted our work in this way we have written to correct the record and ask them to revise their web sites. In the FAQ at <http://www.uoguelph.ca/~rmckitri/research/trc.html>, M&M state explicitly:

“Your graph seems to show that the 15th Century was warmer than today’s climate: is this what you’re claiming?”

No. We’re saying that Mann et al., based on their methodology and corrected data, cannot claim that the 20th century is warmer than the 15th century – the nuance is a little different. To make a positive claim that the 15th century was warmer than the late 20th century would require an endorsement of both the methodology and the common interpretation of the results which we are neither qualified nor inclined to offer.”

Page 1, first paragraph. Since the first sentence incorrectly characterizes the claim made by MM03, the following sentences are irrelevant.

Page 1, bottom paragraph, first sentence. The definition of an “audit” provided in sentence 1 is not correct. The objective of an audit is to verify the accuracy of a report, which may involve revision of data and procedures where necessary. Auditors often produce audited statements, which differ from unaudited statements. The claim that MM03 have used “neither the data nor the procedures of MBH98” is obviously overstated and only undermines the credibility of the authors since the paper itself establishes otherwise. At best they can say that MM03 used data and procedures that turn out to differ in subtle but important ways from MBH98.

Page 1, first paragraph, fourth sentence. Only a *statement* can *misrepresent* MBH98. An analysis may be incorrect, but is not a *misrepresentation*. No examples are provided in this paper of statements by MM03 which “seriously misrepresent” MBH98. MBH cite examples of methodological differences, which are not “*misrepresentations*”, much less “*serious misrepresentations*.”

Page 2, top sentence. MM03 does not claim that they “undertook exactly the same analysis as MBH98”; MM03 states: “We replicated the methodology of MBH98 as closely as we could using publicly available documentation and such private assistance as we were able to obtain.” What is incorrect about this statement?

Page 2, top paragraph, line 5. MBH refer, here and elsewhere, to “omission” of key early proxy indicators. This term is inaccurate. MM03 used the full proxy network after all sites were available. MBH have recently disclosed (but not in MBH98) that they varied their site rosters as a method of dealing with missing data in PC calculations. This is a difference in methodology that relates only to the handling of missing data in principal components, but it does not imply that MM03 opted for “omission” or “inconsistency”.

IMPACT

Page 2, middle paragraph, 3rd last line. MBH98 has 22 indicators for the 1400-1450 period. MM03 handles principal components differently than MBH98, affecting 2-3 out of 22 indicators. This is here described as “remov[ing] the majority of the underlying proxy data”. This sentence is obviously misleading.

Page 2, last paragraph, sentence 1. This is incorrect. MM03 provide FTP references showing indicators used. MM03 provide an FTP site showing the indicators in *.txt and *.xls files and the number of indicators for each year can be trivially calculated. MM03 also provide exact references to updated data versions used in their data – something that is not done in MBH98.

Page 2, last paragraph. No series are “omitted” or “eliminated”. Obsolete data used in MBH98 is replaced with updated versions. Principal component series are calculated for period in which all sites are available, as is clearly described in MM03. As above, this procedure differs from the “stepwise” procedure for PCs recently disclosed by MBH98, but is not an “omission”.

Page 3, top paragraph. This is the most salient issue in the article:

- MBH say that MM03 made a “complete misrepresentation” of the procedures used by MBH98. MM03 states the following: “Because standard PC algorithms fail in the presence of missing data, an important part of the methodology—namely how missing data were treated in the PC calculation—remains unexplained in MBH98.” Far from being a “misrepresentation” of MBH98, it is an entirely correct statement. MM03 explicitly state that they used a conventional PC algorithm and used PCs for the maximum available period – which is necessarily the period with no missing data. This method may not be the same as the “stepwise” method, but is a plausible interpretation of the available public record. If the public record is ambiguous, the responsibility for that rests with MBH.
- MM03 do not “eliminate” the 70 WNA sites, but in fact use these sites in the calculation of their PCs. It is only the period of availability that differs. It is misleading to say that the differing PC calculations eliminated a “majority” (and later in the paragraph the “vast

majority”) of the proxy indicators, since, as stated above, the impact is only on 2-3 indicators out of 22 available at that interval.

- MBH disclose here (but not in MBH98) that they used a stepwise PC calculation. The actual MBH98 procedure is stated later in this paragraph as being done “separately for all intervals used in their [MBH98] stepwise reconstruction”. The Nature SI shows 11 intervals used in the stepwise reconstruction: commencing 1400, 1450, 1500, 1600, 1700, 1730, 1750, 1760, 1780, 1800 and 1820. But at the newly-disclosed FTP site there are not directories for each of these intervals, so either the statement that this is done “separately for all intervals used” is not correct or the FTP site is incomplete. Or some other procedure was used, which has not yet been described.
- The last block of sentences is very misleading. It is perfectly obvious that MM03 obtained data for the NOAMER sites from NOAA Paleoclimatology since these FTP sites are listed in the MM03 Appendix. The data used by MBH98 is not located at NOAA Paleoclimatology as NOAA Paleoclimatology has different versions of many data series, a point stressed by MM03 in connection with other proxies, but this also applied to the NOAMER sites. The data at <ftp://holocene.evsc.virginia.edu/pub/MBH98/TREE/NOAMER> was not available to MM03. The site was not identified to M&M, even in response to queries about data problems, and we had no knowledge of it until after MM03 came out. We were referred to the location <ftp://holocene.evsc.virginia.edu/pub/sdr/pcproxy.txt> and no indication was given that we should look for alternate versions of the same data, even in response to direct queries about the correctness of the data. Even if MBH had directed us the [pub/MBH98](ftp://holocene.evsc.virginia.edu/pub/MBH98/TREE/NOAMER) site, this section of the paragraph would still be irrelevant since the contents of the FTP site in question correspond closely to the data which we obtained directly from WDCP.

Page 3, item 1. MBH state that the original proxy data has been available “since May 2000”. The FTP site indicates availability since July 2002 only. In any case the site is not “available” if its location is kept secret, even in response to a request for it.

Page 3, item (2). The statement that MM03 used “a completely unrelated set of tree-ring records produced by Stahle and coworkers in earlier work” is false. MM03 used the 10 sites named in the MBH98 Supplementary Information and obtained WDCP versions of these. The versions differ from the versions used by MBH, but the onus is on MBH to establish the provenance of their data. In any event, the WDCP versions of the sites listed in the SI are hardly “generic tree-ring data”.

Page 3, item 2. MBH characterize the reference to Stahle et al as “outdated”; if Stahle et al. (1998) was actually used, then the MBH98 reference is more accurately characterized as “incorrect”. The reference to Stahle et al 1998 raises several questions: (a) Stahle et al. (1998) use only earlywood widths, while MBH98 use both earlywood and latewood widths. Use of the latter in MBH98 would therefore be incorrect. (b) Stahle et al (1998) go back only to 1706 in their calculations: what is the basis for MBH98 going earlier?; (c) the sites used in MBH98 according to the Supplementary Information differ from the sites in Table 1 of Stahle et al (1998); (d) TREE/STAHLE/SWM has 11 sites and fails to identify the sites, while MBH98/SI lists only 10 sites: what is the correspondence between the sites at TREE/STAHLE/SWM and

MBH98/SI and what is the 11th site? (e) the data versions at TREE/STAHLE/SWM are not identical with the data versions at WDCP for the SWM sites listed at MBH98/SI: what is the exact provenance of the data versions at WDCP, including date received?

Page 4, item (3). MM03 used the version at WDCP for TTHH. The MBH98 version appears to be an obsolete edition and is not at WDCP. What is the exact provenance of the data version used by MBH98, including date? What is wrong with MM03 using the WDCP version?

Page 4, paragraph 2: this reconstruction does not indicate “warmth during the 15th century” but higher values of the MBH98 temperature index. MBH03 states: “this result should have been discarded”; MM03 explicitly state that the MBH98 dataset cannot yield reliable information and thus clearly do “discard” this result.

Page 4, paragraph, 10 sentences from bottom. “MM03 would have been forced to conclude that a skilful reconstruction is not possible prior to 1500.... It was completely incorrect for MM03 to conclude that their reconstruction provided statistically reliable evidence of anomalous warmth prior in the 15th century...against the instrumental record”. For reasons already stated this misrepresents the MM03 interpretation of the results, and moreover MM03 concluded that MBH98 data could not be used for climate reconstruction – which is the same conclusion as MBH here say that MM03 should have reached, granting their premises.

Page 5, top paragraph, last sentence. “robust with respect to the precise instrumental calibration interval used”. The calculations shows only that the results are insensitive to choice between 1902-1971 and 1902-1980 calibration periods, but does not show robustness for all possible calibration intervals as asserted.

Part 3- IMPORTANT TECHNICAL ERRORS

Page 5, Item (a). Why would the choice of anomaly reference period affect the gridpoint standard deviations?

Page 5, Item (a). This paragraph apparently asserts that the CRU data used for calibrating the proxy network was not the same as that used to graph the NH average up to 1998. Does this mean there is a splice in the MBH98 figure at 1993? Or is the entire instrumental series graphed therein the newer version ending in 1998, and thereby different from the data used to calibrate the proxies? Either way this needs to be clearly stated, considering the role that portion of the graph plays in their conclusions and the claim of this paragraph that editions of CRU data matter so acutely.

Page 5, Item (a) second paragraph. MBH98 did not state that gridpoint standard deviations were detrended. Is the effect material?

Page 5, Item (b). MBH state that PCs of proxy sub-networks were calculated separately for each interval in their stepwise reconstruction. Leaving aside the assertion that “this is the only sensible approach” (we can think of others), MBH98/SI shows a total of 11 subintervals, commencing in 1400, 1450, 1500, 1600, 1700, 1730, 1750, 1760, 1780, 1800 and 1820. The subdirectories at UVA list PCs for *some*, but not all, sub-intervals. Where are the

subdirectories for the sub-intervals not listed at UVA? What is the basis for choosing some intervals for separate PC calculation and not others?

Page 5, Item (b): what is the “objective selection criterion” applied to the network. (also see calculation requests).

Page 5, Item (b): a nomenclature is needed to distinguish between the proxy series used only to make PC indicators and the 112-159 indicators used in the regression calculations. In MM03 we used “#” to denote one of the 112 proxy indicators. By mingling the two types of series MBH are make exaggerated claims about the differences in the 15th century data set, for instance by referring to 70 tree ring series rather than 2 PCs.

Page 6, item (c). Following MBH98, MM03 use 1 eigenvector in 1400-1450 and 2 eigenvectors in 1450-1600. Unless MBH have calculations to show otherwise, it is hard to see that this procedure has a material affect and therefore should not be included under “important” issues, as it is minor.

Page 6, Item (d). This scaling procedure is not described in MBH98. It adds a further linear transform to the reconstructed temperature series, but there is no explanation of why it is necessary or what influence it has on the final results. If it is a “minor” issue, it should not be listed in a section labelled “important” issues.

4. SPURIOUS CRITICISMS

Page 6, item (a) The authors need to explain the basis for selecting temperature gridpoints. In this paragraph they are making reference to an apparently unavailable version of the CRU dataset. Are copies still available? Since the updated version apparently deletes all the entries in 4 grid cells, this would, on the face of it, suggest that the data in those cells was unreliable in the version used by MBH.

Page 6, item (b). MBH03 identify a difference in procedure, but the procedure followed in MM03 is not precluded by disclosure in MBH98, and thus is not a “mistake”. Point (3.b) resolves part of the MM03 concern, but creates another. The calculations yielding series described as “PC’s” in ftp/MBH98 are not “conventional” principal component calculations. MBH98 uses a Fortran program in which SVD is applied to an off-center dataset, instead of to a covariance or correlation matrix as required by the optimization problem from which PC algorithms are derived. Hence the procedure applied by MBH98 is not “optimized with respect to the underlying data.”

Page 6, Item (b). The assertion in MM03 is not incorrect. MBH98 did in fact calculate PCS from data sets with missing data by the now disclosed expedient of stepwise calculations. This may or may not be a valid expedient, but the data sets do have missing data.

Page 6, Item (c). The authors make a claim in strong terms that SWM data exhibit a unique sensitivity to El Nino and hence must be grouped separately. This claim is contradicted by the fact that the NOAMER compilation includes so many SWM sites. Many, if not most, of the

actual Stahle/SWM locations are geographically interwoven with ITRDB/NOAMER sites: look at the NM, AZ and CO sites in Stahle/SWM as compared to NM, AZ and CO sites in ITRDB/NOAMER and the mexi001 series used as a separate proxy. What is the difference between the Stahle et al 1998 site of Eagle Creek AZ (az518), a PIED site, and the following NOAMER PIED sites in Arizona: AZ081, AZ084, AZ086, AZ102, AZ104, AZ106, AZ129, AZ144? Why is the Stahle et al 1998 Table 1 PIED site of Navajo Mountain UT (UT022) included in NOAMER? What is difference between UT022 and the following NOAMER PIED sites in Utah: UT018, UT020, UT022? Why are Stahle et al 1998 PIPO sites of Abouselman Springs (nm555) and Elephant Rock (nm559) included in NOAMER? What is difference between these sites and NOAMER PIPO NM sites: NM021, NM029, NM550, NM556, NM557, NM566, some of which are in very close proximity. What is difference between Table 1 PSME sites in NM of Ditch Canyon (nm503) and Pueblita Canyon (nm537) and NOAMER NM PSME sites of: NM025, NM026, NM565, NM570, NM570X, NM572? What is difference between Table 1 NM PIED site of Paliza Canyon NM (nm043) and NM PIED sites in NOAMER of NM030, NM031, NM035, NM038, NM055, NM118, NM564? As noted above, the MBH98 implementation of Stahle et al (1998) is not consistent.

Page 6, Item (c). What are the similar arguments for other proxy sub-networks?

Page 7, Item (d). MBH say that the deletion of C Eng values prior to 1730 is “in keeping with previous studies (Bradley and Jones 1993)”. Inspection of Figure 2 in Bradley and Jones 1993 shows that these values are indeed deleted in this study, but no justification is provided for the truncation. Is there a quality control principal being applied consistently here? There is a material gap in the Urvina Bay coral record. What is the basis for keeping this record, but truncating the C Eng record? MBH argue that the CET truncation can be justified based on “inhomogeneities that exist in the record prior to the 1730s”, citing Manley 1974 as describing a gap from 1707 to 1722 in daily data that he was seeking to cover and “used an adjusted version of data from the Netherlands to fill this gap”. This does not apply to the Central European Series though. Moreover it is an incorrect characterization of Manley 1974. Manley (1959) relied on Utrecht data. Manley (1974) says that one of its purposes was “to incorporate in the light of more recent discoveries in MS, hitherto unrecorded, some improvement on the estimates that were previously made to cover the months from 1707 to 1722” (392). He refers to the following for 1707-22: “an additional North of England non-instrumental diary of Wiilliam Elmsall; also Christopher Rawlinson MS for 1699-1717; Smith at Richmond; instrumental readings for 1716-22. “wind and weather” in or near London are continuous since 1669. Even accepting the deletion of the early years, no explanation is given for switching to summertime averages. To justify it by saying it makes no difference only raises the question, Then why do it? Additionally, no justification is given for Central Europe truncation or chin04 truncation. And the last sentence is misleading as they do not affect the early periods in which data was truncated.

