Copies of communications relating to Keith Briffa's editorial treatment of a submitted manuscript

The following is a series of communications between Keith Briffa, in his role as an Associate Editor of a journal, two referees and one of the authors of a submitted manuscript being considered for publication. These emails are provided here in support of the responses made to the Muir-Russell Review Team question regarding the appropriateness of Briffa's handling of this submitted paper. It had been alleged that Briffa sought to have this paper rejected other than on acceptable scientific grounds. This allegation arises from the misinterpretation of one of the stolen CRU emails. For example, McIntyre includes the following extract from the email in his submission to this Review

"*I am really sorry but I have to nag about that review – Confidentially I now need a hard and if required extensive case for rejecting*" (see paragraph 27 of McIntyre's submission to the UK Parliamentary Review, appended in his submission to The Muir-Russell Review).

McIntyre includes this under the category of examples of " *Use of the peer review process to suppress or delay adverse publications*", implying that Briffa actively sought to have the paper rejected because its content was in some way "adverse". The accusation was also made in an article published in the *Guardian* (3rd February 2010) where it was also suggested that the paper in question was connected with Briffa's own research. The copied correspondence should be read in conjunction with CRU's response to Issue 5.1b on page 60 of the document "*Submission by the Climatic Research Unit to the Independent Climate Change e-mail Review*" (dated 1st March 2010) and Section 12, page 8, of the CRU document "*Response to Salient Points..and additional requested information*" (23rd June 2010).

Several conclusions can be drawn from reading this correspondence. Briffa had no personal interest in the general subject area (i.e. links between western US drought and the Pacific Decadal Oscillation) or in the specific work of the authors. At the early review stage Briffa neither considered the paper in detail nor sought to get the reviewers to reject it. The email that is the focus of the allegations was concerned only to ensure that if the second reviewer were to come out with a negative opinion they should be sure to provide a detailed case explaining why – rather than just cursorily recommending rejection (as some reviewers do). Briffa was especially concerned that the reviewer might return a brief response because of the pressure being applied to get him to respond quickly, following the delay that had already occurred in arriving at a decision regarding this submission. Briffa had no personal opinion on the subject matter.

As it turns out, while searching through emails to gather these communications one was discovered that sheds light on the issue of why the second reviewer was aware of the identity and general opinion of the other reviewer. Though this email has not previously been released, it was thought important to include it here as Briffa has previously speculated about this issue but did not recall the details from some 6 or 7 years ago (see discussion in the 1st March CRU document cited above). It is clear from the message sent by Briffa on 16th April

2003, that he did give this information to the second reviewer. This is certainly not his usual practice and he was frankly surprised to discover that he had done so. However there would have been no doubt about the independence and frankness of this second reviewer – and this opinion is borne out by noting that this reviewer did not recommend that the paper be rejected, but made several considered remarks and left it to the Editor (Briffa) to judge how to proceed.

The correspondence shows that Briffa did not seek to reject the paper and having read the reviews attempted to be constructive, recommending that the authors consider resubmission of an amended version. Had he been seeking to have the paper rejected as accused he could have done so at this point. The authors' response shows that they considered the reviews "were helpful". To Briffa's knowledge, the authors did not submit a revised manuscript, though their intention to do so at the time was clear.

In summary the copies of the correspondence provided here prove that Briffa did not seek to have this paper rejected. It was not directly relevant to his own work or to the work he undertook on behalf of the IPCC. McIntyre and the Guardian were wrong to imply otherwise. The allegation was based on a selective, misrepresentation of part of an email taken out of context.

Note regarding dates shown on the following communications:

The first two messages are scans of the paper copies. The rest (numbers 3 to 14) are copies of emails. While saving the emails for presentation here the dates of some have not been preserved (nos. 4, 6, 8, 9 and 12). This information has not been lost, however, and the relevant dates are: No.4, 9th August 2002; 6, 14th April 2003; 8, 16th April 2003; 9, 4th June 2003; and 12, 24th July 2003.

