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Climate of the Past Discussions
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Interactive comment on "Blue Intensity based experiments for reconstructing North Pacific temperatures along the Gulf of Alaska" *by* Rob Wilson et al.

Rob Wilson et al.

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WE VERY MUCH APPRECIATE THE DETAILED CRITIQUES FROM BOTH REVIEWERS AND WE HOPE WE HAVE ADDRESSED ALL THEIR ISSUES TO BOTH THEIRS AND THE EDITOR'S SATISFACTION. I SHOULD STATE THAT I HAVE ALREADY MADE CHANGES TO THE MANUSCRIPT AND AM HAPPY TO SUBMIT THE EDITED VERSION IF THE EDITOR AGREES WITH THE CHANGES WE HAVE MADE.

Review 1

General comments:

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Discussion paper

The major problem with this manuscript is the combination of a great number of models tested and the great variability among the different models tested. Because of this combination it could be argued that the successful models have been achieved spuriously. WE THINK THIS IS A LITTLE UNFAIR AS THIS IS AN EXPLORATORY PAPER WHICH PRESENTS A RANGE OF RESULTS (BUILDING FROM SIMPLE RW DATA THROUGH TO A MULTIVARIATE AMALGAM OF USING MULTIPLE TR PARAMETERS) WHICH PROVIDE GUIDANCE FOR THE CONTINUATION OF THIS WORK AS WE EXTEND THESE NEW DATA BACK INTO THE 1ST MILLENNIUM AD. THIS PAPER NEVER AIMED TO DERIVE A NEW DEFINITIVE RECONSTRUCTION FOR THE REGION. THE MAIN CONCLUSION FROM THIS INITIAL LIMITED DATASET IS THAT THE DB PARAMETER LIKELY EXPRESSES THE BEST COMPROMISE BETWEEN HIGH AND LOW FREQUENCY CALIBRATION FIDELITY (I.E. LWB LOW FREQUENCY IS SEVERELY BIASED). WE FIND IT STRANGE THAT THE REVIEWER THINKS THAT WE HAVE ATTAINED A SPURIOUS RESULT AS WE HAVE BEEN CAUTIOUS WITH OUR CONCLUSIONS.

WE ARE HOWEVER WILLING TO MAKE SOME CHANGES TO ALLAY THE REVIEWER'S CONCERNS. AN AGE DEPENDENT SPLINE CHRONOLOGY VERSION CAN BE ADDED TO FIGURE 6. FOR FIGURE 7, RATHER THAN USE A SINGLE RECONSTRUCTION, WE CAN DERIVE A LWB AND DB BASED GOA COMPOSITE RECONSTRUCTION BASED ON ALL THE CHRONOLOGY VARIANTS AS WEIGHTED MEANS RELATED TO THEIR CALIBRATION R2 VALUES TO THE 1901-2010 CALIBRATION PERIOD. THE NEW FIGURES 6 AND 7 ARE ATTACHED SEPARATELY. THE RESULTS DO NOT CHANGE WITH THIS APPROACH BUT DOES DISTANCE THE ANALYSIS FROM ANY SUBJECTIVE CHOICES OF USING ONE PARAMETER CHRONOLOGY VARIANT OVER ANOTHER.

To attempt to avoid this, I recommend to limit the number of models tested, by performing also climate calibrations with high-pass filtered data (see suggestions below) and to combine this with a discussion of which monthly temperatures can have a causal ef-

fect on tree growth. Conducting this additional analysis would narrow down the options that can be tested, but also function as a baseline for the discussion of low-frequency skill in the data. If the high-frequency part of the data is agreeing well with temperature, it is likely safe to assume that a breakdown of agreement when low-frequencies are added is due to low-frequency biases, such as HW-SW-, standardization, etc.. problems. When this is established then tests of how to minimize the loss of signal at lower frequencies can be conducted (different standardizations alternatives). If however, the high-frequency part of the data does not agree very well with temperatures in the first place, it is very unlikely to expect that adding the low-frequency part will contribute with useful information even if correlations are boosted. Therefore, the high-frequency analysis must come first and inform subsequent choices of configurations and options. DENDROCLIMATOLOGY, UNLIKE MOST OTHER PALAEOCLIMATE APPROACHES HAS THE ABILITY TO DERIVE SO-CALLED ROBUST ESTIMATES OF PAST CLIMATE AT INTER-ANNUAL TO CENTENNIAL TIME-SCALES. ALTHOUGH WE AGREE THAT LOW FREQUENCY TRENDS CAN LEAD TO SPURIOUS CORRELATIONS, WE HAVE BEEN VERY CAREFUL IN TESTING BOTH THE HIGH AND LOW FREQUENCY FIDELITY OF THE MODELS WE PRESENT. THE LOW FREQUENCY FIDELITY IS PARTICULARLY DIFFICULT TO EXAMINE AS WE MUST ASSUME GREATER UNCERTAINTY IN THE EARLY INSTRUMENTAL DATA. IN FACT, THE AMBIGUITY OF THIS PAPER IS THE ASSESSMENT OF THE LOW FREQUENCY SIGNAL IN THE LWB AND DB DATA.