Page 7, Item (e). This repeats the criticism of page 4, bottom paragraph. The comments of page 4, bottom paragraph properly belong here, since the issue of the effect of infilling is not an “Important Error” in MM03, although it may be a Spurious Criticism.

Page 7, Item (g). This is false. M&M did not make a “special request” for a spreadsheet version, and at no time was a spreadsheet version prepared for MM (at least none was

supplied). MM requested an FTP location of series in same form as available for MBH99. Instead Mann's associate provided a FTP location of a txt and a mat file containing a data collation, but the file itself was obviously not specially prepared as it was created long before the request by M&M. In the ensuing months the pub/MBH98 FTP site was never divulged.

Page 7, 3rd paragraph from bottom. MBH acknowledge "minor typographical errors", "minor mistakes in the data listings and references" and that the "indicated locations of some of the proxy records were misplaced from their true locations". Where are the current errata listed? Since MBH98 purports to provide regional climate reconstructions, it needs to be shown that the rather spectacular misplacements of precipitation series e.g. the misplacement of Paris and Toulouse series to North America have no effect and, perhaps, more importantly, it raises the question of why such misplacements have no effect on MBH98 results.

Page 7, 2nd last paragraph. Granted that the space in Nature for description of methodology is insufficient, but there is no obstacle to a more copious and systematic description of methodology in Supplementary Information, and there is certainly no page limit on Internet sites. Zorita et al (2003) cannot be cited in defence of the adequacy of disclosure. Zorita et al does not use MBH98 proxy data and do not "independently reproduce" the results of MBH98. They use GCM data and did not have to use "stepwise" methods to deal with missing data. This claim is accordingly incorrect.

Page 8, paragraph 1. A date of July 2000 is given here for FTP availability for proxy indicators. Elsewhere date of May 2000 is given. The actual site does not appear to have been started until July 30, **2002**.

Page 8, paragraphs 2-3. The claim implicit in this section is that all the data have been made available. This is very misleading. The FTP site for MBH98 includes many series that were not used in MBH98 and it is impossible to distinguish one from the other in the present state of data description. Nor is it possible to identify the sections of the PC files that have to be spliced together in the 'stepwise' procedure. The new FTP site referred to is not currently up, but presumably is in process. And in any case, if the existing disclosure is so comprehensive, why is a new FTP site needed?

CONCLUSIONS

Page 8, paragraph 4. "the correct interpretation of MM03 would have been that a skilful reconstruction was not possible.." This was the conclusion of MM03. The next few sentences are irrelevant. We do not make a claim about the 15th century climate.

I appreciate the opportunity, which you have provided for me to carry out this review and hope that this process will contribute to a convergence of understanding and the resolution of various outstanding issues in an effective and amicable way. I also hope that you find these comments helpful. I look forward to receiving the supporting calculations requested above and will provide further comments on receipt of these supporting calculations.

Yours truly,

Stephen McIntyre

Jan 15, 2004 Christian Azar, Clim Chg Editorial Board

<http://www.eastangliaemails.com/emails.php?eid=387>

At 10:36 15/01/2004 +0100, Christian Azar wrote:

Dear all,

I agree with most of what has been said so far. Reproducibility is the key word. If the Mann et al material (to be) posted on the website is sufficient to ensure reproducibility, then there is no compelling need to force them to hand it out. If not, then the source code is warranted. Also, even if there is no compelling need to make the source code public, doing it anyway would clearly be beneficial for the entire debate.

Yours,

Christian

Christian Azar

Professor

Department of physical resource theory

Chalmers University of Technology

Jan 16, 2004 Phil Jones to Clim Chg Editorial Board

Date: Fri, 16 Jan 2004 12:37:29 +0000

To: Christian Azar <christian.azar@xxxxxxxxxxx.xxx>, christian.pfister@xxxxxxxxxxx.xxx

From: Phil Jones <p.jones@xxxxxxxxxxx.xxx>

Subject: Re: AW: CLIMATIC CHANGE needs your advice

Cc: "'David G. VICTOR'" <dgvector@xxxxxxxxxxx.xxx>, 'Katarina Kivel'

<kivel@xxxxxxxxxxx.xxx>, N.W.Arnell@xxxxxxxxxxx.xxx, frtca@xxxxxxxxxxx.xxx,

d.camuffo@xxxxxxxxxxx.xxx, scohen@xxxxxxxxxxx.xxx, pmfearn@xxxxxxxxxxx.xxx,

jfoley@xxxxxxxxxxx.xxx, pgleick@xxxxxxxxxxx.xxx, harvey@xxxxxxxxxxx.xxx,

ahs@xxxxxxxxxxx.xxx, Thomas.R.Karl@xxxxxxxxxxx.xxx, rwk@xxxxxxxxxxx.xxx,

rik.leemans@xxxxxxxxxxx.xxx, diana.liverman@xxxxxxxxxxx.xxx, mccarl@xxxxxxxxxxx.xxx,

lindam@xxxxxxxxxxx.xxx, rmoss@xxxxxxxxxxx.xxx, ogilvie@xxxxxxxxxxx.xxx,

barrie.pittock@xxxxxxxxxxx.xxx, pollard@xxxxxxxxxxx.xxx, nj.rosenberg@xxxxxxxxxxx.xxx,

crosenzweig@xxxxxxxxxxx.xxx, j.salinger@xxxxxxxxxxx.xxx, santer1@xxxxxxxxxxx.xxx,

h.j.schellnhuber@xxxxxxxxxxx.xxx, F.I.Woodward@xxxxxxxxxxx.xxx, gyohe@xxxxxxxxxxx.xxx,

leonid@xxxxxxxxxxx.xxx, shs@xxxxxxxxxxx.xxx

Dear Steve et al,

I've been away this week until today. Although the responses so far all make valid points, I will add my thoughts. I should say I have been more involved in all the exchanges between Mike and MM so I'm probably biased in Mike's favour. I will try and be impartial, though, but I did write a paper with Mike (which came out in GRL in Aug 2003) and we currently have a long paper tentatively accepted by Reviews of Geophysics. With the latter all 4 reviewers think the paper is fine, but the sections referring to MM and papers by Soon and Baliunas are not and our language is strong. We need to work on this.

Back to the question in hand:

1. The papers that MM refer came out in Nature in 1998 and to a lesser extent in GRL in 1999. These reviewers did not request the data (all the proxy series) and the code. So, acceding to the request for this to do the review is setting a VERY dangerous precedent. Mike has made all the data series and this is all anyone should need. Making modelcode available is something else.

2. The code is basically irrelevant in this whole issue. In the GRL paper (in 2003 Mann and Jones), we simply average all the series we use together. The result is pretty much the same as MBH in 1998, Nature and MBH in 1999 in GRL.

3. As many of you know I calculate gridded and global/hemispheric temperature time series each month. Groups at NCDC and NASA/GISS do this as well. We don't exchange codes - we do occasionally though for the data. The code here is trivial as it is in the paleo work. MBH get spatial patterns but the bottom line (the 1000 year series of global temps) is almost the same if you simply average. The patterns give more, though, when it comes to trying to understand what has caused the changes - eg by comparison with models. MM are only interested in the NH/Global 1000-year time series - in fact only in the MBH work from 1400.

4. What has always intrigued me in this whole debate, is why the skeptics (for want of a better term) always pick on Mike. There are several other series that I've produced, Keith Briffa has and Tom Crowley. Jan Esper's work has produced a slightly different series but we don't get bombarded by MM. Mike's paper wasn't the first. It was in Nature and is well-used by IPCC. I suspect the skeptics wish to concentrate their effort onto one person as they did with Ben Santer after the second IPCC report.

5. Mike may respond too strongly to MM, but don't we all decide not to work with or cooperate with people we do not get on with or do not like their views. Mike will say that MM are disingenuous, but I'm not sure how many of you realise how vicious the attack on him has been. I will give you an example. When MM came out, we had several press calls (I don't normally get press calls about my papers unless I really work at it - I very rarely do). This was about a paper in E&E, which when we eventually got it several days later was appalling. I found out later that the authors were in contact with the reviewers up to a week before the article appeared. So there is peer review and peer review !! Here the peer review was done by like-minded colleagues. Anyway, I'm straying from the point. Tim Osborn, Keith Briffa and I felt we should put something on our web site about the paper and directs people to Mike's site and also to E&E and the MM's site. MM have hounded us about this for the last four months. In the MM article, they have a diagram which says 'corrected version' when comparing with MBH. We have seen people refer to this paper (MM) as an alternative reconstruction - yet when we said this is our paragraph MM claim they are not putting forward a new reconstruction but criticizing MBH 1998 !! We have decided to remove the sentence on our web page just to stop these emails. But if a corrected version isn't a new or alternative reconstruction I don't know what is.

So, in conclusion, I would side with Mike in this regard. In trying to be scrupulously fair, Steve, you've opened up a whole can of worms. If you do decide to put the Mann response into CC then I suspect you will need an editorial. MM will want to respond

also. I know you've had open and frank exchanges in CC before, but your email clearly shows that you think this is in a different league. MM and E&E didn't give Mann the chance to respond when they put their paper in, but this is a too simplistic. It needs to be pointed out in an editorial though - I'm not offering by the way.

I could go on and on

Cheers

Phil

Jan 16, 2004 Jones to Mann

From: Phil Jones <p.jones@xxxxxxxxxxx.xxx>

To: mann@xxxxxxxxxxx.xxx

Subject: CLIMATIC CHANGE needs your advice - YOUR EYES ONLY !!!!!

Date: Fri Jan 16 13:25:59 2004

Mike,

This is for YOURS EYES ONLY. Delete after reading - please ! I'm trying to redress the balance. One reply from Pfister said you should make all available !! Pot calling the kettle black - Christian doesn't make his methods available. I replied to the wrong Christian message so you don't get to see what he said. Probably best. Told Steve separately and to get more advice from a few others as well as Kluwer and legal.

PLEASE DELETE - just for you, not even Ray and Malcolm

Cheers

Phil

Jan 22, 2004 McIntyre to Schneider

From: Steve McIntyre

Sent: January 22, 2004 5:31 PM

To: Stephen H Schneider; Katarina Kivel

Cc: Ross McKittrick

Subject: Review of Mann et al.

I've not heard back from you with the supporting calculations which I requested for my review. Could you update me on the status of this request? Thanks, Steve McIntyre

Jan 22, 2004 Kivel to McIntyre

From: Katarina Kivel

Sent: January 22, 2004 5:31 PM

To: Steve McIntyre

Subject: [Re: Review of Mann et al.]

I will be out of the office until February 2, and will respond to your message after that time.

Thank you, Katarina Kivel

Jan 22, 2004 Schneider to McIntyre

From: Stephen H Schneider
Sent: January 22, 2004 9:29 PM
To: Steve McIntyre
Cc: Katarina Kivel; Ross McKitrick
Subject: Re: Review of Mann et al.

Thanks for the request--I have run the issue by the full Climatic Change Editorial Board since a source code request by a reviewer is unprecedented in the 28 years since I founded the journal. It spawned a very interesting set of discussions about to conclude. I'll respond fairly soon with my opinion on how to handle this interesting issue. SHS

Jan 23, 2004 McIntyre to Schneider

From: Steve McIntyre
Sent: January 23, 2004 1:50 AM
To: Stephen H Schneider
Subject: Re: Review of Mann et al.

Perhaps there are some advantages to a cross-fertilization of experience. As you perhaps know, I have considerable business experience, including experience with public securities offerings and ongoing corporate disclosure, both of which impose considerable obligations of due diligence on directors and officers. I'm surprised to learn that what appears to me as elementary due diligence is unprecedented for a journal of record in the climate field. I'd be interested in learning why the disclosure of supporting calculations would be an issue at all. Obviously, disclosure and due diligence requirements for public corporations have increased over the last 28 years and perhaps this is an opportunity to establish some useful precedents for improved and more adequate due diligence on the part of academic reviewers.

Regards, Steve Mc.

Jan 23, 2004 McIntyre to Schneider

From: Steve McIntyre
Sent: January 23, 2004 11:40 AM
To: Stephen H Schneider
Subject: Re: Review of Mann et al.

Ross McKitrick has pointed out to me that the Journal of Applied Econometrics (their archive is at <http://qed.econ.queensu.ca/jae/> and their policies are stated at <http://www3.interscience.wiley.com/cgi-bin/jabout/4079/ProductInformation.html>) strongly encourages authors to submit their command files, especially if replication requires use of a particular program. The example at the data archive includes a copy of such a computer program. This is a case where the policies extend further than merely providing the command file to an interested reviewer, but extend to the archiving of the command file with the data. This seems to me to be an effective methodology and a policy which you might well consider adopting for your journal.

Regards, Steve McIntyre

Jan 25, 2004 McIntyre to Schneider

From: Steve McIntyre
Sent: January 25, 2004 5:29 PM
To: Stephen H Schneider
Cc: Ross McKittrick
Subject: Data Archiving and Citation Policy

Dear Prof. Schneider,
I've noticed recently that AGU has a policy for AGU publications (e.g. GRL, JGR) for citation of data sets and for requiring the archiving of original data sets, set out at http://www.agu.org/pubs/data_policy.html. Do you have a similar or corresponding policy at Climatic Change or is it pretty much laissez-faire? If you have such a policy, would you please send it to me. I should be able to check the MBH paper for compliance, as I'm obviously quite familiar with the data.
Regards,
Steve McIntyre

Jan 25, 2004 Schneider to McIntyre

From: Stephen H Schneider
Sent: January 25, 2004 11:57 PM
To: Steve McIntyre
Cc: Ross McKittrick
Subject: Re: Data Archiving and Citation Policy

Hello and thanks for the various citations to the evolving policies of journals in trade-offs between reasonable requests to scientists from reviewers--a common practices "doctrine" not a legal set of codes--to permit replication without odious burdens or violations of intellectual property norms for authors--their codes are analogous to machines invented by copyrighters. I think we in science should have a higher standard of disclosure--certainly enough to permit most colleagues to perform replicate studies--but does that mean simply providing all data & clear statements of methods/algorithms used, or posting of source codes which are often personalized to individual machines, systems software, statements not transparent to outsiders etc. In fact, if others got different answers using the same code it still could be from different hardware/software. So what the Editorial board is debating is how to define a reasonable practice to insure any investigator who is so inclined could try replications work.