[redacted]

17 June 2002

Dear [redacted]

I would be very grateful for your review of the enclosed manuscript

'Using a new 672-year tree-ring drought reconstruction from west-central Montana to evaluate severe drought teleconnections in the western U.S. and possible climatic forcing by the Pacific Decadal Oscillation.' by [redacted]

recently submitted to [redacted]

I would be happy to receive an email opinion, if this is more convenient.

Yours sincerely,

Keith Briffa
Associate Editor
[redacted]

Enclosures

[redacted]

17 June 2002

Dear [redacted]

I would be very grateful for your review of the enclosed manuscript

'Using a new 672-year tree-ring drought reconstruction from west-central Montana to evaluate severe drought teleconnections in the western U.S. and possible climatic forcing by the Pacific Decadal Oscillation.' by [redacted]

recently submitted to [redacted]

I would be happy to receive an email opinion, if this is more convenient.

Yours sincerely,

Keith Briffa
Associate Editor
[redacted]

Enclosures

Briffa Keith Prof (ENV)

From: [redacted]
Sent: 07 August 2002 15:25
To: k.briffa@uea.ac.uk
Subject: [redacted]

<x-flowed>
Dear Keith:

Hope you are well and that I get to see you again soon.
Sorry for the delay on these reviews, but finally I have an opinion.
The [redacted] paper is weak and unsuited for publication. The [redacted] paper is more problematic. Pretty strong analyses, but the calibration and verification are awful, and I'm afraid that can probably be traced back to the half ass chronology development procedures. I wouldn't publish that one either, unless they go all the way back and fix the chronology development, and then substantially improve the calibration and verification. The reviews are attached and reproduced below.

Sincerely, [redacted]

[redacted]

Review of [redacted] "Using a new 672-year tree-ring drought reconstruction from westcentral MontanaŠ" submitted to [redacted]

This is a well written, well executed paper that I would unfortunately not recommend for publication in [redacted]. It's a shame to read a paper like this. It is very well informed, well referenced, places the work in a good scientific context, and includes strong statistical analyses. However, the attention paid to the analyses and interpretation of the reconstruction was evidently not paid so carefully to the fundamental tree-ring chronology development. They call it "crossdating," but the best I can tell from the limited discussion it was simply computerized correlation matching of measured time series, with a massive culling of the data to pare down to those time series

that produced straightforward correlations in a COFECHA analysis. I was astounded to read that their final chronology used only 61 out of the 152 trees sampled for the study. The 60% of the trees not included apparently suffered from "complacency, unresolvable sections of missing rings, or low interseries correlation values." This appears to be the first penalty for not applying rigorous dendrochronological methods to the chronology development. I find it incredible that over half of the Ponderosa pine samples would not be useful. I can't help but suspect that by relying on COFECHA output, without any hard-nosed microscope work and rigorous crossdating with the wood samples themselves, you at best default to the simple, straightforward trees without missing rings. That is, you default to a less climatically sensitive subset of trees. This appears to be the second penalty for the seemingly inexpert, quick and dirty chronology development.

These authors have obviously worked hard on this study and bring excellent analytical skills and knowledge of the literature.

The paper itself is exceptionally well written (with a minor complaint concerning the over use, and at times incorrect use of the term "teleconnection"). But the calibration and validation reported in the paper are clearly awful, and that surely ought not be the case for Ponderosa pine on moisture-stressed sites in Montana. One hates to be non-supportive of their work, so much of which is high quality, but it seems to come down to fundamentals, and here the fundamental dendrochronology and chronology development are in question. And I also do not think it advisable to publish a reconstruction that explains maybe 21% of the variance in the instrumental climate data, when using an arid site conifer as the predictor (the persistence in the standard chronology may be inflating even that figure). I just can't believe the calibration could be so weak. It seems they need to revisit their chronology development work, and dig deeper into the climate response of their chronology. Then look very carefully at climate data itself. These climate data are not guaranteed to be homogeneous, especially in the mountain West during the early 20th century. If all this could be done, and if the variance explained in both the calibration and verification periods could be improved, then publication in [redacted] would be well justified.