HOWEVER, THE REVIEWER IS ENTIRELY CORRECT THAT WE IDEALLY WANT “EQUAL” COHERENCE WITH PAST TEMPERATURES AT BOTH HIGH AND LOW FREQUENCIES TO THE SAME SEASON. RATHER THAN ADD FURTHER CALIBRATION EXPERIMENTS USING MORE FLEXIBLE DETRENDING OPTIONS, WE FEEL THAT A VALID COMPROMISE IS TO ALSO PRESENT (AS AN EXTRA SUPPLEMENTARY FIGURE) THE CORRELATION RESPONSE FUNCTION ANALYSIS RESULTS OF FIGURE 2 AFTER THE DATA (TR AND TEMPERATURE) HAVE BEEN TRANSFORMED TO 1ST DIFFERENCES. THIS NEW FIGURE VERSION IS ALSO

ATTACHED.

THE RW BASED CORRELATIONS SHOW STRONGEST CORRELATIONS WITH JJA, BUT ARE STILL RELATIVELY HIGH WITH THE BROADER WINDOW OF FEB-AUG. THE REVIEWER IS THEREFORE CORRECT THAT THERE IS SOME SPURIOUS TREND RELATED CORRELATION CREEPING IN FOR RW, BUT IS ONLY MINOR AND ARGUABLY IRRELEVANT FOR THIS PAPER AS THE FOCUS OF THE PAPER IS ON THE BLUE INTENSITY BASED PARAMETERS ANYWAY.

THE EWB 1ST DIFFERENCED CORRELATIONS AT INTER-ANNUAL TIME-SCALES ARE NON-SIGNIFICANT. FOR LWB AND DB, THE CORRELATIONS ARE OVERALL SIMILAR AS THE ORIGINAL FIGURE 2 ALTHOUGH THE SEASONS INCLUDING WINTER MONTHS ARE WEAKER. AGAIN – THIS DOES NOT IMPACT THE PAPER AS THE SUMMER MONTHS WERE THE TARGET CALIBRATION SEASONS. AGAIN, AS WITH THE ORIGINAL FIGURE 2, THE HIGH FREQUENCY CORRELATIONS ARE GENERALLY HIGHER FOR LWB THAN DB AND ULTIMATELY. AS IS CLEAR LATER IN THE PAPER, LWB PORTRAY RATHER SPURIOUS LOW FREQUENCY TRENDS. THE MAIN ISSUE THEREFORE IS WHICH OF JJA OR JJAS ARE THE OPTIMAL SEASONS TO CALIBRATE AGAINST (JJAS ALWAYS SLIGHTLY BETTER) AND, MORE IMPORTANTLY, HOW ONE COMBINES THE DIFFERENT PARAMETERS – I.E. USE RW AND DB IN A MULTIPLE REGRESSION (AS SHOWN HERE), OR UTILISE A BAND-PASS APPROACH AND USE THE RW AND LWB DATA AT THE FREQUENCIES WHERE THEIR SIGNAL IS “ROBUST” – SEE RYDVAL ET AL. 2017 FOR AN EXAMPLE. THIS LATTER APPROACH WILL BE EXPLORED IN A LATER PAPER.

A secondary issue is that the authors use reflected BI. This type of data is negatively correlated with what the authors claim to measure in the wood: cell wall, lignin content, but also with the discolorations. If the authors would opt to use the absorbed BI it would let them completely avoid many confused elaborations (see detailed comments below) with regard to standardization and comparisons with MXD etc. THIS IS A SEMANTIC

COMMENT ABOUT TERMINOLOGY AND DOES NOT IMPACT THE ANALYSIS IN ANY WAY. I BELIEVE THAT OUR METHODOLOGICAL DESCRIPTION IS CLEAR ALTHOUGH A MINOR CLARIFICATION IS POSSIBLE (SEE BELOW).