In some cases a source code would be needed, but not in most and as that is very rare in climate sciences to date, we are trying to use this case to define a reasonable and explicit policy--though some degree of case by case flexibility will likely be part of it since peer review is NOT a legalistic standard of "truth" beyond a reasonable doubt--an unobtainable abstraction, particularly in complex systems research--but rather a way to bring published work up to community standards without tying up authors with endless questions on how their code works etc. Reviewers are not expected to rerun authors codes, just to provide a carefully thought out commentary on scholarship, methods and fairness of conclusions--do they follow logically from the analysis provided or cited in the literature. Reviewers who want to replicate or disprove in replications attempts, are encouraged to publish that in separate publications--as you did in Energy and Environment on MBH, and they have done in response here. Doubters can make their own codes and report what they find--provided they have enough data and revelation of methods to do replicate or next-step studies, and authors who have been supported/challenged have enough info to judge the

analysis "fairly"--the latter being made by multiple peer reviewers' and editors' judgments of best practices.

This is a preview of how I am interpreting the still ongoing debate among my Editorial Board, and shortly--probably when I get home from my current trip to Japan on Feb 4--I will be more concrete. So please take this e-mail as a characterization of my interim thinking, but as this is a precedent changing policy for CLimatic Change, it will take a lot of debate among the board first--nearly completed I think. Thanks for your continuing input,
Steve S.

Jan 25, 2004 McIntyre to Schneider

From: Steve McIntyre

Sent: January 27, 2004 11:22 AM

To: Stephen H Schneider

Cc: Ross McKittrick

Subject: MBH Submission

Dear Prof. Schneider,

Thank you for your interesting comments. In my capacity as a reviewer, I requested "supporting calculations", including computer programs, for certain claims made in the MBH paper, which were based on calculations carried out by MBH, but not provided in the paper. You have obviously focussed on the request for computer programs, which I will discuss a bit later. Therefore (and I am numbering bring-forward items):

1. Would you please ask Professor Mann for the code in question, in case he has no objection to providing the code? Would you please then advise me of the response to this inquiry?

Perhaps Professor Mann has no objection to providing the code, in which case the matter should be completely moot. If Professor Mann is willing to provide the code and I am willing to review it, I hardly see how this does anything but enhance the quality of review for your journal or why, as a matter of policy, *Climatic Change* as a journal should prevent such review from taking place. An issue would only arise for *Climatic Change* if Professor Mann refused to provide the supporting calculations and codes involved and concurrently insisted on continuing to offer his article for publication.

I think that it is helpful to develop policy from concrete situations and to try to deal with specific situations first. I am not advocating that you that you change your publication policies to require peer reviewers to verify supporting calculations in all cases, if that is not your present policy, although it seems to me that this is a quite separate issue that bears some consideration.

If Professor Mann refused to provide the supporting calculations and codes involved and concurrently insisted on continuing to offer his article for publication, I think that one would have to consider the reasons that he offered for his refusal, rather than putting words into his mouth or anticipating grounds for such refusal. Some of the issues that you raise seem very unlikely to be raised in this particular case, as follows:

(a) I doubt that Professor Mann will claim that the provision of the code in this case would be onerous or in any way “an odious burden”. Any author, who uses computer code to generate results and graphics in a published article, should, as a simple matter of good housekeeping, archive a directory of scripts, programs and outputs used in the article. I expect that this is the case with Professor Mann. No more is required from Professor Mann than to copy this archive to an FTP location (perhaps password protected). This would take a few minutes only and hardly be an “odious burden”, if he has done his housekeeping. If he does claim that this would be an “odious burden”, you could point that out to him.

(b) I doubt that Professor Mann will claim that making his code available for inspection will be a “violation of intellectual property norms for authors” or that his code is “analogous to machines invented by copyrighters”. In the highly unlikely event that he made such a claim, one might point out to Professor Mann that he is, after all, seeking to *publish* his results and, if the protection of his intellectual property is a priority for him, he need not publish these results. But the idea that, in this particular case, there is any intellectual “property” in the codes is pretty laughable.

(c) I doubt that Professor Mann will express concern that his source code is specific to one particular computer system and might yield different results in another system. Professor Mann appears to write code in Fortran, which is highly transportable.

(d) I doubt that Professor Mann will refuse, because he might have to answer “endless questions”. Between making the calculations available for inspection and “endless questions”, there are obviously many intermediate cases, including the possibility of no comments whatever. If “endless questions” became a problem, I could see a cut-off point being reached. However, the mere making of supporting calculations available for inspection falls within any reasonable cut-off point. In fact, I view the delivery of code as actually simplifying methodological description. An accurate methods section would set out in words the steps of the computer algorithm. It seems to me simpler in many cases to simply produce the algorithm.

Please note that, by making this request in this instance, I am not advocating that you change your overall policy to require all reviewers to do the same. I happen to be in a position where I can readily carry out this form of due diligence and am prepared to do so. So why don't you simply ask Professor Mann for the supporting calculations and codes and see what happens. If Professor Mann refuses to make the supporting calculations and codes available, we can consider any reasons offered at that time.

I might also add that it is my belief that there may be important and material undisclosed procedures in Mann's computer programs. Professor Mann's verbal descriptions of his algorithms have tended to be somewhat incomplete and even inaccurate. His proposed article criticizes the algorithm in MM03 for not using detrended gridpoint standard deviations. But MBH98 explicitly states that the opposite method was used. We also point out an important undisclosed transformation of data carried out in MBH98 prior to the calculation of tree ring principal components. This is an extremely important and material data transformation, which we are aware of only because a computer program showing this transformation is on Mann's FTP site. This provides an obvious precedent of undisclosed material data transformations.

Even the stepwise calculation of principal components for tree ring regions is a material undisclosed method. Even after recent disclosure, this method cannot be accurately replicated without a statement of the exact number of principal components by region by interval, which are used.

I do not suggest that I regard “peer review” as a “legalistic standard of ‘truth’”. On the contrary, I am guided by the following concepts, which are applied in connection with securities offerings:

- “full, true and plain disclosure”
- “due diligence”

The trend in securities regulation has been towards ever increasing standards of disclosure. This does not guarantee “truth”, but it provides an objective standard for disclosure. The application of this standard has required much more copious and prompt disclosure than was the case 28 years ago. If someone wants to offer securities to the public, then “full, true and plain disclosure” is required. If they are not prepared to do so, then they need not offer securities. I think that this applies here. If Mann wishes to *publish*, which necessarily implies the dissemination of his views to the scientific public, then “full, true and plain disclosure” is mandatory. This reasonably includes making supporting code and calculations available for inspection by a reviewer, especially where a reviewer, as in this case, places you on notice that he believes that there may be material undisclosed data transformations.

Secondly, the concept of “due diligence” has important legal meaning for directors and officers who authorize a public offering. Among other things, it mandates the inclusion of audited statements and independent engineering reports. To some extent, there seems to be a view among the public that “peer reviewers” are carrying out a greater degree of due diligence than appears to be typical. Your statement of the expectations of the scope of report from a reviewer sounds all too much like the job description of a judge at a figure skating competition. In my experience, if there is some resistance to due diligence, the real reason is generally not the stated reason and it is usually a good idea to find out what’s going on. For you to advocate lower standards of due diligence than a reviewer is prepared to carry out is surely an unsupportable policy.

Since my request for supporting calculations seems to have been pigeon-holed into the request for computer programs, pending the decision on whether the computer programs are to be delivered,

2. I request that you make available all supporting data and calculations, up to the computer programs themselves, which should present no problem. For greater certainty, this includes the following (as well as matters raised in my prior comments not itemized below):

- a) all data output from calculations carried out in “experiments” or reconstructions, including all reconstructed temperature principal components in each “experiment”;
- b) all residual series from such experiments;
- c) Durbin-Watson statistics for all such experiments;
- d) Gridpoint standard deviations;

- e) Output from the “objective selection criteria” calculations;
- f) Output from calculations demonstrating the “insensitivity” of results to the use of summer rather than annual mean CET and Central European data, use of the 30-year fill for series #50 and correction of all misplaced geographical locations;
- g) Identification of all misplaced geographical locations and all other “errata”;

In the particular case of this submission, all assertions of the paper pertaining to “experiments” and calculations carried out by Professor Mann are, in the absence of such supporting calculations, really only bald assertions, based neither on the “analysis provided” or “cited literature”. I would therefore appreciate prompt delivery of these supporting calculations, as it has already been some time since the initial request was made. I would also appreciate prompt resolution of the issue of the code itself.

Regards,

Stephen McIntyre

Feb 4, 2004 Linda Mearns to Clim Chg Editorial Board

At 08:46 PM 2/4/04 +0100, Mearns Linda O wrote:

Peter et al.,

Thanks for reminding me about the new email list.

My point about the code is still that 'providing the code' can be interpreted alot of ways. I have thought about this, and imagined if in one of my larger and more complex projects, I was asked to provide all code. I could do that just by sending the pieces with a summary file explaining what each piece was used for. It still theoretically allows someone to see how coding was done. And I do think that is a far sight easier than providing stuff that can be run, etc. I am suggesting that one could do the minimum. Then the point is, one isn't faced with garish headlines about 'refusal to provide code'. I think it is harder to come up with a garish headline about 'refusal to provide completely documented code with appropriate readme files and handholding for running it'.

Linda

Feb 4, 2004 Peter Gleick to Clim Chg Editorial Board

At 11:40 04/02/2004 -0800, Peter H. Gleick wrote:

Yes, excellent point. This should be what we do. Further, we can point out that we've bent over backward here and provided more than typically necessary in order to satisfy persistent but inappropriate demands.

Peter

Dr. Peter H. Gleick

Director, 2003 MacArthur Fellow

Pacific Institute for Studies in Development, Environment, and Security

654 13th Street

Oakland, California 94612

Feb 6, 2004 Phil Jones to Clim Chg Editorial Board

<http://www.eastangliaemails.com/emails.php?eid=398>

From: Phil Jones <p.jones@xxxxxxxxxxx.xxx>

To: "Peter H. Gleick" <pgleick@xxxxxxxxxxx.xxx>, Mearns Linda O
<lmearns@xxxxxxxxxxx.xxx>

Subject: Re: MBH Submission (fwd)

Date: Fri Feb 6 10:58:17 2004

Cc: Stephen H Schneider <shs@xxxxxxxxxxx.xxx>, N.W.Arnell@xxxxxxxxxxx.xxx,
firtca@xxxxxxxxxxx.xxx, d.camuffo@xxxxxxxxxxx.xxx, scohen@xxxxxxxxxxx.xxx,
pmfearn@xxxxxxxxxxx.xxx, jfoley@xxxxxxxxxxx.xxx, harvey@xxxxxxxxxxx.xxx,
ahssec@xxxxxxxxxxx.xxx, Thomas.R.Karl@xxxxxxxxxxx.xxx, rwk@xxxxxxxxxxx.xxx,
rik.leemans@xxxxxxxxxxx.xxx, diana.liverman@xxxxxxxxxxx.xxx, mccarl@xxxxxxxxxxx.xxx,
lindam@xxxxxxxxxxx.xxx, rmoos@xxxxxxxxxxx.xxx, ogilvie@xxxxxxxxxxx.xxx,
pfister@xxxxxxxxxxx.xxx, barrie.pittock@xxxxxxxxxxx.xxx, pollard@xxxxxxxxxxx.xxx,
nj.rosenberg@xxxxxxxxxxx.xxx, crosenzweig@xxxxxxxxxxx.xxx, j.salinger@xxxxxxxxxxx.xxx,
santer1@xxxxxxxxxxx.xxx, h.j.schellnhuber@xxxxxxxxxxx.xxx, dgvictor@xxxxxxxxxxx.xxx,
F.I.Woodward@xxxxxxxxxxx.xxx, gyohe@xxxxxxxxxxx.xxx, yurganov@xxxxxxxxxxx.xxx

Dear All,

So now it seems that we're separating 'providing the code' from 'running the code'. I can't see the purpose of one without the other. Even if Mike complies I suspect there will need to be several sessions of interaction, which neither side will be very keen on. As I said before I know the code will involve lots of combinations (for different periods with different proxies).

Also I would expect, knowing the nature of the PC-type regression approach, that there will be library routines. If the code is sent, there needs to be conditions. We don't want McIntyre (MM) to come out and say he can't get it to work after a few days.

So, it is far some simple. I'm still against the code being given out. Mike has made the data available. That is all they should need. The method is detailed in the original paper - in the online (methods) and also in several other papers Mike has written.

As an aside, Mike is now using a different method from MBH98. Also, as an aside, whilst we've been deliberating, MM have submitted another comment on MBH98 to another journal. In this they say they have a program that replicates MBH98 (although it isn't very convincing that they have it exactly right, as they never show a like for like comparison) , but most of the comment goes on about the results being different due to different combinations of proxies. The latter isn't surprising.

It might appear they want the code to check whether their version works properly. If this is the case, then there are issues of IPR. So, if they get the code, how do we stop them using it for anything other than this review.

Cheers

Phil

Feb 11, 2004 McIntyre to Schneider

From: Stephen McIntyre
Sent: February 11, 2004 5:57 PM
To: Stephen H Schneider
Cc: Katarina Kivel; Ross McKitrick
Subject: FW: MBH Submission

Any progress on this with Professor Mann?

Feb 12, 2004 Schneider to McIntyre

From: Stephen H Schneider
Sent: February 12, 2004 4:33 AM
To: Stephen McIntyre
Cc: Katarina Kivel; Ross McKitrick; Katarina Kivel
Subject: Re: FW: MBH Submission

Hello. Last few comments are coming in from Editorial board--meanwhile not all reviews are in on MBH either, so no time is really being squandered--at Climatic Change review is typically 3-4 months, and I suspect this will be faster than average. I'll get back to everybody pretty soon with some suggestions to move forward here. Thanks for the inquiry, Steve S.

Feb 19, 2004 Schneider to McIntyre

From: Stephen H Schneider
Sent: February 19, 2004 2:18 AM
To: Steve McIntyre
Cc: Katarina Kivel
Subject: Re: submission to "Climatic Change"

Hello, and thanks for your patience in the question you raised about source code as part of the review process for the MBH submission I asked you to review. As I have telegraphed to you quite openly for a few weeks now, there has been a lively debate among the 30 or so members of the editorial board of Climatic Change on the issue of source code transfers to reviewers--a request I have not had in the 28 years I have been Editor.

I checked with other editors as well, and in main the atmospheric sciences and climatology journals (and Science, Nature and PNAS), to the extent I can glean, there are no policies requiring source codes to be made available--except it happens sometimes by the voluntary action of authors.