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Attachment Converted: [redacted]

Attachment Converted: [redacted]
<x-flowed>
--
[redacted]

</x-flowed>

Briffa Keith Prof (ENV)

From: Keith Briffa [k.briffa@uea.ac.uk]
To: [redacted]
Subject: Re: [redacted]

<html>
[redacted]thanks for these - there may some dispute (difference of opinion with other referee) so I will have to read the paper(s) in detail and I will let you know for interest which way we go. I would love to have one of your classic papers in [redacted](by which I mean any one). How about that? I genuinely hope you are thriving - I am pretty fed up I have to admit. Seem to have got my priorities messed up somewhere. Growing fat and old before my time. Don't you . Best wishes
 Keith

 <x-sigsep><p></x-sigsep> --
Professor Keith Briffa,
 Climatic Research Unit
 University of East Anglia
Norwich, NR4 7TJ, U.K.

Phone: +44-1603-593909
Fax: +44-1603-507784

<a href="http://www.cru.uea.ac.uk/cru/people/briffa/"
eudora="autourl">http://www.cru.uea.ac.uk/cru/people/briffa<a
href="http://www.cru.uea.ac.uk/cru/people/briffa/" eudora="autourl">/</html>

Briffa Keith Prof (ENV)

From: [redacted]
Sent: 18 September 2002 17:38
To: K.Briffa@uea.ac.uk
Cc: [redacted]
Subject: [redacted] manuscript status

Dear Dr. Briffa,

I would like to inquire about the review status of the following manuscript my colleague, [redacted], and I submitted in early June to [redacted]:

"Using a new 672-year tree-ring drought reconstruction from west-central Montana to evaluate severe drought teleconnections in the western US and possible climatic forcing by the Pacific Decadal Oscillation"

[redacted]and I very much appreciate the time invested by you and the referees.

Sincerely,
[redacted]

[redacted]

Briffa Keith Prof (ENV)

From: Keith Briffa [k.briffa@uea.ac.uk]
To: [redacted]
Subject: review?

<html>
[redacted]can you check whether you returned a review I requested of a paper by [redacted]
 Keith
 <x-sigsep><p></x-sigsep> --
 Professor Keith Briffa,
 Climatic Research Unit
 University of East Anglia
 Norwich, NR4 7TJ, U.K.

Phone: +44-1603-593909
Fax: +44-1603-507784

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href="http://www.cru.uea.ac.uk/cru/people/briffa/" eudora="autourl">/</html>

Briffa Keith Prof (ENV)

From: [redacted]
Sent: 16 April 2003 13:06
To: Keith Briffa
Subject: Re: review?

<x-flowed>
Hi Keith,

Send me the title. I can't remember anything about this paper.

[redacted]can you check whether you returned a review I requested of a paper
>by [redacted]
>
>--
>Professor Keith Briffa,
>Climatic Research Unit
>University of East Anglia
>Norwich, NR4 7TJ, U.K.
>
>Phone: +44-1603-593909
>Fax: +44-1603-507784
>
>http://www.cru.uea.ac.uk/cru/people/briffa/

[redacted]

Briffa Keith Prof (ENV)

From: Keith Briffa [k.briffa@uea.ac.uk]
To: [redacted]
Subject: Re: review?

<html>
It is " using a new 672 year tree ring drought reconstruction from west central Montana etc.. . Confidentially, [redacted] also reviewed and said very well written BUT NO GO. It was sent to you on 17 June 2002!!
 Thanks
 AND WHY HAVE YOU NOT F>>>G RANG ME ? Am off home now. love
 Keith

 At 08:06 AM 4/16/03 -0400, you wrote:
 <blockquote type=cite class=cite cite>Hi Keith,

 Send me the title. I can't remember anything about this paper.
 [redacted]
 <blockquote type=cite class=cite [redacted] can you check whether you returned a review I requested of a paper by [redacted] Keith

 --
 Professor Keith Briffa,
 Climatic Research Unit
 University of East Anglia
 Norwich, NR4 7TJ, U.K.

 Phone: +44-1603-593909
Fax: +44-1603-507784

<a href="http://www.cru.uea.ac.uk/cru/people/briffa/"
eudora="autourl">http://www.cru.uea.ac.uk/cru/people/briffa/</blockquote>

[redacted]

<x-sigsep><p></x-sigsep>
--
Professor Keith Briffa,
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Briffa Keith Prof (ENV)

From: Keith Briffa [k.briffa@uea.ac.uk]
To: [redacted]
Subject: Re: Review- confidential REALLY URGENT

<html>

I am really sorry but I have to nag about that review - Confidentially I now need a hard and if required extensive case for rejecting - to support [redacted] and really as soon as you can. Please
 Keith
 [redacted]

--
Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.