AS LIGNIN ABSORBS [BLUE] LIGHT, THEN DENSE LATEWOOD (HIGH DENSITY) WILL REFLECT LESS BLUE LIGHT. HENCE RAW LWB AND MXD ARE INVERSELY CORRELATED. THE ONLY REASON THAT RAW LWB NEED TO BE INVERTED IS DUE TO LIMITATIONS OF THE FREELY AVAILABLE DETRENDING SOFTWARE (I.E. ARSTAN) WHERE IT IS THE NORM TO REMOVE NEGATIVE/ZERO SLOPE TRENDS AND RETAIN POSITIVE TRENDS. THEORETICALLY, THE TRENDS IN LWB WILL BE OPPOSITE TO MXD, BUT IN THE SOFTWARE THERE ARE NO OPTIONS TO REMOVE POSITIVE/ZERO SLOPE TRENDS AND RETAIN NEGATIVE ONES. HENCE THE RAW LWB DATA NEED TO BE INVERTED FOR DETRENDING. THIS APPROACH HAS BEEN USED IN WILSON ET AL. (2011, 2014, 2017) AND RYDVAL ET AL (2014, 2016, 2017) PLUS OTHER PAPERS AND AS FAR AS WE ARE AWARE IT IS ONLY BJÖRKLUND ET AL. WHO HAVE SUGGESTED USING THE TERM “MAXIMUM LATEWOOD BLUE ABSORPTION INTENSITY (MXBI)”.

AS A SUBTLE RE-WORDING WE ARE WILLING TO TWEAK THE METHODOLOGICAL TEXT AS FOLLOWS:

“RAW EWB AND LWB VARIABLES WERE MEASURED USING COORECORDER 8.1 SOFTWARE (CYBIS 2016 - HTTP://WWW.CYBIS.SE/FORFUN/DENDRO/INDEX.HTM), WHICH HAS STATE-OF-THE-ART CAPABILITIES TO ACQUIRE ACCURATE REFLECTANCE INTENSITY RGB COLOUR MEASUREMENTS FROM SCANNED WOOD SAMPLES (SEE RYDVAL ET AL. 2014). DB VALUES WERE CALCULATED WITHIN COORECORDER BY SUBTRACTING THE RAW LWB VALUES FROM THE RAW EWB VALUES FOR EACH YEAR. SINCE RAW LWB IS NEGATIVELY CORRELATED TO MXD (HIGH DENSITY ‘DARK’ LATEWOOD = LOW REFLECTANCE), VALUES WERE INVERTED FOLLOWING THE METHOD DETAILED IN RYDVAL ET AL. (2014) TO ALLOW FOR

LWB (HEREAFTER DENOTED AS LWBINV) TO BE DETRENDED IN A SIMILAR WAY TO MXD (SEE ALSO WILSON ET AL. 2014). THE NATURE OF THE DB CALCULATION RESULTS IN THIS PARAMETER BEING POSITIVELY CORRELATED WITH INVERTED LWBINV, SO THESE DATA COULD ALSO BE THEORETICALLY DETRENDED IN A SIMILAR WAY."

AS A FURTHER COMPROMISE TO THE REVIEWER, WE ARE ALSO HAPPY TO ADD IN A CLEAR STATEMENT REFERRING TO BJÖRKLUND ET AL. (2014/2015) STATING THEIR TERMINOLOGY FOR INVERTED LWB.

FINALLY, WE BELIEVE IT IS IMPORTANT TO TREAT BI RELATED PARAMETERS INDEPENDENTLY OF DENSITY. THEY ARE RELATED NO DOUBT, BUT TRYING TO FIT BI TO DENSITY IS A POTENTIALLY DANGEROUS APPROACH. ALTHOUGH MEASURING SIMILAR PROPERTIES, WE CANNOT EXPECT THEM TO BE EXACTLY SIMILAR – ESPECIALLY W.R.T. AGE RELATED TRENDS. ALSO - IT IS NOT REALLY CLEAR WHAT MAXIMUM EARLY WOOD REFLECTANCE (EWB) ACTUALLY REPRESENTS AND IT IS LIKELY THAT THIS PARAMETER IS RELATED TO LUMEN SIZE RATHER THAN ANY PROPERTY REFLECTING SPECIFICALLY COMPOUNDS IN THE EARLYWOOD CELL WALLS.

In conclusion, I find the manuscript well written and prepared but I strongly suggest adding a high-frequency analysis, and using absorbed BI. After these revisions and the implementation of the comments below, the manuscript should be suitable for publication. We hope the 1st differenced based results shown above can address the reviewer's concerns on the FIRST POINT AND WE FEEL NO OBLIGATION TO CHANGE OUR TERMINOLOGY TO ADDRESS THE SECOND POINT AS IT DOES NOT CHANGE THE ANALYSIS/RESULTS IN ANY WAY AND WE FEEL OUR CURRENT DESCRIPTION IS ADEQUATE AND CONSISTENT WITH MOST PREVIOUS PUBLICATIONS.