However, the CLimatic Change editorial board is firm in believing that authors must publish or make available by other means--typically websites these days--the data used and the methods/algorithms, so independent scientists so inclined can engage in replication studies--using codes of their own design for both independence and to prevent a major burden on authors--particularly with old codes or codes that are long and undocumented for others outside of their groups. Here are typical paragraphs from recent editorial board members on this topic:

I am persuaded that a standard of review probably does not extend into detailed audits of computer code, just as it would not extend into similar audits of medical tests, tree cores, trade models, demographic surveys, and archaeological digs. These activities would normally be pursued by other

researchers in other research fora and publications, not by reviewers providing opinion on manuscripts submitted for publication...I would not require the author to provide computer code to the reviewer, but to indicate literature sources that are used as the basis for methods used, as is standard practice.

ANother recent comment:

I agree with what seems to be the growing consensus that it is unreasonable to request authors to supply all their computer codes. This applies especially in cases where diverse approaches are needed for different data, and where expert judgement is needed re the quality of the data, its representativeness etc. It is likely that the real differences between the two contending sets of authors is over selection of the data, and both should be using expert judgement...Data selection is clearly the problem.

or another similar comment:

So in the present case I think a refusal to require the code to be handed over is reasonable and defensible on the grounds that the data is available, readily available codes are available to process data, and that expert judgement is the key to selecting reliable and relevant data.

Thus, the strong majority sentiment of the board is that authors must provide enough information for replication studies to be done independently--but that is not generally a reviewer responsibility to perform replication analyses--as a practical matter we'd have precious few pro bono reviewers if each were required to perform replication work on complex codes--theirs or anyone elses.

So in this case I will not at this time ask MBH to provide a source code on their website or for CLimatic Change review. However, given the unusual nature of your request, and my presumption you feel strongly about this, let me ask you then to take the step of spelling out very specifically why your performing a standard reviewing job requires a (quite old I think) source code that is not documented for general users to be provided. In other words, do you believe you do not have enough data or methods from MBH to perform a normal review--or even a replication study (like that you published in Energy and Environment?). I hope you can perform the review with the amount of information you have--a very typical practice for journals in this field--but if you have a clear argument that this review requires a source code, I would still be willing to listen to a very well specified request with reasons of course for this very unusual departure from our conventional practices in this field.

If I felt it was necessary, I might share this request with selected members of the editorial board or the scinetific community to get additional opinions on how they see your specific arguments relative to the generic advice from the editorial board to only require data and methods to be given by authors to reviewers or commenters.

In any case, from your early draft review without source code, I glean you will be able to provide a useful critique on MBH, but I will certainly entertain strong arguments for your unprecedented request for source code if you wish to make them.

With thanks for your helping to stimulate an important debate at
CLimatic Change,
Sincerely, Steve Schneider
Editor

Stephen H. Schneider
Professor, Dept. of Biological Sciences;
Co-Director, Center for Environmental Science and Policy
Stanford University
Stanford, CA 94305-5020 U.S.A.

Feb 24, 2004 McIntyre to Schneider

From: Steve McIntyre
Sent: February 24, 2004 4:48 PM
To: Stephen H Schneider
Cc: Katarina Kivel; Ross McKittrick
Subject: Re: submission to "Climatic Change"

Dear Dr. Schneider,

Thank you for your reply, which is, however, not fully responsive to my request. I have replied to the main issues in your email below.

1. You state:

there are no policies requiring source codes to be made available—
except it happens sometimes by the voluntary action of > authors.

Since you acknowledge that there are voluntary precedents, you can easily suggest to Professor Mann that he provide the code on a voluntary basis. As I mentioned before, it is possible that he will do so, in which case the matter becomes moot. I see no reason why journal policy as articulated would prevent you from making such a request. If Professor Mann refuses, then the implications of such refusal could be considered at that time. However, it seems premature to assume that he will refuse.

2. You state:

"the Climatic Change editorial board is firm in believing that authors must publish or make available by other means—typically > websites these days--the data used and the methods/algorithms, so > independent scientists so inclined can engage in replication > studies"

This is a commendable policy. In the MBH submission, they state:

We have set up a new website providing, in even more transparent format than that set up in 2000, the complete data and methodological details of the MBH98 analysis:
<ftp://holocene.evsc.virginia.edu/pub/Data/MANNETAL98>.

However, this URL is inactive at present and the above statement is obviously premature. Until the promised web disclosure is carried out, MBH have not complied with the above policy. I assume that ascertaining

compliance of the website with the above policy is part of the mandate of reviewers and that the review process cannot be finalized until this website is established. Please confirm this.

3. As you surmise, regardless of the outcome of (1) above, it is strongly my view that the computer code used to generate the diagrams and claims in MBH-CC (and MBH98) should be provided at a website. While I understand your editorial board does not wish to insist on provision of computer code as a general policy, it is my submission that the editorial board failed to consider whether such a requirement should be imposed in this particular case under these special circumstances (by focussing on the advantages and disadvantages of a more general requirement). The principal and very unflattering reason for such a requirement (in this particular case) is that Professor Mann has, in the past, failed to provide accurate descriptions of data, accurate data upon request and accurate descriptions of the methodology. Given this very unsatisfactory track record, it is not possible to rely on Professor Mann's written descriptions without specific verification of the computer code. While these comments may seem harsh, the following examples are readily verifiable. If you believe that the following examples are incorrect, I request that you forthwith inform me.

Examples of such prior inaccuracies include the following:

- a. The listing of series in the MBH Supplementary Information lists series NOT actually used in MBH98 calculations according to the evidence of Mann's website.
- b. The Stahle/SWM data at Mann's FTP site appears to include duplicate or spliced versions of several series (not reported in the MBH98 SI).
- c. Attributions of many series in MBH98 are inaccurate. For example, 22 instrumental temperature and precipitation series in MBH98 are attributed to Jones and Bradley (1992), but do not come from the cited source. The actual source is undocumented to this date.
- d. In addition to inaccurate attributions of these instrumental series, the geographical locations of many series, especially instrumental precipitation series are incorrect. Data from Paris, France is applied to New England. While MBH-CC claims that such errors are immaterial, MBH98 purports to carry out precise geographical reconstructions and, until contrary proof is provided, one can reasonably assume that a substantially incorrect geographical location will have an impact on the calculations somewhere.
- e. MBH 98 states, referring to temperature principal component calculations:
"For each grid-point, the mean was removed, and the series was normalized by its standard deviation."(page 786, column 1).

In their CC submission, MBH criticize MM03 as follows:

"MM03 also appear to have estimated gridpoint standard deviations from the un-detrended surface temperature data, while MBH98 had normalized their EOFs by detrended gridpoint standard deviations."

Obviously, MM03 followed the procedure explicitly stated in MBH98, while the procedure used in MBH98 (as described in MBH-CC) differs in this instance from the procedure described in MBH98/Nature. MBH-CC characterize the MM03 calculation as an "important technical error".

However, the problem (if any) originates with a false methodological description in MBH98.

f. MBH- CC states:

"MBH98 insure that each of the reconstructed Principal Components (RPCs) have the same variance as the corresponding instrumental annual mean temperature Principal Components (PCs) over the calibration period, based on an explicit scaling of each RPC series against its corresponding PC series over the 1902-1980 interval. If this step is not followed, qualitatively similar results are obtained, but cross-validation scores are found to be modestly lower."

Whatever the merits of this particular step in the algorithm, a careful reading of the corresponding description of methodology in MBH98 (see page 786, column 2) shows that there is no disclosure of this methodology in the four corners of MBH98 (nor is there in supplementary information nor was it disclosed in a private request for further particulars.)

- g. MBH98 stated that the calculation of temperature principal components used "conventional" methods. But conventional principal component algorithms fail with missing data. Some undisclosed method for filling in missing data was used and the description in MBH98 is commensurately inaccurate.
- h. Professor Mann has archived some Fortran computer code at Professor Mann's FTP site for tree ring principal component calculations. Inspection of the Fortran code shows that the data was transformed to a scaling on 1902-80 prior to calculation of principal components. There is no disclosure of this transformation in MBH98. This undisclosed (and in my opinion, complete unjustifiable) data transformation has a major effect on the calculation of tree ring principal components. In the absence of the archiving of this computer code, no one would ever have been able to figure out what was done.
- i. MBH98 states that they use a "stepwise" procedure, but do not provide an explicit listing of the actual "steps". Here is a recent posting from David Ball (a critic of M&M and supporter of MBH) on sci.environment[i]:

"One of the really frustrating things I'm finding about the MBH paper is that they are all over the map with their timesteps. In their points 1 to 7 on pages 781-782 they discuss steps from 1820-present, back to 1760, back to 1700, ... Meanwhile in discussing the temperature calibrations on page 783, they're talking about 11 from 1780-1980, 9 from 1760-1779, 8 from 1750-1759, 5 from 1700-1749, 4 from 1600-1699, 2 from 1450-1599, 1 from 1400-1449. Note the slight differences especially in the 1780-1980 period. These are relatively small, and still fit reasonably well with the timesteps on the UVA website. Trouble is, Mann says something different again in his rebuttal to M&M..."

j. In MBH-CC, MBH state:

"MBH98 calculated PCs of proxy sub-networks separately for each interval in their stepwise reconstruction. This is the only sensible approach, since it allows the use of all data available over each sub-interval used in the MBH98 stepwise reconstruction (i.e., one set of PCs for 1400-1980, another for 1450-1980, another for 1500-1980, and so on)."

However, in Mann's FTP site, principal component subdirectories are not available for each interval. For example, only one region (Stahle/SWM) has a subdirectory commencing in 1500. Whatever the actual procedure used, it is not the one described above (which was not itself described in MBH98 or the supplementary information thereto.)

- k. In MBH-CC, MBH state that they used 159 different series in MBH98. The figure of "159" series is nowhere used in MBH98 or supplementary information thereto. There is no conceivable method by which a reader of MBH98, even informed by access to Mann's current FTP site, could have deduced that 159 series were used. Even now, it is impossible to precisely deduce which series from the FTP site make up the 159 series. (To accurately specify the method, it is necessary to provide a table setting out the number of principal component series used for each tree ring interval for each calculation interval, together with the PC series subdirectory used for each calculation interval.) In addition to failing to provide this information at a website or in the publication, Professor Mann has refused to disclose the information upon specific request
- l. The disclosure of the number of indicators used in each interval in MBH98/Nature is inaccurate. For example, the Vaganov PC1 is stated in the SI to commence in 1450, but this is inconsistent with the proxy rosters.
- m. It seems highly probable to me that the figure of 159 series is inconsistent with MBH98/Nature disclosure of the number of proxies used in each interval or that the FTP site has failed to list all the principal component subdirectories used in MBH98.
- n. While MBH state their proxy data can be obtained from public archives at WDCP, the versions used in MBH98 frequently do not coincide with the versions archived at WDCP.
- o. Professor Mann provided a version of MBH98 to me which was faulty. Even in response to a direct inquiry as to the authenticity of this data set, Professor Mann failed to identify the faults or an alternative location to verify the data set. (I note once again that Professor Mann made untrue statements about my allegedly requesting data in a spreadsheet form.) Professor Mann also falsely stated that the version pproxy.txt was prepared in response to my request for data, when the file was dated much earlier on his FTP site and is referred to by Scott Rutherford over 2 years ago.

This is a long and lamentable list and is not exhaustive. Other examples are cited in MM03. Given the poor track record of Professor Mann in respect to inaccurate and incomplete disclosure, Climatic Change can easily justify much more stringent disclosure requirements on Professor Mann than it might require of another author, including the specific requirement of disclosing

computer programs at his website.

Again, I specifically and categorically re-iterate that I am not suggesting or proposing any changes to general Climatic Change editorial policy to require the archiving of computer code in every case. I presume that Climatic Change has NOT adopted a policy that it will never require the archiving of computer code. In my opinion, this is an extreme case. Any one of the above prior defects in disclosure would justify putting Professor Mann on an extremely short leash. The combination gives you no valid alternative other than to require the archiving of computer code in this particular instance.

4. One of the editorial board comments suggests that data selection is the only issue. This is not correct, since both sides have and continue to argue that differences in methodology are playing important roles. Indeed. The MBH submission is perhaps unusual relative to previous submissions, since it is focussed entirely on whether MM03 adequately replicated MBH98 methodology. You previously stated that reviewers are expected to ask about statements in a submission:

"do they follow logically from the analysis provided or cited in the literature"?

Applying this criterion provides an alternative ground for requiring MBH to disclose their computed programs. As matters currently stand, MBH make repeated methodological assertions that are simply bald assertions - and not grounded in any analysis provided within the submission or previous literature. The instances below replicate some of the points above, but illustrate this different point:

a. For example, MBH-CC state that MM03 did not follow "the procedure of MBH98 of calculating the PC series separately for all intervals used in their stepwise reconstruction. This procedure employs one set of PC series for the AD 1400-1980 interval, another for AD 1450-1980, and so on (see technical point "b" later on in this document), thus allowing for the use of an increasingly large amount of underlying proxy data in their representations of the various data sub-networks for later periods as more data joined the set." While this may or may not be an accurate characterization of MBH98 methodology, this is merely an assertion without any evidence, since MBH98 failed to disclose this procedure.

b. MBH-CC states: "we have re-computed northern hemisphere temperatures by incorporating the methodological and data changes outlined above, and obtain a qualitatively similar reconstruction to MM03 (Figure 2)". Again, as it stands, this is a mere assertion.

c. MBH-CC states: "MBH98 had normalized their EOFs by detrended gridpoint standard deviations." This is merely asserted. There is no evidence of this in MBH98, which, as noted above, states the opposite.

d. MBH-CC states: "MBH98 calculated PCs of proxy sub-networks separately for each interval in their stepwise reconstruction". Again, this is merely asserted, and there is no evidence of this in MBH98.

e. MBH-CC states: "Because the PCA representation of the various proxy networks was employed in this step-wise fashion, 159 independent time

series were required to represent all indicators used in the reconstructions of all the various sub-intervals, even though the maximum number ever used for a particular sub-interval is 112." Again, this is a mere assertion. There is no evidence of this in MBH98 or in any of the analysis provided.

f. MBH98 states: "The subsets were selected by MBH98 based on the application of an objective selection criteria to the specific available proxy networks available, and were optimized with respect to those networks." Again, there is no evidence of this in MBH98 or elsewhere.

g. MBH-CC states: "MBH98 insure that each of the reconstructed Principal Components (RPCs) have the same variance as the corresponding instrumental annual mean temperature Principal Components (PCs) over the calibration period, based on an explicit scaling of each RPC series against its corresponding PC series over the 1902-1980 interval". Again, there is no evidence of this in MBH98.