Phone: +44-1603-593909
Fax: +44-1603-507784

Briffa Keith Prof (ENV)

From: [redacted]
Sent: 04 June 2003 16:16
To: Keith Briffa
Subject: Re: Review- confidential REALLY URGENT

<x-flowed>
Hi Keith,

Here is my review. I must admit to not being quite as negative about it as [redacted] but I do feel that it is marginal at best and could be justifiably rejected. Read my review. Of course, you will want to cut out the review and send it to the authors as a separate document.

Review of "Using a New 672-Year Tree-Ring Drought Reconstruction from West-Central Montana to Evaluate Severe Drought Teleconnections in the Western U.S. and Possible Climatic Forcing by the Pacific Decadal Oscillation" by [redacted]

This paper is reasonably well written, but has some problems in it that bother me. The first issue relates to the tree-ring chronology that was developed at Lindberg Lake. Anytime less than half of the core samples (61 or 152) are used in developing a chronology, this is cause for concern. The fact that there are "unresolvable sections of missing rings" (p. 10) can mean a lot of things. However, ponderosa pine is known to cross-date well, which includes "locating"
locally-absent rings during the cross-dating phase, so it is surprising that the authors have chosen not to work through these problems. Presumably, the trees with missing rings are also those most sensitive to drought, so isn't there a chance that the chronology being analyzed in this paper is less sensitive to drought than it ought to be? I also wonder how much their chronology is truly contributing to the overall stated goal of this paper, i.e.
evaluating "Severe Drought Teleconnections in the Western U.S. and Possible Climatic Forcing by the Pacific Decadal Oscillation". The authors extensively use the PDSI reconstructions of Cook et al.
(1999) in their analyses. Aside from the increased length of their new tree-ring chronology, what does it contribute that was not possible simply by using the Cook et al. reconstructions to test for teleconnections and forcing. None of the indices of forcing (ENSO, PDO, sunspots) extend back before the beginning of the Cook et al. reconstructions, so there is little to be gained in using one longer series from west-central Montana in this analysis. One could point to Fig. 3, which compares the MT reconstruction vs the SWDI series. But even this comparison is limited in its overall contribution to the paper. I also don't like the use of the FFT for estimating power spectra, even if the confidence limits are determined by bootstrapping. The power spectra calculated by the FFT are still inconsistent estimates. A more contemporary and consistent method of spectral estimation, like the Multi-Taper Method, should be used.

For the reasons stated above, I do not consider this paper to be ready for publication as is. I will leave it to the Editor to decide how to proceed with it past this point.

--
[redacted]

Briffa Keith Prof (ENV)

From: [redacted]
Sent: 24 July 2003 04:33
To: K.Briffa@uea.ac.uk
Cc: [redacted]
Subject: [redacted] manuscript

Dear Keith,

I have not yet received an editorial response or reviews for the manuscript entitied "Using a new 672-year tree-ring drought reconstruction from west-central Montana to evaluate severe drought teleconnections in the western US and possible climatic forcing by the Pacific Decadal Oscillation"
by [redacted]. This manuscript has been in review for 14 months.

Can you indicate when I can expect these materials?

Many thanks,
[redacted]

[redacted]

Briffa Keith Prof (ENV)

From: Keith Briffa [k.briffa@uea.ac.uk]
To: [redacted]
Subject: Re: Holocene manuscript- sorry