Detailed comments:

L45 Remove “However” AGREED – THIS CAN BE DONE WHEN “ALLOWED” TO EDIT THE PAPER FOR FINAL PUBLICATION

L49 replace “for” with “covering” AGREED – THIS CAN BE DONE WHEN “ALLOWED” TO EDIT THE PAPER FOR FINAL PUBLICATION

L54-59 This section only discuss the non-climatological variance distorting RW signal, and does not acknowledging that RW and LWB actually may contain different climatic fingerprints. I suggest adding something along this line. PLEASE SEE ATTACHED 1ST DIFFERENCED CORRELATION RESPONSE FUNCTION ANALYSIS RESULTS. THESE CAN BE ADDED TO THE SUPPLEMENTARY AND APPROPRIATE DISCUSSION ADDED.

L70 Björklund et al worked with absorbed Maximum BI BJÖRKLUND’S ABSORBED MAXIMUM BI IS THE SAME AS THE INVERTED LWB USED IN THIS PAPER. WE FEEL THE CURRENT DESCRIPTION IS CLEAR ENOUGH – ESP. WITH SUGGESTIONS MENTIONED ABOVE.

L71-72 Here and in many other places it would be much simpler to start talking about absorbed BI values because these values will be positively correlated with the properties that you mention as potential measurement targets. Why measure the inverted value of what you are interested in? In this way just confuse readers about what you had to do before standardization to make them work properly and why BI is inversely correlated with density etc. SEE COMMENTS ABOVE. I AM SURE BJÖRKLUND MEASURED RAW INTENSITY VALUES OF BLUE REFLECTANCE AND THEN INVERTED THE DATA AS WE HAVE DONE. THE DIFFERENCE AFTER THAT IS SEMANTIC ONLY.

L80-81 This is true if we disregard the principle of diminishing records back in time. THIS HOLDS TRUE DESPITE THE REDUCTION IN REPLICATION BACK IN TIME. THE COMMUNITY NEEDS TO BE CAREFUL AS TO DEFINE CLEAR THRESHOLD OF TRUNCATION. IN THIS PAPER, THE DATA ARE FAR FROM IDEAL W.R.T. REPLI-

CATION – THAT WAS PARTLY STRATEGIC. THE FACT THAT THE RESULTS ARE ENCOURAGING SUGGESTS THAT CALIBRATION/VERIFICATION WILL IMPROVE SUBSTANTIALLY AS REPLICATION IS INCREASED. NOT SURE THIS COMMENT WARRANTS ANY SPECIFIC CHANGE.

L92-93 Björklund et al 2014 subtracted average absorption Earlywood BI from maximum absorption BI. THIS AGAIN HIGHLIGHTS WHY WE PREFER OUR CURRENT METHODOLOGICAL DESCRIPTION. WE HAVE USED THE RAW EWB AND LWB VALUES AND USED THE DIFFERENCE TO DERIVE THE DB VALUE. THIS IS MATHEMATICALLY THE SAME AS WHAT THE REVIEWER WANTS US TO IMPLEMENT, BUT WE SEE NO GAIN WITH SUCH A CHANGE AS WE ARE DOING THE SAME METHOD, BUT USING DIFFERENT TERMINOLOGY WHICH IS CLEARLY DEFINED.

L96-98 This sounds like a hypothesis you are going to test later in this paper, but it is not really tested. I would phrase it more like a discussion point: If EWB and LWB contain similar climatic responses and similar standard deviations. . . AGREED – THIS IS A LITTLE VAGUE AND REVIEWER 2 ALSO FLAGGED THIS. WE WOULD GLADLY CHANGE THIS TEXT (AND ASSOCIATED LATER DISCUSSION) TO BASICALLY HIGHLIGHT THAT IF EWB AND LWB BOTH EXPRESS THE SAME CLIMATIC RESPONSE (THIS SHOULD NOT BE THE CASE), THE RESULTANT DERIVED DB DATA WILL NOT SHOW THIS COMMON RESPONSE AND LIKELY BE INFERIOR TO LWB. THIS IS AN EVOLVING PROPERTY OF DB AND CERTAINLY NEEDS MORE EXAMINATION USING MORE SPECIES AND LOCATIONS.

L100-101 Not really “another concern”. I suggest changes to something like this (I let you worry bout the grammar and English): Finally, although BI based variables hold great promise as an alternative proxy to MXD at inter-annual time-scales, the potential ability of BI to capture decadal to centennial time-scales related to long term-climate changes is still under question. AGREED – HAPPY TO RE-WRITE THIS SECTION AND CLARIFY THE MESSAGE BETTER.

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L102 Please clarify if you mean HW-SW color difference AGREED. YES – HW-SW – THIS CAN BE CLARIFIED WHEN "ALLOWED" TO EDIT THE PAPER FOR FINAL PUBLICATION

L131-134 If you decide to use absorbed BI values this entire section can be removed. If you decide to keep it as is, I strongly recommend to go in to a discussion about why the detrending alternatives are sensitive to this. For example, deterministic detrending such as Neg. exp. or hugershof assume a decline in data values with age. If data values instead have an assumed increase, these methods will be useless. The reason for wanting this added discussion is that some researchers have missed this point and use these methods also for reflected BI. AS DISCUSSED ABOVE, WE ARE HAPPY WITH OUR CURRENT TERMINOLOGY AND METHODS DESCRIPTION.