I find it very difficult to contemplate a method under which a reviewer could verify whether the above statements "follow logically from the analysis provided or cited in the literature", without inspecting computer code - mostly because the methodological description in the first instance was so defective. Had the original methodological description been satisfactory, one could now cite such description. But such expedients are now not possible and, thus, other than providing the computer code used in MBH98, I cannot think of any means of Professor Mann providing evidence for the above statements. This does not mean that the reviewer himself needs to inspect the code - merely that the reviewer has ensured that the code is available as evidence for the otherwise bald statements.

5. Please note that my outstanding requests are not limited to requests for computer code. In a prior email, I made the following request (pending disposition of the computer code issue):

"I request that you make available all supporting data and calculations, up to the computer programs themselves. For greater certainty, this includes the matters below (as well as matters raised in my prior comments):

- a. all data output from calculations carried out in "experiments" or reconstructions, including all reconstructed temperature principal components in each "experiment";
- b. all residual series from such experiments;
- c. Durbin-Watson statistics for all such experiments;
- d. Gridpoint standard deviations;
- e. Output from the "objective selection criteria" calculations;
- f. Output from calculations demonstrating the "insensitivity" of results to the use of summer rather than annual mean CET and Central European data, use of the 30-year fill for series #50 and correction of all misplaced geographical locations;

- g. Identification of all misplaced geographical locations and all other "errata";

To date, I have received no response to this request. It would be satisfactory (and indeed preferable) if this data were located on a website pursuant to the policy cited in (2) above, as opposed to being supplied to me directly. The provision of the above data is required both under policy (2) above and also because the supporting calculations requested above are used to make statements, which otherwise are merely baldly stated in the article (not following from analysis elsewhere in the article or otherwise in the literature), such as the results of various experiments specifically undertaken for this submission.

Additionally, in their submission (page 6), MBH stated that MBH98 used an obsolete version of the CRU temperature data and that this obsolete version did not contain only null values for certain data cells. Under the policy referred to in (2) above, please provide me with a URL for the temperature data set actually used. This applies also to the "gridpoint standard deviations" mentioned above as well. A description of the "objective selection criteria" calculations referred to in (e) above is also required.

While this comment is outside the scope of this review, may I take the liberty of suggesting that the World Data Center for Paleoclimatology is a much more adequate means of archiving data than an author's own website. The American Geophysical Union, as publisher of GRL and JGR, has policies prohibiting the use of such private archives, likening them to "gray" literature. Acquiescence in archival methods explicitly rejected by AGU for GRL and JGR would seem to be inconsistent with your commendable objective for Climatic Change is to be a "journal of record".

The temperature history in MBH98 has been widely applied for purposes of public policy. Even if one could justify a refusal to provide adequate methodological disclosure for the reasons, which your editorial board have put forward (which I mostly find unacceptable), once the graphic has been applied to public policy, copious and exhaustive disclosure is surely mandated. Obviously, some of my particular interest in adequate disclosure by Professor Mann of his methodology (including computer code) pertains to outstanding controversy. Some of your editorial board (and perhaps you yourself) seem to take the position that analysis of methodology pertains properly to such controversy and is outside the scope of a review per se. I obviously have many points to make in respect to this article, some of which may be controversial. But, in my capacity as a reviewer, surely I am both entitled to and obliged to verify whether disclosure within and accompanying the submission is adequate for such further consideration.

An author confident of his results would willingly provide complete disclosure and one can only wonder at Professor Mann's apparent reluctance to provide complete disclosure or why your journal would condone it. I am perhaps influenced by own experience with prospectuses, where "full, true and plain disclosure" is a legal obligation. I believe that this is a useful standard for scientific matters as well, especially where there is application to public policy. I see no reason why Climatic Change should be content with anything less than "full, true and plain disclosure". I trust that the reasoning here will dissuade you from acquiescing in the lesser and inadequate disclosure that you appear to be contemplating.

Regards,
Stephen McIntyre

Mar 22, 2004 McIntyre to Schneider

From: Steve McIntyre
Sent: March 22, 2004 3:31 PM
To: Stephen H Schneider
Cc: Katarina Kivel; Ross McKitrick
Subject: Fw: submission to "Climatic Change"

Hi, how are you coming along with getting the supporting calculations and related requests from Dr. Mann? Regards, Steve McIntyre

Mar22, 2004 Schneider to McIntyre

From: Stephen H Schneider
Sent: March 22, 2004 5:31 PM
To: Steve McIntyre
Cc: Katarina Kivel; Ross McKitrick
Subject: Re: Fw: submission to "Climatic Change"

Hello and thanks for the inquiry. The request was sent to the MBH team several weeks ago and as is our practice, they will be reminded to reply in the next week. Things at Cl Ch move inexorably, but slowly. Soon we shall see how to proceed. Steve S.

Mar 29, 2004 McIntyre to Schneider

From: Steve McIntyre
Sent: March 29, 2004 10:35 AM
To: Stephen H Schneider
Cc: Ross McKitrick
Subject: Re: submission to "Climatic Change"

Dear Dr. Schneider,

On another topic, I am looking for digital versions of the Dundee, Guliya and Dasuopu data reported in Thompson et al. (2003), Climatic Change 59, which I have been unable to locate at the World Data Center for Paleoclimatology or elsewhere and was unsuccessful in requesting the data from the authors. In keeping with the following policy:

However, the Climatic Change editorial board is firm in believing that authors must publish or make available by other means--typically websites these days--the data used and the methods/algorithms

could you request Dr. Thompson to publish this data forthwith, preferably at WDCP.

Thanks, Steve McIntyre

Apr 13, 2004 McIntyre to Schneider

From: Steve McIntyre
Sent: April 13, 2004 11:49 AM
To: Stephen H Schneider
Cc: Ross McKitrick
Subject: Fw: quote of the day from Professor Stephen Schneider

Dear Dr. Schneider,

I agree with your view that an honest climate debate is very much to be desired. One step toward achieving honesty is full, true and plain disclosure of methodologies, as I've requested from Professor Mann, but which he has refused to provide.

How are you coming along with obtaining supporting calculations from Professor Mann? and with requesting the underlying computer programs?

Regards,
Steve McIntyre

Apr 13, 2004 Schneider to McIntyre

From: Stephen H Schneider
Sent: April 13, 2004 2:19 PM
To: Steve McIntyre
Cc: Ross McKittrick; katarina kivel
Subject: Re: Fw: quote of the day from Professor Stephen Schneider

Hello and thanks for the newstory--you are certainly up to date--I never saw it before this, not possible for me to be following news stories in real time. Indeed, that is a fairly accurate set of quotes from the talk I just gave--and what I usually say each time.

We have just received a long response from MBH, but Katarina is out for a day so I will get to it later this week. I suspect he proposed to put materials up to do replications, but let me not speculate until I actually get a download later this week. ShS

Apr 16, 2004 McIntyre to Schneider

From: Steve McIntyre
Sent: April 16, 2004 12:47 PM
To: Stephen H Schneider
Subject: Fw: AER submission policy

----- Original Message -----

From: "Ross McKittrick"
To: "Steve McIntyre"
Sent: Friday, April 16, 2004 10:59 AM
Subject: AER submission policy

> Dear Steve
> The American Economic Review--our very top journal, recently reiterated
> its submission policy for empirical papers in light of a recent attempt
> by 2 economists to replicate some papers. At
> <http://www.aeaweb.org/aer/submissions.html>
>
> it states
> It is the policy of the American Economic Review to publish papers only
> if the data used in the analysis are clearly and precisely documented
> and are readily available to any researcher for purposes of replication.
> Details of the computations sufficient to permit replication must be
> provided. The Editor should be notified at the time of submission if the
> data used in a paper are proprietary, or if, for some other reason, the
> above requirements cannot be met.

>
> The Editors today published the following statement:
>
> In an article recently published in this journal, B. D. McCullough and H.
D. Vinod (2003) report that they tested the replication policy by
soliciting data and code from authors of articles appearing
> in the June 1999 issue. While some authors responded in a timely and
> forthcoming way, not all did, according to McCullough and Vinod.
Replicability is essential if empirical findings are to be credible
> and usable as a starting point for other researchers. The findings of
> McCullough and Vinod have led us to conclude that more active enforcement
of the replication policy by this journal is necessary.

Submitters should be aware that the Editors now routinely require, as a
condition of publication, that authors of papers including empirical
results (including simulations) provide to this office, in electronic form,
data and code sufficient to permit replication.

Exceptions, for proprietary data for example, must be approved by the
Editor in advance of the review process. Electronic data appendices will be
posted on the journal's Web site,

> <http://www.aeaweb.org/aer/>.

Authors of previously published articles who wish to submit a data appendix
for posting are

> welcome to do so. In any case, published authors are strongly encouraged
> to comply with the

> replication policy, should requests for data and programs be received.

> The Editors should be notified

> if an author of a previously published article does not make a

> reasonable effort to comply with the

> journal's replication policy

> Ben S. Bernanke, Editor

>

>

> --

> Ross McKittrick

>

> Associate Professor of Economics

> The University of Guelph

> (519) 824-4120 x52532

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> School of Business and Economics

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> (519) 884-1970 x2281

May 7, 2004 Schneider to McIntyre

From: Stephen H Schneider
Sent: May 7, 2004 4:12 AM
To: Steve McIntyre
Cc: Katarina Kivel
Subject: Re: submission to "Climatic Change"

Hello and thanks for waiting for me to send the response from MBH to your request of a few months back. I attach two responses MBH sent, along with a cover letter from me to you. I am hopeful that you will now be better able to complete your review, though not all items you requested--in particular source code--are included. However, a considerable amount more data and description of algorithms, that should be useful both to you and anyone else intersted in the MBH methods and results, are. As noted in the cover letter, if you believe that more information is still needed, or that information mentioned in the response is somehow not obtainable, please let me know and I'll consider any such additional requests. With thanks for your efforts, Steve S.

Stephen H. Schneider
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Climatic Change

An Interdisciplinary, International Journal Devoted to the Description, Causes and Implications of Climatic Change

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30 April 2004

Dr. Stephen McIntyre
512-120 Adelaide St., West
Toronto, Ontario
Canada M5H 1T1

Dear Dr. McIntyre,

Authors Mike Mann, Ray Bradley, and Malcolm Hughes have now responded to your requests and have posted additional data on several web sites, which should help you to prepare your review of their manuscript, as well as be useful for anyone wanting to do further analysis on

this topic, since these postings are supposed to be generally available. I attach documents on this that they have sent to me.

Two additional reviews have also come in (one more to go—plus yours), so we are reaching the end of the review process at this time and I will be pleased to receive your referee's comments as well in due course, hoping that this additional information attached to this e-mail will improve your capacity to do the kind of review you would like—though it does not go as far as providing a source code. Remember, at Climatic Change a reviewer is not normally expected to perform a full replication exercise, nor are authors normally required to provide source codes. But if you wish to submit a Comment later on—should the Mann et al. paper eventually be accepted if the authors respond adequately to all the reviewers' comments, including yours—a more thorough analysis approaching a broader replication exercise may well be an option you'd consider attempting.

If you feel that any further information from MBH is necessary or some of the information they say they have provided is incomplete or inaccessible, please do not hesitate to contact me with such a request, and I will consider it, as I did the last such request, which produced the attached documents.

Thank you for preparing your review and participating in the process. I look forward to the receipt of your referee's report.

Sincerely,

Stephen H. Schneider,
Editor

SM NOTE - Attachments 1. AlgorithmDescription.txt with URL
 2. Reply

RESPONSES TO ISSUES RAISED BY REVIEWER A

Even after recent disclosure, this method cannot be accurately replicated without a statement of the exact number of principal components by region by interval, which are used.

This information is available here:

<ftp://holocene.evsc.virginia.edu/pub/sdr/temp/nature/MANNETAL98/MASTER>
<ftp://holocene.evsc.virginia.edu/pub/sdr/temp/nature/MANNETAL98/PROXY/README>

- a) *all data output from calculations carried out in "experiments" or reconstructions, including all reconstructed temperature principal components in each "experiment";*

It is not our responsibility to provide "all data output from calculations carried out...", we have neither the time nor the inclination to do so. These can be readily produced by anyone seeking to reproduce our analysis, based on the data we have made available, and our method which we have described in detail. Reconstructed Principal Components are available here:

<ftp://holocene.evsc.virginia.edu/pub/sdr/temp/nature/MANNETAL98/FIGUREDATA/README>

b) *all residual series from such experiments;*

see response to "a".

c) *Durbin-Watson statistics for all such experiments;*

We did not describe such statistics in our study.

d) *Gridpoint standard deviations;*

These are available, along with the raw instrumental temperature data used in our study, here:
<ftp://holocene.evsc.virginia.edu/pub/sdr/temp/nature/MANNETAL98/INSTRUMENTAL/>

e) *Output from the "objective selection criteria" calculations;*

see response to "a".

f) *Output from calculations demonstrating the "insensitivity" of results to the use of summer rather than annual mean CET and Central European data, use of the 30-year fill for series #50 and correction of all misplaced geographical locations;*

Any errors in descriptions of data series in the original supplementary information are described in the Nature supplementary information correction, and on the website:

<ftp://holocene.evsc.virginia.edu/pub/sdr/temp/nature/MANNETAL98/PROXY/mbh98datasummaryadd-delete.txt>

<ftp://holocene.evsc.virginia.edu/pub/sdr/temp/nature/MANNETAL98/PROXY/README>

<ftp://holocene.evsc.virginia.edu/pub/sdr/temp/nature/MANNETAL98/PROXY/mbh98datasummary.txt>

We are baffled by the request that we demonstrate "insensitivity" to the use of the particular versions of the CET and Central European records in our study, when the interval during which McIntyre and McKittrick obtain a different estimate, as verified independently by the Swiss group as noted earlier, is confined to the pre-AD 1500 interval, predating either series!

g) *Identification of all misplaced geographical locations and all other "errata";*

see response to "f".

This is a commendable policy. In the MBH submission, they state:

We have set up a new website providing, in even more transparent format than that set up in 2000, the complete data and methodological details of the MBH98 analysis: <ftp://holocene.evsc.virginia.edu/pub/Data/MANNETAL98>.

However, this URL is inactive at present and the above statement is obviously premature.