<html>

[redacted]
I am really sorry for the delay , that was a result of initially tardy reviewers, my subsequent illness, and then a delay while [redacted]is away while I wished to consult with him . The reason for the last wish will be clear when you read the reviews below.
 The referees are not enthusiastic and in their private comments to me one is strongly negative and the other ambivalent. The pressure on space means that this would normally be a rejection (and we have, since your submission ,developed new , stricter rules regarding possible re-submission.)
 However, in the circumstances (the delay that is down to me) , I am overruling these and (despite not discussing it with [redacted]) asking you to read these reviews and come back with a frank opinion of whether you consider them fair and the paper publishable with some work . I am doing this because I believe it is. If you can answer these remarks and feel you can submit a valid manuscript that accounts for them - I will review your argument (without recourse to the reviewers) and if I agree , I guarantee speedy process through the last publication phase.
 Again , you and I are well aware that this manuscript could have been dealt with much better and I am really sorry for it.
 Keith

 REVIEWERS REMARKS FOLLOW

 Referee 1

 Review of [redacted]: "Using a new 672-year tree-ring drought reconstruction from westcentral MontanaŠ" submitted to [redacted]
 This is a well written, well executed paper that I would unfortunately not recommend for publication in [redacted]. It's a shame to read a paper like this. It is very well informed, well referenced, places the work in a good scientific context, and includes strong statistical analyses. However, the attention paid to the analyses and interpretation of the reconstruction was evidently not paid so carefully to the fundamental tree-ring chronology development. They call it "crossdating," but the best I can tell from the limited discussion it was simply computerized correlation matching of measured time series, with a massive culling of the data to pare down to those time series that produced straightforward correlations in a COFECHA analysis. I was astounded to read that their final chronology used only 61 out of the 152 trees sampled for the study. The 60% of the trees not included apparently suffered from "complacency, unresolvable sections of missing rings, or low interseries correlation values." This appears to be the first penalty for not applying rigorous dendrochronological methods to the chronology development. I find it incredible that over half of the Ponderosa pine samples would not be useful. I can't help but suspect that by relying on COFECHA output, without any hard-nosed microscope work and rigorous crossdating with the wood samples themselves, you at best default to the simple, straightforward trees without missing rings. That is, you default to a less climatically sensitive subset of trees. This appears to be the second penalty for the seemingly inexpert, quick and dirty chronology development.
 These authors have obviously worked hard on this study and bring excellent analytical skills and knowledge of the literature. The paper itself is exceptionally well written (with a minor complaint concerning the over use, and at times incorrect use of the term "teleconnection"). But the calibration and validation reported in the paper are clearly awful, and that surely ought not be the case for Ponderosa pine on moisture-stressed sites in Montana. One hates to be non-supportive of their work, so much of which is high quality, but it seems to come down to fundamentals, and here the fundamental dendrochronology and chronology development are in question. And I also do not think it advisable to publish a reconstruction that explains maybe 21% of the variance in the instrumental climate data, when using an arid site conifer as the predictor (the persistence in the standard chronology may be inflating even that figure). I just can't believe the calibration could be so weak. It seems they need to revisit their chronology development work, and dig deeper into the climate response of their chronology. Then look very carefully at climate data itself. These climate data are not guaranteed to be homogeneous, especially in the mountain West during the early 20th century. If all this

could be done, and if the variance explained in both the calibration and verification periods could be improved, then publication in [redacted] would be well justified.

 Referee 2

 Review of "Using a New 672-Year Tree-Ring Drought Reconstruction from West-Central Montana to Evaluate Severe Drought Teleconnections in the Western U.S. and Possible Climatic Forcing by the Pacific Decadal Oscillation" by [redacted] This paper is reasonably well written, but has some problems in it that bother me. The first issue relates to the tree-ring chronology that was developed at Lindberg Lake. Anytime less than half of the core samples (61 or 152) are used in developing a chronology, this is cause for concern. The fact that there are "unresolvable sections of missing rings" (p. 10) can mean a lot of things. However, ponderosa pine is known to cross-date well, which includes "locating" locally-absent rings during the cross-dating phase, so it is surprising that the authors have chosen not to work through these problems. Presumably, the trees with missing rings are also those most sensitive to drought, so isn't there a chance that the chronology being analyzed in this paper is less sensitive to drought than it ought to be? I also wonder how much their chronology is truly contributing to the overall stated goal of this paper, i.e. evaluating "Severe Drought Teleconnections in the Western U.S. and Possible Climatic Forcing by the Pacific Decadal Oscillation". The authors extensively use the PDSI reconstructions of Cook et al. (1999) in their analyses. Aside from the increased length of their new tree-ring chronology, what does it contribute that was not possible simply by using the Cook et al. reconstructions to test for teleconnections and forcing. None of the indices of forcing (ENSO, PDO, sunspots) extend back before the beginning of the Cook et al. reconstructions, so there is little to be gained in using one longer series from west-central Montana in this analysis. One could point to Fig. 3, which compares the MT reconstruction vs the SWDI series. But even this comparison is limited in its overall contribution to the paper. I also don't like the use of the FFT for estimating power spectra, even if the confidence limits are determined by bootstrapping. The power spectra calculated by the FFT are still inconsistent estimates. A more contemporary and consistent method of spectral estimation, like the Multi-Taper Method, should be used.
 For the reasons stated above, I do not consider this paper to be ready for publication as is. I will leave it to the Editor to decide how to proceed with it past this point.