L136-138 I recommend expanding this to also include a more aggressive detrending, perhaps a 25- 35-year spline. This will give more robust climate correlation result. If there are lingering trends in the tree-ring data, and there will be some using 200-year-splines, the risk of spurious trend correlations is relatively high. Adding a high frequency alternative can help to better identify important months for tree-growth. I suspect that some months enter your models just because they have similar trends as the tree-ring data. Also, before performing the aggressive alternative, the climate data should also be detrended similarly. Furthermore, I recommend to restructure the presented results; The high frequency monthly data analysis should be in the main manuscript and the seasonal climate correlations in the supplement together with the low-frequency counterparts. The HW-SW problem will still be present in the analysis using a 200-year spline, if you want to remove this for the analysis you need a softer spline. Rbar, PCA, climate response, between variable correlations should all be done with data with less autocorrelation: softer spline. The low-frequency alternative can be presented on the background of this analysis, but not stand alone. The models' monthly targets for reconstruction should be informed in the first place with high-frequency results. A discussion can be conducted referring to the low-frequency results but not as

a major informant of the models. THE MEAN/MEDIAN SEGMENT LENGTHS OF ALL SITES IS > 200 YEARS SO ANY “LINGERING” TRENDS FOR INDIVIDUAL SERIES WOULD BE MINIMAL USING A 200-YEAR SPLINE. THAT IS WHY WE CHOSE IT. WE CAN ADD A COMMENT TO THIS END.

WE HOPE THE ADDED 1ST DIFFERENCED BASED CRFA WILL ADDRESS THE REVIEWER’S CONCERNS AS TO OUR ANALYSIS AND THE IDENTIFICATION OF THE “CORRECT” MONTH FOR CALIBRATION.

THE REVIEWER IS SUGGESTING A MAJOR RE-ANALYSIS HERE AND IT IS NOT CLEAR WHAT GAIN THERE WOULD BE. THE 200-YR SPLINE APPROACH IS A COMPROMISE BETWEEN MINIMISING THE HW-SW BIAS WHILE RETAINING SOME REALISTIC MULTI-DECADAL INFORMATION. WE SEE NO JUSTIFICATION OF RE-DOING THE ANALYSIS USING A MUCH MORE FLEXIBLE SPLINE.

L167 Please specify which function was used to model the regional curve. YES – SORRY – WE CAN ADD THIS INFORMATION IN. THE REGIONAL CURVE WAS SMOOTHED WITH A SPLINE OF 10% THE RC SERIES LENGTH AND THAT FUNCTION USED FOR DETRENDING.

L171-172 LINres has been shown to create quite some bias in resulting chronologies, see works of Melvin and Briffa, especially if used to model the RC. I instead recommend time-varying response smoother Melvin et al., 2007. AGREED THAT A LINRES APPROACH MAY IMPART BIASES, BUT WE WOULD ARGUE THAT ALL DETRENDING APPROACHES HAVE THEIR OWN BIASES. THAT IS WHY WE HAVE PRESENTED A SMALL SUB-SET OF POSSIBLE DETRENDING CHOICES. WE COULD HAVE EXPANDED ON THIS SUBSTANTIALLY, BUT THAT WILL BE A FOCUS WHEN WE HAVE THE FULL 1000+ YEAR DATA-SET. I WOULD LIKE TO REMIND THE REVIEWER THAT THE LINRES APPROACH HAS BEEN USED ON MANY DENSITY BASED STUDIES IN THE PAST.

MORE IMPORTANTLY, THE AGE-DEPENDENT SPLINE APPROACH OF MELVIN ET

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AL. (2007) IS VERY MUCH AN UNTESTED DETRENDING APPROACH. I HAVE EXPERIMENTED WITH THIS OPTION AND (1) IT CAN OFTEN INFLATE RECENT PERIOD VALUES AND (2) IMPLICITLY WILL REMOVE SECULAR SCALE VARIATION – IT IS STILL A SPLINE APPROACH.