See <ftp://holocene.evsc.virginia.edu/pub/sdr/temp/nature/MANNETAL98/> and supplementary information to be provided along w/ Nature correction.

a. *The listing of series in the MBH Supplementary Information lists series NOT actually used in MBH98 calculations according to the evidence of Mann's website.*

See:

<ftp://holocene.evsc.virginia.edu/pub/sdr/temp/nature/MANNETAL98/PROXY/mbh98datasummaryadd-delete.txt>

<ftp://holocene.evsc.virginia.edu/pub/sdr/temp/nature/MANNETAL98/PROXY/README>

<ftp://holocene.evsc.virginia.edu/pub/sdr/temp/nature/MANNETAL98/PROXY/mbh98datasummary.txt>

b. *The Stahle/SWM data at Mann's FTP site appears to include duplicate or spliced versions of several series (not reported in the MBH98 SI).*

See response to "f".

c. *Attributions of many series in MBH98 are inaccurate. For example, 22 instrumental temperature and precipitation series in MBH98 are attributed to Jones and Bradley (1992), but do not come from the cited source. The actual source is undocumented to this date.*

See response to "f".

d. *In addition to inaccurate attributions of these instrumental series, the geographical locations of many series, especially instrumental precipitation series are incorrect. Data from Paris, France is applied to New England. While MBH-CC claims that such errors are immaterial, MBH98 purports to carry out precise geographical reconstructions and, until contrary proof is provided, one can reasonably assume that a substantially incorrect geographical location will have an impact on the calculations somewhere.*

See response to "f".

e. *MBH 98 states, referring to temperature principal component calculations:*

"For each grid-point, the mean was removed, and the series was normalized by its standard deviation."(page 786, column 1).

In their CC submission, MBH criticize MM03 as follows:

"MM03 also appear to have estimated gridpoint standard deviations from the un-detrended surface temperature data, while MBH98 had normalized their EOFs by detrended gridpoint standard deviations."

Obviously, MM03 followed the procedure explicitly stated in MBH98, while the procedure used in MBH98 (as described in MBH-CC) differs in this instance from the procedure described in MBH98/Nature. MBH-CC characterize the MM03 calculation as an "important technical error". However, the problem (if any) originates with a false methodological description in MBH98.

See enhanced description of methods, provided on supplementary website:

<ftp://holocene.evsc.virginia.edu/pub/sdr/temp/nature/MANNETAL98/METHODS/>

f. MBH- CC states:

"MBH98 insure that each of the reconstructed Principal Components (RPCs) have the same variance as the corresponding instrumental annual mean temperature Principal Components (PCs) over the calibration period, based on an explicit scaling of each RPC series against its corresponding PC series over the 1902-1980 interval. If this step is not followed, qualitatively similar results are obtained, but cross-validation scores are found to be modestly lower."

Whatever the merits of this particular step in the algorithm, a careful reading of the corresponding description of methodology in MBH98 (see page 786, column 2) shows that there is no disclosure of this methodology in the four corners of MBH98 (nor is there in supplementary information nor was it disclosed in a private request for further particulars.)

See enhanced description of methods and

<ftp://holocene.evsc.virginia.edu/pub/sdr/temp/nature/MANNETAL98/METHODS/README>

g. MBH98 stated that the calculation of temperature principal components used "conventional" methods. But conventional principal component algorithms fail with missing data. Some undisclosed method for filling in missing data was used and the description in MBH98 is commensurately inaccurate.

This question is based on a misunderstanding the instrumental dataset we actually used – of course we did not attempt to calculate principal components on a dataset with missing values. See

<ftp://holocene.evsc.virginia.edu/pub/sdr/temp/nature/MANNETAL98/INSTRUMENTAL/README>

And

<ftp://holocene.evsc.virginia.edu/pub/sdr/temp/nature/MANNETAL98/INSTRUMENTAL/ anomalies-new>

i. MBH98 states that they use a "stepwise" procedure, but do not provide an explicit listing of the actual "steps".

The timesteps used in the calculation may be found at:

<ftp://holocene.evsc.virginia.edu/pub/sdr/temp/nature/MANNETAL98/PROXY/README>

j. In MBH-CC, MBH state:

"MBH98 calculated PCs of proxy sub-networks separately for each interval in their stepwise reconstruction. This is the only sensible approach, since it allows the use of all data available over each sub-interval used in the MBH98 stepwise reconstruction (i.e., one set of PCs for 1400-1980, another for 1450-1980, another for 1500-1980, and so on)."

However, in Mann's FTP site, principal component subdirectories are not available for each interval. For example, only one region (Stahle/SWM) has a subdirectory commencing in 1500. Whatever the actual procedure used, it is not the one described above (which was not itself described in MBH98 or the supplementary information thereto.)

See enhanced description of methods and

<ftp://holocene.evsc.virginia.edu/pub/sdr/temp/nature/MANNETAL98/PROXY/README>

k. In MBH-CC, MBH state that they used 159 different series in MBH98. The figure of "159" series is nowhere used in MBH98 or supplementary information thereto. There is no conceivable method by which a reader of MBH98, even informed by access to Mann's current FTP site, could have deduced that 159 series were used. Even now, it is impossible to precisely deduce which series from the FTP site make up the 159 series. (To accurately specify the method, it is necessary to provide a table setting out the number of principal component series used for each tree ring interval for each calculation interval, together with the PC series subdirectory used for each calculation interval.)

See enhanced description of methods and

<ftp://holocene.evsc.virginia.edu/pub/sdr/temp/nature/MANNETAL98/PROXY/README>

l. The disclosure of the number of indicators used in each interval in MBH98/Nature is inaccurate. For example, the Vaganov PC1 is stated in the SI to commence in 1450, but this is inconsistent with the proxy rosters.

See response to "f" above and:

<ftp://holocene.evsc.virginia.edu/pub/sdr/temp/nature/MANNETAL98/PROXY/README>

m. It seems highly probable to me that the figure of 159 series is inconsistent with MBH98/Nature disclosure of the number of proxies used in each interval or that the FTP site has failed to list all the principal component subdirectories used in MBH98.

See <ftp://holocene.evsc.virginia.edu/pub/sdr/temp/nature/MANNETAL98/PROXY/README>

n. While MBH state their proxy data can be obtained from public archives at WDCP, the versions used in MBH98 frequently do not coincide with the versions archived at WDCP.

Authors can only describe where they got the data they used, and cannot anticipate what changes may occur after publication. In any case, the data we used are now to be found at: <ftp://holocene.evsc.virginia.edu/pub/sdr/temp/nature/MANNETAL98/PROXY/README>

May 22, 2004 McIntyre to Schneider

From: Steve McIntyre

Sent: May 22, 2004 2:27 PM

To: Stephen H Schneider

Cc: Ross McKittrick

Subject: Referee Report on Mann et. al. Comment on MM03

Dear Dr. Schneider,

Thank you for your email of May 7, 2004, enclosing Professor Mann's response.

The response is very unsatisfactory. The MB& H paper does not meet mandatory CC policies in the following respects and should not be accepted:

- 1) Professor Mann has refused to disclose key data, including even the output of his "experiments".
- 2) The submission is not up-to-date or "state of the art". It fails to consider the critical arguments related to key indicators, especially on principal components methodology, which are the subject of an exchange now at 2nd review at *Nature*.
- 3) There are many factual errors, method inaccuracies and disclosure defects. This is not the first such occasion for these authors, as *Nature* has recently provided notice to them to issue a Corrigendum with respect to data descriptions in MBH98.

In the event that, despite the above defects, you wish to request re-submission by Professor Mann, please obtain responses to the various matters set out in the referee's report attached hereto.

Professor Mann argued that the provision of requested data was burdensome. I disagree with this. As I have constructed an emulation of MBH98 method, I am completely familiar with what is involved in delivery of the requested data. The data files could be produced in no more than a couple of minutes and the statistics in only a couple of more minutes. It takes more time to argue about it than to produce them. But even if some time were involved in the production

of data resulting from the “experiments”, production is obligatory both under CC policies and reasonable practices.

I note that the attached review has been carried out without the source code, which you did not agree to request (although you said that this might be re-considered with cause). If there is to be further consideration of a re-submission, I re-iterate my request that the source code be provided. The attached report identifies many discrepancies between stated procedures and data and actual procedures and data. These are all problems, which remain even after the supposed correction of errors reported in a recent Corrigendum requested by Nature. Of the matters dealt with in the Corrigendum, the reported use of series that were not actually used seems inexcusable to me. Under the circumstances, it is impossible to have any confidence in the integrity of the data set and methods without a careful examination of source code and I request that you make this source code available, even if all of the above matters are resolved in a re-submission.

While I acknowledge that it is not an obligation of a reviewer to attempt replication, it seems to me that one of the most important jobs that a reviewer can do is to ensure that the submitting author’s provision of data and methods enable replication, or at a minimum, comply with applicable journal guidelines on archiving and description. In this case, the submitting authors have complied with neither obligation.

In my opinion, policies on archiving of data and description of methods are very inadequate in most paleoclimate publications. I believe that the guidelines of CC (and other journals dealing in paleoclimate matters) should be made more stringent to require the archiving of source code at the time of publication, which would deal with most deficiencies in verbal descriptions of methods. While you have expressed concerns about the burden that adequate disclosure would place on submitting authors, you should also consider the burden that is placed on the reader or potential replicator without this availability. I note that American Economic Review has adopted policies ensuring much broader disclosure, including source code, at the time of publication, as shown below.

In an article recently published in this journal, B. D. McCullough and H. D. Vinod (2003) report that they tested the replication policy by soliciting data and code from authors of articles appearing in the June 1999 issue. While some authors responded in a timely and forthcoming way, not all did, according to McCullough and Vinod. Replicability is essential if empirical findings are to be credible and usable as a starting point for other researchers. The findings of McCullough and Vinod have led us to conclude that more active enforcement of the replication policy by this journal is necessary.

Submitters should be aware that the Editors now routinely require, as a condition of publication, that authors of papers including empirical results (including simulations) provide to this office, in electronic form, data and code sufficient to permit replication. Exceptions, for proprietary data for example, must be approved by the Editor in advance of the review process. Electronic data appendices will be posted on the journal’s Web site, <http://www.aeaweb.org/aer/>.

...The Editors should be notified if an author of a previously published article does not make a reasonable effort to comply with the journal’s replication policy

Ben S. Bernanke, Editor

I would urge you to consult the McCullouch and Vinod article, as well as the Dewald et al. article cited therein, as they have been pertinent comments on the role of journals in ensuring replication.

I hope that these comments and the attached report are helpful to you. I have read many interesting CC articles and have been happy to be of any small assistance to CC in this matter.

Yours truly,
Stephen McIntyre

REFEREE COMMENTS

A. Refusal to Supply Data

You stated that CC has a high standard of disclosure and, this involved, at a minimum:

providing all data & clear statements of methods/algorithms used...

In response to a specific request from CC, Professor Mann has refused to provide the data resulting from his “experiments”. In your cover letter, you asked whether I would be able to complete my review in the absence of this data. Arguably, a reviewer should not proceed past this step. The refusal to provide data is a clear breach of mandatory CC policies. In the cover letter hereto, I have pointed out policies of the American Economic Review, which effectively do not permit a review process to commence until provision of not only data, but source code have been provided. I re-emphasize that the failure to provide data is a distinct issue from the failure to provide source code.

B. Failure to consider most recent materials

CC guidelines to reviewers require that the submission be “state of the art”. The submission by Mann et al. fails to consider the exchange between ourselves and Mann et al. presently under review at *Nature*, although the SI makes a veiled reference to this exchange by referring to Mann et al. (under review). This exchange is now at 2nd review at *Nature*.

Aside from the failure to consider the most recent material on the topic, it would be inappropriate to publish one side of the debate pre-emptively in *Climatic Change*, especially since at the first round the *Nature* referees did not find the MBH rebuttal persuasive. In particular, the submission (see section 2) fails to consider the following major points, which are the heart of the *Nature* exchange:

1. we argued that the TTHH proxy is irrelevant to early 15th century values since it doesn't begin until 1459 and any difference in handling this proxy does not affect early 15th century values. Point 3 of section 2 does not deal with this.
2. we argued that quality defects in the 15th century portion of the SWM network (see also below) prevented its use in that period. In their reply submitted to *Nature*, Mann et al. resiled from their earlier position that this was a “key indicator” (although this position is still held in the present submission) and now stated that the SWM PC1 had an immaterial effect on early 15th century values – a conclusion with which we agree. Point 2 in section 2 does not deal with this.

3. we argued that there was a significant problem arising from the undisclosed extrapolation of the Jacoby Gaspé chronology, which had an effect on the MBH98 temperature index inconsistent with robust statistical methods. This point is not addressed in Point 3 of section 2.
4. we argued that there was a methodological error in the calculation of proxy principal components in MBH98 through standardization on short segments, which resulted in the overweighting of the problematic Graybill-Idso cambial dieback sites. It is the short-segment standardization, rather than the presence or absence of the NOAMER PC1 that affects early 15th century values. Mann et al. conflate two entirely distinct issues. We illustrate this in the figure below. Panel a shows our emulation of the MBH98 temperature reconstruction; b shows our emulation of MBH98 without the short-segment data transformation and without the Gaspé extrapolation, but *with* NOAMER PC1 and PC2 back to 1400. Panel c shows the same as Panel b but with the NOAMER PCs truncated at 1500. Clearly, if a conventional PC methodology is used – without short-segment standardization- the presence or absence of this indicator actually has relatively little effect. Point 1 in section 2 does not deal with the matter of PC methodology or short-segment standardization at all. This is a fundamental issue and needs to be addressed.