 At 10:33 PM 7/23/03 -0500, you wrote:

 <blockquote type=cite class=cite cite>Dear Keith,

 I have not yet received an editorial response or reviews for the manuscript
 entitled "Using a new 672-year tree-ring drought reconstruction from
 west-central Montana to evaluate severe drought teleconnections in the
 western US and possible climatic forcing by the Pacific Decadal Oscillation"
 by [redacted] This manuscript has been in review for 14 months.

 Can you indicate when I can expect these materials?

 Many thanks,
 [redacted]

[redacted]

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--
Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.

Phone: +44-1603-593909
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href="http://www.cru.uea.ac.uk/cru/people/briffa/" eudora="autourl">/</html>

Briffa Keith Prof (ENV)

From: [redacted]
Sent: 24 July 2003 19:50
To: Keith Briffa
Cc: [redacted]
Subject: Re: [redacted] manuscript- sorry

Thanks, Keith. I will contact the author, [redacted] and see how he wants to proceed. The reviews below were helpful, and we would like to revise the manuscript to improve its quality. A first place to start will be to simplify the approach, focusing mainly on the new chronology and its comparison with Cook et al's data set (i.e., reviewer 2's suggestion to omit the fft of sunspots and ENSO, which are well reviewed in the literature).

My first read of the reviews were mixed. Reviewer 1's comments that we only included trees with no missing rings is false. Our chronology includes several cores with missing rings but where identifying and incorporating missing rings was fairly straightforward. And there were plenty of hard hours at the scope looking for and incorporating them. Perhaps we should back off our statement about omitting cores with "complacency, unresolvable sections of missing rings, or low interseries correlation values" which was obviously misleading. Nonetheless, there will always be cores that are simply too difficult to verify with COFECHA, and I was surprised to see his/her reaction that 61 cores couldn't establish an adequate chronology. I doubt there there is literature suggesting a minimum fraction of cores that should be incorporated into a chronology.

We will also re-analyze the response function analysis and climate data, although our approach in examining both raw and detrended data were pretty exhaustive. It may turn out that reviewer 1 is correct that climate data "are not guaranteed to be homogeneous, especially in the mountain West during the early 20th century." Given that we analyzed both pre-whitened and standardized data sets, do you or the reviewer have additional suggestions on how to proceed?

If we were to undertake substantial revision in an effort to resubmit, is there an appropriate timetable that would work for you, say our getting a manuscript to you sometime in the next 3-5 months?

Thanks again, Keith.
All the best,
[redacted]

--On Thursday, July 24, 2003, 1:57 PM +0100 "Keith Briffa" <k.briffa@uea.ac.uk> wrote:

>
[redacted]
> I am really sorry for the delay , that was a result of initially tardy
> reviewers, my subsequent illness, and then a delay while [redacted] is away
while
> I wished to consult with him . The reason for the last wish will be
> clear when you read the reviews below.
> The referees are not enthusiastic and in their private comments to me
> one is strongly negative and the other ambivalent. The pressure on
> space means

> that this would normally be a rejection (and we have, since your

submission
> ,developed new , stricter rules regarding possible re-submission.)
> However, in the circumstances (the delay that is down to me) , I am
> overruling these and (despite not discussing it with [redacted]) asking you
> to read these reviews and come back with a frank opinion of whether
> you consider them fair and the paper publishable with some work . I am
> doing this because I believe it is. If you can answer these remarks
> and feel you