HOWEVER, WE ARE HAPPY TO ADD IN AN AGE DEPENDENT SPLINE VERSION INTO THE MIX FOR FIGURES 6 AND 7 – SEE ATTACHED FIGURES. TABLE 5 CAN BE UPDATED – SEE ALSO ATTACHED FIGURE. THE R2 VALUES FOR THE 1901-2010 PERIOD CAN THEN BE USED AS WEIGHTING TERM TO COMBINE ALL THESE DIFFERENT VARIANTS TO DERIVE GOA WEIGHTED COMPOSITES FOR PARAMETER. THE UPDATED FIGURE 7 IS ATTACHED. THE UPDATED FIGURE 8 (WHICH NOW INCLUDES THE WEIGHTED LWB DATA) IS ALSO ATTACHED, WHICH CLEARLY SHOWS THAT DESPITE REASONABLE CALIBRATION AND VERIFICATION (FIGURE 7, TABLE 5), THE LOW FREQUENCY TRENDS OF THE LWB DATA DO APPEAR AT ODDS WITH THE OTHER DATA-SETS. WE WILL EXPAND THE DISCUSSION ON OTHER STUDIES INDICATING A COOLER RATHER THAN WARMER LITTLE ICE AGE.

L177-181 I suspect the results could be somewhat different with the high-frequency data analysis, see recommendations above. If they are, this is going to be vital information for your main question in the introduction: b) whether meaningful low frequency information can be gleaned from these data? Furthermore, if they are very different, the continuation of the question: “exploiting the long monthly instrumental temperature records that go back into the mid-19 validate secular trends in the TR data” becomes heavily diluted. THE CURRENT ANALYSIS, AS A FIRST ATTEMPT, ALREADY ADDRESSES THE LOW FREQUENCY ISSUE WITH APPROPRIATE DISCUSSION AND THE 1ST DIFFERENCE RESULTS VALIDATE WELL THAT THE APPROPRIATE SEASON HAS BEEN TARGETED.

ULTIMATELY, IT WILL NEVER BE POSSIBLE TO IDENTIFY THE “CORRECT” DETRENDING APPROACH AND WHEN THE FINAL DATA HAVE BEEN FINALISED

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WE WILL APPROACH THE RECONSTRUCTION USING A SIMILAR ENSEMBLE APPROACH AS INTRODUCED BY WILSON ET AL. (2014) FOR A RELATED STUDY IN THE CANADIAN ROCKIES. THIS WILL ALLOW DETRENDING UNCERTAINTY TO BE EVALUATED IN THE ERROR ESTIMATES. AT THIS TIME, WE JUST WANTED TO HIGHLIGHT THE SENSITIVITY TO SUCH METHODOLOGICAL CHOICES – FIGURE 6 DOES THIS WELL.

L198 Again, must be done also with high-frequency data. Should likely cut off some month, and give a better causal reflection of which months are important for radial tree growth. High and persistent correlations with consecutive months makes me suspect trend-correlations. SEE ABOVE. THE 1ST DIFFERENCED CRFA ADDRESSES THIS.

L217-219 Awkward sentence, please rephrase. THIS SENTENCE CAN BE TWEAKED WHEN “ALLOWED” TO EDIT THE PAPER FOR FINAL PUBLICATION

L227 in both or just in the new one? WE BELIEVE THE CURRENT WORDING IS QUITE CLEAR THAT THIS IS ONLY FOR THE LATTER “NEW DATA” RW BASED RECON.

L265-271 Use absorption BI to avoid confusing comparisons with MXD. SEE COMMENTS ABOVE.

L272-273 The original DB was introduced in Björklund et al., (2014), but it was further developed in Björklund et al., (2015) were they used a contrast adjustment. More discolored samples had a systematically lower contrast between earlywood and latewood than less discolored samples. If there is a systematic difference in discoloration then this will affect also the traditional DB data. You can easily test if there is a contrast problem in your data with scatterplots of DB vs EWBI, as done in Björklund et al., (2015). If there is a relationship you might at least want to discuss this. If there is not a relationship you will have cleared a question mark. THIS IS A GOOD POINT, BUT IS NOT RELEVANT TO THE ANALYSIS PERFORMED FOR THIS CURRENT PAPER. WE BELIEVE, FOR THE CURRENT DATA ADAPTIVE DETRENDING TECH-

NIQUES USED HEREIN, THAT THIS ISSUE IS NOT YET RELEVANT, BUT, WILL BECOME RELEVANT AS WE INCORPORATE SNAG AND SUB-FOSSIL MATERIAL TO EXTEND THE REGIONAL COMPOSITE CHRONOLOGY BACK IN TIME.

L276-281 According to my experience the age-trend of MXD would be more similar to DB than LWB. Perhaps different detrending options are needed, but if age-dependent splines are used, as suggested before, these would adapt to the small differences in the data. Neg. exp. or linear functions, for instance, may be directly inappropriate when having juvenile phases of increase and then followed by a decline. AS PREVIOUSLY MENTIONED THE CURRENT "CONSERVATIVE" APPROACHES AIM TO RETAIN LOW FREQUENCY INFORMATION. WE BELIEVE THE AGE DEPENDENT SPLINE WILL REMOVE SUCH TRENDS. HOWEVER, AN AGE DEPENDENT SPLINE OPTION IS NOW INCLUDED IN THE ANALYSIS. IT DOES NOT CHANGE ANY OF THE RESULTS!