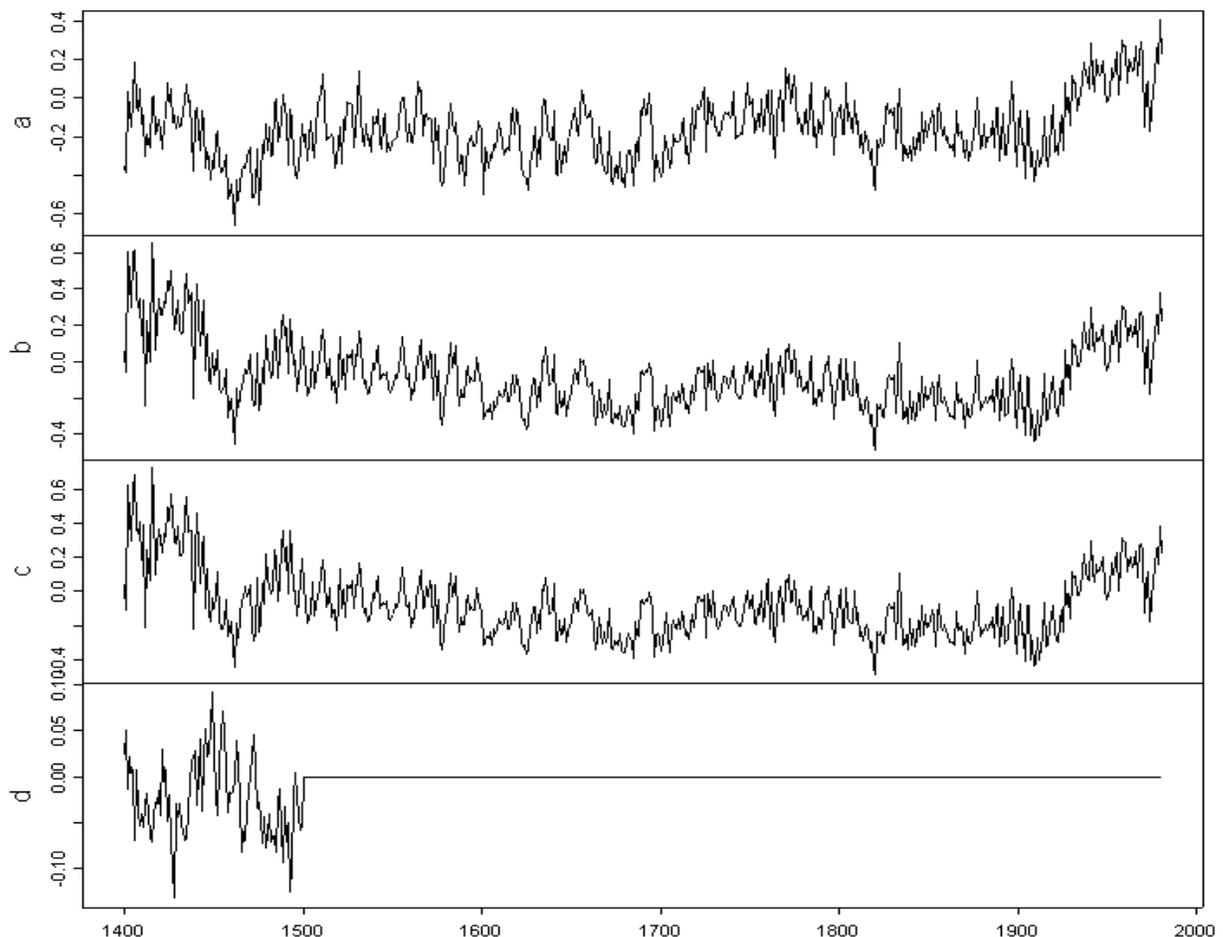


Figure 1. a: authors' emulation of Northern Hemisphere temperature index from MBH98. b: authors' calculation of North American temperature index using conventional calculation of NOAMER PC1 and without Gaspe extrapolation; c: same calculation omitting NOAMER PC1 in the 15th century; d: difference between (b) and (c). Methodology for all panels is identical. (Base 1902-1980)

C. Factual Errors, Method Inaccuracies and Disclosure Defects

In your first communication to me inviting me to review the MBH submission for Climatic Change, you stated:

The procedures at Climatic Change are to be sure, first, technical discussions are state-of-the-art and methods appropriate and accurate.

and also that:

technical or logical errors or factual misinterpretations ... always must be corrected by authors in revisions – often several revisions.

There are many factual errors and method inaccuracies in the present submission, many of which were stated in my January email. There was no discussion of the points raised in the prior email in the response by Mann et al. I have itemized an appendix listing various factual errors and method inaccuracies within CC policies, both in the paper and the SI, and have noted requests for key data and computational details (highlighted in yellow for convenience in the current requests, but not in the replicated January 6, 2004 email, which also contains response items).

1. **Zorita et al.:** Mann et al. claim in various places that Zorita et al. have carried out a replication of MBH98, suggesting that the disclosure of methods and data to date is somehow sufficient. Zorita et al. do not use **any** proxy data and obviously cannot stand as authority for replication. All references to Zorita et al. as having provided replication for MBH98 should be **deleted**.
2. **Identification of 159 Series.** In the original submission, Mann et al. state that:

Because the PCA representation of the various proxy networks was employed in this step-wise fashion, 159 independent time series were required to represent all indicators used in the reconstructions of all the various sub-intervals, even though the maximum number ever used for a particular sub-interval is 112.

There was no reference to this figure of 159 series in MBH98, and no way to identify the files on the old FTP site. I requested a listing directly from Dr. Mann, which he refused to provide. Surprisingly this information is still not available on the new FTP site. The new SI confirms the use of 11 calculation intervals and contains a listing of the series used in each of the 11 calculation intervals (“datalists”). But according to my count, there are only 81 non-PC and 58 PC series, making a total of 139 series, not 159. I have attached a listing (Appendix B). So the claim that 159 series were used in MBH98 remains unsubstantiated. **Please provide a listing of the 159 series or correct this figure.**

3. The datalist for AD1500 does not include 5 direct-use proxy series which are used in the AD1450 datalist and excludes one available direct-use series. **Please explain.**
4. **Procedure for Stepwise Calculation of Proxy Principal Components:** Elsewhere in the submission, they state:
 MBH98 calculated PCs of proxy sub-networks *separately* for *each* interval in their stepwise reconstruction. *[my emphasis]*

This is not correct. Inspection of the PC series as actually used shows that in some cases, the PC used in the calculation interval is from the contemporary PC network while in other cases, it is from an older network. There is no explanation why PCs on proxy sub-networks were calculated only for some intervals and not other, but in any event, the second statement cited above needs to be replaced with an accurate statement of the principle used for selection of the BACKTO period used for a calculation interval.

5. **Regional PC Initiation.** In the algorithm description in the new SI, Mann states the following:
 PCs were no longer calculated back in time once a given network contained fewer than 7 available series (with the exception that PCs were calculated for the 'Stahle Southwest U.S./Mexico network' with 6 series available).

The following table shows, for each region, the first year in which there were 7 series available, the first calculation interval used and the first calculation interval in which 7 series were available. The SOAMER and AUSTRAL intervals do not square with this criterion. The above description is obviously incorrect and needs to be replaced with an accurate description of the principle for deciding when to initiate PCs.

Region	First Year in which 7 series available	First Calculation Interval with PC	First Calculation Interval in which 7 series available
Stahle/OK	1681	1700	1700
Stahle/SWM	1475	1400	1500
NOAMER	1400	1400	1400
SOAMER	1418	1600	1450
AUSTRAL	1403	1600	1450
VAGANOV	1413	1450	1450

6. **Problems in the SWM Data Descriptions:** Mann et al. made a special exemption for the SWM network, but there are several problems here as well. The original SI listed 10 sites (20 series) for the SWM network. Mann now says that 11 sites (22 series) were used and attributes the extra 2 series to Stahle (pers.comm.) and has referred to this in the Nature Corrigendum. The start dates of the two Stahle pers. comm. series are incorrectly stated at 1376 in the new SI, when they are in fact 1375. Further, the first 120 values of earlywood series #11 are identical to the first 120 values of earlywood series #1 (and similarly for latewood series #11 and #1.) Obviously, it is impossible for this to happen with series from different sites. Either Stahle has sent Mann two versions from the same site or someone spliced values from one series into another. Note that these series were deemed so

exceptionally important that they got a 100-year exemption from the ‘7 series’ rule so they could enter the 15th century calculation, yet even now the data source is a mere “pers. comm.” and none of these series can be verified at WDCP. Please provide an explanation for the identity of early values of these series, identification of the location of series now described as pers.comm.. and affirmation that these series are not duplicate or spliced versions.

7. **Temperature Gridcell Selection:** Mann has provided the gridpoint standard deviations for the 1082 gridcells (item e in my data request), together with the underlying CRU dataset and a readme for the instrumental data, which states:

MBH98 made use of all nearly continuous monthly gridpoint surface temperature records (no single gap greater than 24 months, and no more than 10 years of total missing data. Any remaining gaps were linearly interpolated.

By my count these two conditions were not met in 242 of the 1082 specified gridcells in the temperature data set provided. For each cell actually used, please provide a count of the number of missing months and the longest NA run.

8. **Location of 219 Long Nearly Continuous Cells:** I plotted the location of the 219 gridcells with the greatest number of observations in the period 1854-1993 and compared it to the corresponding figure from *Nature*. While there is similarity between the two figures, there is not an exact match, as there should be. Please provide a listing of the 219 cells and the number of months with observations during the 1854-1993 period.
9. The SI states that missing values of the temperature data set were interpolated. There are many gridcell series which are missing values either at the beginning or the end of the record. Please state the methodology for filling initial and closing values.
10. The original article in *Nature* says that a temperature data set up to 1995 was used and 1128 months of data. The temperature data set at the new SI only goes to 1993. Please reconcile.
11. I note that the temperature data set posted up in both the new SI and the old SI does not include the version of the CRU northern hemisphere temperature index from 1854-1901, which is hereby requested.
12. The new SI states that “A decreasing number of PCs of these sub-networks are retained increasingly further back in time, as dictated by application of objective selection criteria (consideration of results of both Preisendorffers Rule N and Scree test).” Please provide a statement of these “objective selection criteria”. We were unable to replicate the calculations which resulted in the same number of retained principal components in the NOAMER and SWM networks. Please provide the 95 percent values used for each of these two networks.
13. **Results of “Experiments”:** As noted in point in the cover letter, the requested output from MBH98 “experiments” was not provided, despite the CC request. For good order’s sake, I will re-state my request a little more precisely as follows.

For each of the 11 “experiments” mentioned in MBH98 and any emulations of MM03 discussed in the CC submission,

- (i) the reconstructed temperature principal components;
- (ii) the reconstructed northern hemisphere temperature index;
- (iii) residual series;
- (iv) Durbin-Watson statistics;
- (v) R^2 statistics;

As noted in the cover letter, disclosure of results of experiments is obviously mandatory.

14. Geographical Mislocations. In their submission, Mann et al. state the following:

The indicated locations of some of the proxy records in Figure 1 were misplaced from their true locations. Such issues have no bearing on the validity of the results provided by MBH98.

The geographical mis-locations are mostly in the precipitation proxies, where Toulouse and Paris precipitation series are used for North American gridcells. They do affect the validity of the model if MBH98 had deemed it important to include additional North American precipitation records rather than oversampling French locations. Although these errors are implicitly acknowledged in the above statement, the geographical locations are not corrected in the new SI. **Please provide the correct geographical locations for the precipitation series (and any other series with incorrect locations in the new SI.)**

15. Adequate Citation of Instrumental Series: In their submission, Mann et al. state:

There were some minor mistakes in the data listings and references listed in the supplementary information, available here:

The citations for the instrumental series to Jones and Bradley 1992, now stated to be incorrect, are replaced in the new SI only with the reference:

(individual series from NOAA Climate Data Center)

The NOAA Climate Date Center has thousands of series. Correct gridcell locations have not been provided, so this simply amounts to pointing to a haystack when asked for a needle. **Please provide an accurate data citation for these series.** I recommend using AGU data citation policies, if CC has no policies of its own.

16. In their submission, Mann et al. state:

Such issues [geographical mislocations, etc.] have no bearing on the validity of the results provided by MBH98.

Again, they do matter if they indicate that data MBH98 had intended to include was replaced with data they did not intend to include. And even if these errors in MBH98 do not affect the values of the early 15th century Northern Hemisphere temperature index, this does not mean that the errors do not affect other areas of MBH98 analysis. **Again, some evidence or backup for the validity of the above statement is needed.**

17. The new SI (PROXY/mbh98datasummaryadd-delete.txt) states:

These series met all the tests used for screening the ITRDB data (Mann et al., 2000), except one, namely, that in 1997 we were either not able to ascertain how these 34 series had been

standardized by the original contributors or knew they had been aggressively standardized, removing multidecadal to century-scale fluctuations.

Please explain the difference between the Schweingruber series included in the network and the Schweingruber series excluded from the network. Also, since Villalba series were excluded under this criterion from SOAMER network, please explain why temperature reconstructions by Villalba from the same series were used elsewhere.

18. The standardization practices of the Stahle series (as used in the SWM, OK and NOAMER networks) and in the SE USA precipitation reconstructions appear to have been aggressively standardized as they have virtually no remaining autocorrelation. Please justify their retention under the criterion stated in 15 above.

DETAILED COMMENTS EXCERPT FROM JANUARY 4 EMAIL

INTRODUCTION

Page 1, sentence 1. MM03 does not make the specific claim that “15th century temperatures were warmer than those of the late 20th century”. No such claim is made in MM03, and where some prominent commentators have interpreted our work in this way we have written to correct the record and ask them to revise their web sites. In the FAQ at <http://www.uoguelph.ca/~rmckitri/research/trc.html>, M&M state explicitly:

“Your graph seems to show that the 15th Century was warmer than today’s climate: is this what you’re claiming?”

No. We’re saying that Mann et al., based on their methodology and corrected data, cannot claim that the 20th century is warmer than the 15th century – the nuance is a little different. To make a positive claim that the 15th century was warmer than the late 20th century would require an endorsement of both the methodology and the common interpretation of the results which we are neither qualified nor inclined to offer.”

Page 1, first paragraph. Since the first sentence incorrectly characterizes the claim made by MM03, the following sentences are irrelevant.

Page 1, bottom paragraph, first sentence. The definition of an “audit” provided in sentence 1 is not correct. The objective of an audit is to verify the accuracy of a report, which may involve revision of data and procedures where necessary. Auditors often produce audited statements, which differ from unaudited statements. The claim that MM03 have used “neither the data nor the procedures of MBH98” is obviously overstated and only undermines the credibility of the authors since the paper itself establishes otherwise. At best they can say that MM03 used data and procedures that turn out to differ in subtle but important ways from MBH98.

Page 1, first paragraph, fourth sentence. Only a *statement* can *misrepresent* MBH98. An analysis may be incorrect, but is not a *misrepresentation*. No examples are provided in this paper of statements by MM03 which “seriously misrepresent” MBH98. MBH cite examples of methodological differences, which are not “*misrepresentations*”, much less “*serious misrepresentations*.”

Page 2, top sentence. MM03 does not claim that they “undertook exactly the same analysis as MBH98”; MM03 states: “We replicated the methodology of MBH98 as closely as we could using publicly available documentation and such private assistance as we were able to obtain.” What is incorrect about this statement?

Page 2, top paragraph, line 5. MBH refer, here and elsewhere, to “omission” of key early proxy indicators. This term is inaccurate. MM03 used the full proxy network after all sites were available. MBH have recently disclosed (but not in MBH98) that they varied their site rosters as a method of dealing with missing data in PC calculations. This is a difference in methodology that relates only to the handling of missing data in principal components, but it does not imply that MM03 opted for “omission” or “inconsistency”.

IMPACT

Page 2, middle paragraph, 3rd last line. MBH98 has 22 indicators for the 1400-1450 period. MM03 handles principal components differently than MBH98, affecting 2-3 out of 22 indicators. This is here described as “remov[ing] the majority of the underlying proxy data”. This sentence is obviously misleading.

Page 2, last paragraph, sentence 1. This is incorrect. MM03 provide FTP references showing indicators used. MM03 provide an FTP site showing the indicators in *.txt and *.xls files and the number of indicators for each year can be trivially calculated. MM03 also provide exact references to updated data versions used in their data – something that is not done in MBH98.