> can submit a valid manuscript that accounts for them - I will review
> your argument (without recourse to the reviewers) and if I agree , I
> guarantee speedy process through the last publication phase.
> Again , you and I are well aware that this manuscript could have been
dealt
> with much better and I am really sorry for it.
> Keith
>
> REVIEWERS REMARKS FOLLOW
>
> Referee 1
>
> Review of [redacted]: "Using a new 672-year
> tree-ring

> drought reconstruction from westcentral MontanaŠ" submitted to [redacted]
[redacted]
> This is a well written, well executed paper that I would unfortunately
> not

> recommend for publication in the Holocene. It's a shame to read a
> paper like this. It is very well informed, well referenced, places the
> work in a

> good scientific context, and includes strong statistical analyses.
However,
> the attention paid to the analyses and interpretation of the
reconstruction
> was evidently not paid so carefully to the fundamental tree-ring
chronology
> development. They call it "crossdating," but the best I can tell from
> the limited discussion it was simply computerized correlation matching
> of measured time series, with a massive culling of the data to pare
> down to those time series that produced straightforward correlations
> in a COFECHA analysis. I was astounded to read that their final
> chronology used only 61

> out of the 152 trees sampled for the study. The 60% of the trees not
> included apparently suffered from "complacency, unresolvable sections
> of missing rings, or low interseries correlation values." This appears
> to be the first penalty for not applying rigorous dendrochronological
> methods to

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> Ponderosa pine samples would not be useful. I can't help but suspect
> that by relying on COFECHA output, without any hard-nosed microscope
> work and rigorous crossdating with the wood samples themselves, you at
> best default

> to the simple, straightforward trees without missing rings. That is,
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> appears to be the second penalty for the seemingly inexpert, quick and

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> These authors have obviously worked hard on this study and bring
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> analytical skills and knowledge of the literature. The paper itself is
> exceptionally well written (with a minor complaint concerning the over
use,
> and at times incorrect use of the term "teleconnection"). But the
> calibration and validation reported in the paper are clearly awful,
> and that surely ought not be the case for Ponderosa pine on
> moisture-stressed sites in Montana. One hates to be non-supportive of
> their work, so much of

> which is high quality, but it seems to come down to fundamentals, and
> here

> the fundamental dendrochronology and chronology development are in
> question. And I also do not think it advisable to publish a
> reconstruction

> that explains maybe 21% of the variance in the instrumental climate
> data, when using an arid site conifer as the predictor (the
> persistence in the standard chronology may be inflating even that
> figure). I just can't believe the calibration could be so weak. It
> seems they need to revisit their chronology development work, and dig
> deeper into the climate
response
> of their chronology. Then look very carefully at climate data itself.
These
> climate data are not guaranteed to be homogeneous, especially in the
> mountain West during the early 20th century. If all this could be
> done,
and
> if the variance explained in both the calibration and verification
> periods

> could be improved, then publication in [redacted] would be well
justified.
>
> Referee 2
>
>
> Review of "Using a New 672-Year Tree-Ring Drought Reconstruction from
> West-Central Montana to Evaluate Severe Drought Teleconnections in the
> Western U.S. and Possible Climatic Forcing by the Pacific Decadal
> Oscillation" by [redacted]
>
> This paper is reasonably well written, but has some problems in it
> that bother me. The first issue relates to the tree-ring chronology
> that was developed at Lindberg Lake. Anytime less than half of the
> core samples (61

> or 152) are used in developing a chronology, this is cause for concern.
The
> fact that there are "unresolvable sections of missing rings" (p. 10)
> can mean a lot of things. However, ponderosa pine is known to
> cross-date well,

> which includes "locating" locally-absent rings during the cross-dating
> phase, so it is surprising that the authors have chosen not to work
through

> these problems. Presumably, the trees with missing rings are also
> those most sensitive to drought, so isn't there a chance that the
> chronology being analyzed in this paper is less sensitive to drought
> than it ought to