L278 Again use absorption BI. SEE ABOVE.

L283-288 Use absorption BI to avoid having to clarify what you mean. NO – SEE ABOVE. THE DISCUSSION WILL NOT CHANGE BY USING DIFFERENT TERMINOLOGY.

L288-292 It seems as a contradiction to write that LWB (as temperature proxy) should not have a negative trend w.r.t glacier advancements? The glacier advancement was stable up until 1800 CE and glacier advancement peaked around the turn of the 20th century. Would fit very well with the LWB record that has no trend from 1600-1800 CE and then a negative trend from 1800-1900 CE. The problem would be that there is no pronounced positive trend in the 20th century to melt away the glaciers that expanded prior to this. WE BELIEVE THE REVIEWER IS PERHAPS A LITTLE CONFUSED HERE. ALL LWB VARIANTS IMPLY WARMER THAN AVERAGE (20TH CENTURY) TEMPERATURES PRIOR TO 1850. THIS DOES NOT FIT WITH THE GLACIAL EXPANSION DATA SHOWN IN FIGURE 8. THE DB BASED RECONSTRUCTION (AND

VARIANTS) HOWEVER, DENOTE COOLER THAN AVERAGE (20TH CENTURY) TEMPERATURES PRIOR TO 1850 WHICH IS IN LINE WITH COOLER CONDITIONS NEEDED FOR CONTINUED GLACIAL EXPANSION THROUGH THIS PERIOD.

L343 Conclusions sections is very long and more like a summary of the discussion 1.5 PAGES OF SUMMARY AND RECOMMENDATIONS APPEAR APPROPRIATE TO US. IN FACT, A LITTLE MORE DISCUSSION MAY BE ADDED ABOUT POTENTIAL FUTURE STRATEGIES TO ADDRESS THE HIGH AND LOW FREQUENCY ISSUES OF THE LWB VS DB VS RW DATA.

L349-353 I would recommend to test high frequency results before making these bold statements. That is, to first to rule out any trend correlations with winter months for ring-width. After all it is very unusual for ring width to have a broader temperature response than BI or density se e.g. Briffa et al., (2002). BEYOND THE SCOPE OF THIS PAPER, IF THE RW DATA ARE REGRESSED ON THE DB DATA, THE RESIDUALS FROM THIS ANALYSIS CORRELATE WITH WINTER TEMPERATURES. SEE WILSON ET AL. (2007) WHERE THE RW COMPOSITE CLEARLY PICKS UP DECADE SCALE SHIFTS SEEN IN THE PDO AND OTHER METRICS OF PACIFIC DECADAL VARIABILITY. SUCH SHIFTS ARE NOT SEEN IN THE BI BASED PARAMETERS.

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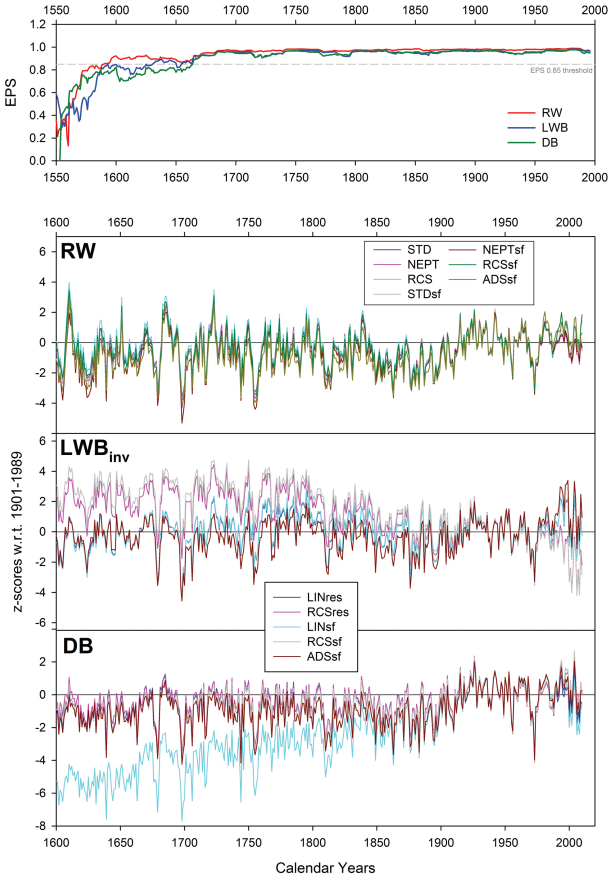

Fig. 1.