Page 2, last paragraph. No series are “omitted” or “eliminated”. Obsolete data used in MBH98 is replaced with updated versions. Principal component series are calculated for period in which all sites are available, as is clearly described in MM03. As above, this procedure differs from the “stepwise” procedure for PCs recently disclosed by MBH98, but is not an “omission”.

Page 3, top paragraph. This is the most salient issue in the article:

- MBH say that MM03 made a “complete misrepresentation” of the procedures used by MBH98. MM03 states the following: “Because standard PC algorithms fail in the presence of missing data, an important part of the methodology—namely how missing data were treated in the PC calculation—remains unexplained in MBH98.” Far from being a “misrepresentation” of MBH98, it is an entirely correct statement. MM03 explicitly state that they used a conventional PC algorithm and used PCs for the maximum available period – which is necessarily the period with no missing data. This method may not be the same as the “stepwise” method, but is a plausible interpretation of the available public record. If the public record is ambiguous, the responsibility for that rests with MBH.
- The last block of sentences is very misleading. It is perfectly obvious that MM03 obtained data for the NOAMER sites from NOAA Paleoclimatology since these FTP sites are listed in the MM03 Appendix. The data used by MBH98 is not located at NOAA Paleoclimatology as NOAA Paleoclimatology has different versions of many data series, a point stressed by MM03 in connection with other proxies, but this also applied to the

NOAMER sites. The data at ftp://holocene/evsc.virginia.edu/pub/MBH98/TREE/NOAMER was not available to MM03. The site was not identified to M&M, even in response to queries about data problems, and we had no knowledge of it until after MM03 came out. We were referred to the location ftp://holocene/evsc.virginia.edu/pub/sdr/pcproxy.txt and no indication was given that we should look for alternate versions of the same data, even in response to direct queries about the correctness of the data. Even if MBH had directed us the pub/MBH98 site, this section of the paragraph would still be irrelevant since the contents of the FTP site in question correspond closely to the data which we obtained directly from WDCP.

Page 3, item 1. MBH state that the original proxy data has been available “since May 2000”. The FTP site indicates availability since July 2002 only. In any case the site is not “available” if its location is kept secret, even in response to a request for it.

Page 3, item (2). The statement that MM03 used “a completely unrelated set of tree-ring records produced by Stahle and coworkers in earlier work” is false. MM03 used the 10 sites named in the MBH98 Supplementary Information and obtained WDCP versions of these. The versions differ from the versions used by MBH, but the onus is on MBH to establish the provenance of their data. In any event, the WDCP versions of the sites listed in the SI are hardly “generic tree-ring data”.

Page 3, item 2. MBH characterize the reference to Stahle et al as “outdated”; if Stahle et al. (1998) was actually used, then the MBH98 reference is more accurately characterized as “incorrect”. The reference to Stahle et al 1998 raises several questions: (a) Stahle et al. (1998) use only earlywood widths, while MBH98 use both earlywood and latewood widths. Use of the latter in MBH98 would therefore be incorrect. (b) Stahle et al (1998) go back only to 1706 in their calculations: what is the basis for MBH98 going earlier?; (c) the sites used in MBH98 according to the Supplementary Information differ from the sites in Table 1 of Stahle et al (1998); (d) TREE/STAHLE/SWM has 11 sites and fails to identify the sites, while MBH98/SI lists only 10 sites: what is the correspondence between the sites at TREE/STAHLE/SWM and MBH98/SI and what is the 11th site? (e) the data versions at TREE/STAHLE/SWM are not identical with the data versions at WDCP for the SWM sites listed at MBH98/SI: what is the exact provenance of the data versions at WDCP, including date received?

Page 4, item (3). MM03 used the version at WDCP for TTHH. The MBH98 version appears to be an obsolete edition and is not at WDCP. What is the exact provenance of the data version used by MBH98, including date? What is wrong with MM03 using the WDCP version?

Page 4, paragraph 2: this reconstruction does not indicate “warmth during the 15th century” but higher values of the MBH98 temperature index. MBH03 states: “this result should have been discarded”; MM03 explicitly state that the MBH98 dataset cannot yield reliable information and thus clearly do “discard” this result.

Page 4, paragraph, 10 sentences from bottom. “MM03 would have been forced to conclude that a skilful reconstruction is not possible prior to 1500.... It was completely incorrect for MM03 to conclude that their reconstruction provided statistically reliable evidence of anomalous warmth prior in the 15th century...against the instrumental record”. For reasons

already stated this misrepresents the MM03 interpretation of the results, and moreover MM03 concluded that MBH98 data could not be used for climate reconstruction – which is the same conclusion as MBH here say that MM03 should have reached, granting their premises.

Page 5, top paragraph, last sentence. “robust with respect to the precise instrumental calibration interval used”. The calculations shows only that the results are insensitive to choice between 1902-1971 and 1902-1980 calibration periods, but does not show robustness for all possible calibration intervals as asserted.

Part 3- IMPORTANT TECHNICAL ERRORS

Page 5, Item (a). This paragraph apparently asserts that the CRU data used for calibrating the proxy network was not the same as that used to graph the NH average up to 1998. Does this mean there is a splice in the MBH98 figure at 1993? Or is the entire instrumental series graphed therein the newer version ending in 1998, and thereby different from the data used to calibrate the proxies? Either way this needs to be clearly stated, considering the role that portion of the graph plays in their conclusions and the claim of this paragraph that editions of CRU data matter so acutely.

Page 5, Item (a) second paragraph. MBH98 did not state that gridpoint standard deviations were detrended. Is the effect material?

Page 5, Item (b): a nomenclature is needed to distinguish between the proxy series used only to make PC indicators and the 112-159 indicators used in the regression calculations. In MM03 we used “#” to denote one of the 112 proxy indicators. By mingling the two types of series MBH are make exaggerated claims about the differences in the 15th century data set, for instance by referring to 70 tree ring series rather than 2 PCs.

Page 6, item (c). Following MBH98, MM03 use 1 eigenvector in 1400-1450 and 2 eigenvectors in 1450-1600. Unless MBH have calculations to show otherwise, it is hard to see that this procedure has a material affect and therefore should not be included under “important” issues, as it is minor.

Page 6, Item (d). This scaling procedure is not described in MBH98. It adds a further linear transform to the reconstructed temperature series, but there is no explanation of why it is necessary or what influence it has on the final results. If it is a “minor” issue, it should not be listed in a section labelled “important” issues.

4. SPURIOUS CRITICISMS

Page 6, item (b). MBH03 identify a difference in procedure, but the procedure followed in MM03 is not precluded by disclosure in MBH98, and thus is not a “mistake”. Point (3.b) resolves part of the MM03 concern, but creates another. The calculations yielding series described as “PC”s” in ftp/MBH98 are not “conventional” principal component calculations. MBH98 uses a Fortran program in which SVD is applied to an off-center dataset, instead of to a covariance or correlation matrix as required by the optimization problem from which PC algorithms are derived. Hence the procedure applied by MBH98 is not “optimized with respect to the underlying data.”

Page 6, Item (b). The assertion in MM03 is not incorrect. MBH98 did in fact calculate PCS from data sets with missing data by the now disclosed expedient of stepwise calculations. This may or may not be a valid expedient, but the data sets do have missing data.

Page 6, Item (c). The authors make a claim in strong terms that SWM data exhibit a unique sensitivity to El Nino and hence must be grouped separately. This claim is contradicted by the fact that the NOAMER compilation includes so many SWM sites. Many, if not most, of the actual Stahle/SWM locations are geographically interwoven with ITRDB/NOAMER sites: look at the NM, AZ and CO sites in Stahle/SWM as compared to NM, AZ and CO sites in ITRDB/NOAMER and the mexi001 series used as a separate proxy. What is the difference between the Stahle et al 1998 site of Eagle Creek AZ (az518), a PIED site, and the following NOAMER PIED sites in Arizona: AZ081, AZ084, AZ086, AZ102, AZ104, AZ106, AZ129, AZ144? Why is the Stahle et al 1998 Table 1 PIED site of Navajo Mountain UT (UT022) included in NOAMER? What is difference between UT022 and the following NOAMER PIED sites in Utah: UT018, UT020, UT022? Why are Stahle et al 1998 PIPO sites of Abousselman Springs (nm555) and Elephant Rock (nm559) included in NOAMER? What is difference between these sites and NOAMER PIPO NM sites: NM021, NM029, NM550, NM556, NM557, NM566, some of which are in very close proximity. What is difference between Table 1 PSME sites in NM of Ditch Canyon (nm503) and Pueblita Canyon (nm537) and NOAMER NM PSME sites of: NM025, NM026, NM565, NM570, NM570X, NM572? What is difference between Table 1 NM PIED site of Paliza Canyon NM (nm043) and NM PIED sites in NOAMER of NM030, NM031, NM035, NM038, NM055, NM118, NM564? As noted above, the MBH98 implementation of Stahle et al (1998) is not consistent.

Page 6, Item (c). What are the similar arguments for other proxy sub-networks?

Page 7, Item (d). MBH say that the deletion of C Eng values prior to 1730 is “in keeping with previous studies (Bradley and Jones 1993)”. However those papers used the temperature series *as temperatures* whereas here they are used as *proxies* subject to regression-mining for a temperature signal. As proxies the early portions of these series are likely more usable than many proxy segments retained elsewhere in the data set. Moreover, inspection of Figure 2 in Bradley and Jones 1993 shows that while these values are indeed deleted in this study, no justification is provided for the truncation. Is there a quality control principal being applied consistently here? There is a material gap in the Urvina Bay coral record. What is the basis for keeping this record, but truncating the C Eng record? MBH argue that the CET truncation can be justified based on “inhomogeneities that exist in the record prior to the 1730s”, citing Manley 1974 as describing a gap from 1707 to 1722 in daily data that he was seeking to cover and “used an adjusted version of data from the Netherlands to fill this gap”. This does not apply to the Central European Series though. Moreover it is an incorrect characterization of Manley 1974. Manley (1959) relied on Utrecht data. Manley (1974) says that one of its purposes was “to incorporate in the light of more recent discoveries in MS, hitherto unrecorded, some improvement on the estimates that were previously made to cover the months from 1707 to 1722” (392). He refers to the following for 1707-22: “an additional North of England non-instrumental diary of Wiilliam Elmsall; also Christopher Rawlinson MS for 1699-1717; Smith at Richmond; instrumental readings for 1716-22. “wind and weather” in

or near London are continuous since 1669. Even accepting the deletion of the early years, no explanation is given for switching to summertime averages. To justify it by saying it makes no difference only raises the question, Then why do it? Additionally, no justification is given for Central Europe truncation or chin04 truncation. And the last sentence is misleading as they do not affect the early periods in which data was truncated.

Page 7, Item (e). This repeats the criticism of page 4, bottom paragraph. The comments of page 4, bottom paragraph properly belong here, since the issue of the effect of infilling is not an “Important Error” in MM03, although it may be a Spurious Criticism.

Page 7, Item (g). This is false. M&M did not make a “special request” for a spreadsheet version, and at no time was a spreadsheet version prepared for MM (at least none was supplied). MM requested an FTP location of series in same form as available for MBH99. Instead Mann’s associate provided a FTP location of a txt and a mat file containing a data collation, but the file itself was obviously not specially prepared as it was created long before the request by M&M. In the ensuing months the pub/MBH98 FTP site was never divulged.

Page 7, 2nd last paragraph. Granted that the space in Nature for description of methodology is insufficient, but there is no obstacle to a more copious and systematic description of methodology in Supplementary Information, and there is certainly no page limit on Internet sites. Zorita et al (2003) cannot be cited in defence of the adequacy of disclosure. Zorita et al does not use MBH98 proxy data and do not “independently reproduce” the results of MBH98. They use GCM data and did not have to use “stepwise” methods to deal with missing data. This claim is accordingly incorrect.

Page 8, paragraph 1. A date of July 2000 is given here for FTP availability for proxy indicators. Elsewhere date of May 2000 is given. The actual site does not appear to have been started until July 30, **2002**.

Page 8, paragraphs 2-3. The claim implicit in this section is that all the data have been made available. This is very misleading. The FTP site for MBH98 includes many series that were not used in MBH98 and it is impossible to distinguish one from the other in the present state of data description. Nor is it possible to identify the sections of the PC files that have to be spliced together in the ‘stepwise’ procedure. The new FTP site referred to is not currently up, but presumably is in process. And in any case, if the existing disclosure is so comprehensive, why is a new FTP site needed?

CONCLUSIONS

Page 8, paragraph 4. “the correct interpretation of MM03 would have been that a skilful reconstruction was not possible..” This was the conclusion of MM03. The next few sentences are irrelevant. We do not make a claim about the 15th century climate.

APPENDIX B **LISTING OF PC SERIES IN DATALISTS – 58 series**

OK/BACKTO_1750/pc01.out

OK/BACKTO_1750/pc02.out
OK/BACKTO_1750/pc03.out
SWM/BACKTO_1400/pc01.out
SWM/BACKTO_1450/pc01.out
SWM/BACKTO_1500/pc01.out
SWM/BACKTO_1600/pc01.out
SWM/BACKTO_1700/pc01.out
SWM/BACKTO_1500/pc02.out
SWM/BACKTO_1600/pc03.out
SWM/BACKTO_1700/pc03.out
SWM/BACKTO_1600/pc04.out
SWM/BACKTO_1700/pc04.out
SWM/BACKTO_1700/pc05.out
SWM/BACKTO_1700/pc06.out
SWM/BACKTO_1700/pc07.out
SWM/BACKTO_1700/pc08.out
SWM/BACKTO_1700/pc09.out
VAG/BACKTO_1450/pc01.out
VAG/BACKTO_1600/pc01.out
VAG/BACKTO_1750/pc01.out
VAG/BACKTO_1600/pc02.out
VAG/BACKTO_1750/pc02.out
VAG/BACKTO_1750/pc03.out
NOAMER/BACKTO_1400/pc01.out
NOAMER/BACKTO_1450/pc01.out
NOAMER/BACKTO_1600/pc01.out
NOAMER/BACKTO_1750/pc01.out
NOAMER/BACKTO_1400/pc02.out
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NOAMER/BACKTO_1750/pc06.out
NOAMER/BACKTO_1600/pc07.out
NOAMER/BACKTO_1750/pc07.out
NOAMER/BACKTO_1750/pc08.out
NOAMER/BACKTO_1750/pc09.out
SOAMER/BACKTO_1600/pc01.out
SOAMER/BACKTO_1600/pc02.out
SOAMER/BACKTO_1600/pc03.out
AUS/BACKTO_1600/pc01.out
AUS/BACKTO_1750/pc01.out
AUS/BACKTO_1600/pc02.out
AUS/BACKTO_1750/pc02.out
AUS/BACKTO_1600/pc03.out
AUS/BACKTO_1750/pc03.out
AUS/BACKTO_1750/pc04.out