> be? I also wonder how much their chronology is truly contributing to
> the overall stated goal of this paper, i.e. evaluating "Severe Drought
> Teleconnections in the Western U.S. and Possible Climatic Forcing by
> the Pacific Decadal Oscillation". The authors extensively use the PDSI
> reconstructions of Cook et al. (1999) in their analyses. Aside from
> the increased length of their new tree-ring chronology, what does it
contribute
> that was not possible simply by using the Cook et al. reconstructions
> to test for teleconnections and forcing. None of the indices of
> forcing
(ENSO,
> PDO, sunspots) extend back before the beginning of the Cook et al.
> reconstructions, so there is little to be gained in using one longer
series
> from west-central Montana in this analysis. One could point to Fig. 3,
> which compares the MT reconstruction vs the SWDI series. But even this
> comparison is limited in its overall contribution to the paper. I also
> don't like the use of the FFT for estimating power spectra, even if
> the confidence limits are determined by bootstrapping. The power
> spectra calculated by the FFT are still inconsistent estimates. A more
contemporary
> and consistent method of spectral estimation, like the Multi-Taper
> Method,

> should be used.
> For the reasons stated above, I do not consider this paper to be ready
> for

> publication as is. I will leave it to the Editor to decide how to
> proceed with it past this point.
>
>
>
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>
>
>
> At 10:33 PM 7/23/03 -0500, you wrote:
>
>>Dear Keith,
>>
>>I have not yet received an editorial response or reviews for the
manuscript
>>entitied "Using a new 672-year tree-ring drought reconstruction from
>>west-central Montana to evaluate severe drought teleconnections in the
>>western US and possible climatic forcing by the Pacific Decadal
Oscillation"
>>by [redacted] This manuscript has been in review for 14
months.
>>
>>Can you indicate when I can expect these materials?
>>
>>Many thanks,
[redacted]
>>
>>

Briffa Keith Prof (ENV)

From: [redacted]
Sent: 24 July 2003 21:22
To: Keith Briffa
Cc: [redacted]
Subject: Re: [redacted] manuscript- sorry

Dear Keith,

I have been contacted by the lead author [redacted], and he is enthusiastic about resubmitting after substantial revision. We feel that we can address several of the reviewers comments about sample size and core inclusion.

First, we both agree that our original statement in the methods unfortunately misled the reviewers into believing that we didn't use cores with missing rings. This is not the case. Second, I wrote the section on sample size, and, unfortunately, I misinterpreted the lead author's description of how he established the chronology vis-a-vis sample size. The following explanation, which will be clarified in a potential revision, shows that we did use most (84%) of climatically sensitive cores collected:

Paraphrased notes from [redacted]

1) 152 is the total number of trees I cored. Of those, I only measured the rings of 118 cores, the remaining 34 being young (mostly less than 180
years) and composed entirely or almost entirely of rapidly growing, complacent, juvenile growth rings. I, therefore, sampled 118 trees with sufficiently long (and potentially sensitive) records for dendrochronological analysis.

2) Of the 118 cores, 61 were used to establish the chronology (many with missing rings), and 57 cores were rejected due to "complacency, unresolvable sections of missing rings, or low interseries correlation values." Probably half of these 57 cores (~30) were from trees growing near the lake level or in small, potentially wetter, ravines on the slope where you would expect a weak climate signal. I cored them anyway because they were some of the largest trees in the region and to increase sample size, but I anticipated that they may be problematic, which they were. These 30 cores were therefore rejected.

3) Approximately 13 of the 57 were rejected for other reasons such as multiple breakage points.

4) The remaining 12 rejected samples were extremely difficult to include in the chronology and may heve been included had replicate cores been sampled from each tree to aid in identifying multiple sets of missing rings. Given logistical and time constraints during sampling, however, I opted for increasing the sample size of the number of replicate trees rather than pseudoreplicated cores from a single tree. It's possible we can re-investigate these 12 samples.

Thus, of the 118 cores with long records, only 73 were truly suitable for analysis. Of these cores, 84% (61 cores) were used to build the chronology, only 12 cores were omitted because of being extremely difficult to include in the chronology.

We would be grateful for any initial thoughts you might have.
All the best,
[redacted]

--On Thursday, July 24, 2003, 1:49 PM -0500 [redacted]