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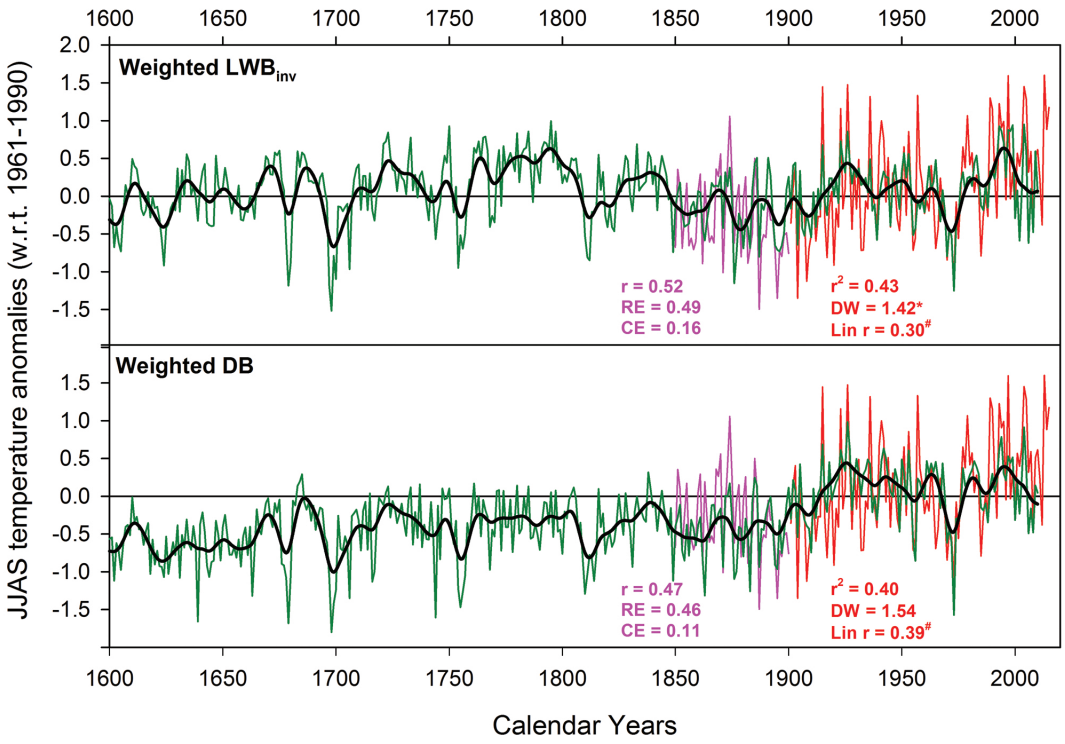

Fig. 2.

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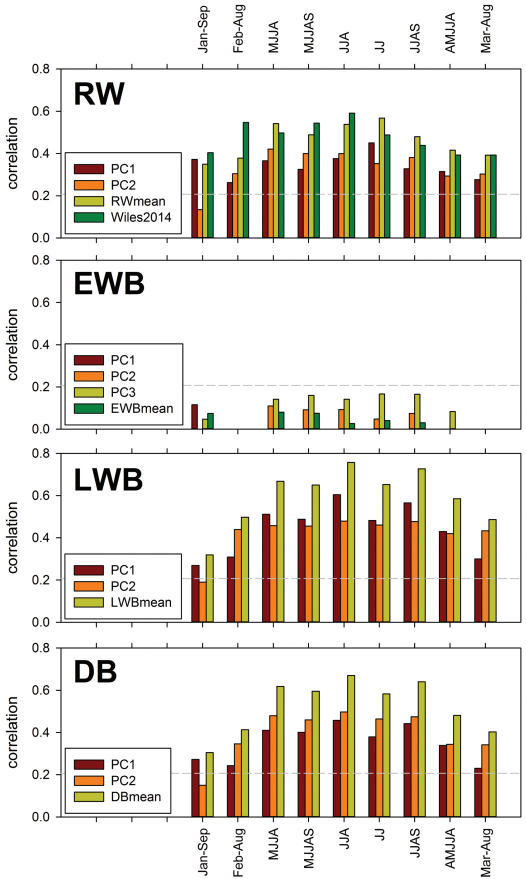
RW
PC1
PC2
RWmean
Wiles2014
EWB
PC1
PC2
PC3
EWBmean
LWB
PC1
PC2
LWBmean
DB
PC1
PC2
DBmean
correlation
0.0
0.2
0.4
0.6
0.8
Jan-Sep
Feb-Aug
MJJA
MJJAS
JJA
JJ
JJAS
AMJJA
Mar-Aug

Fig. 3.

	1901-2010 Calibration					1850-1900 Validation		
	series entered	r	r2	DW	LINr	r	RE	CE
LWB	LINres	0.64	0.41	1.36	0.36	0.53	0.44	0.07
	RCSres	0.26	0.07	1.28	0.48	0.56	0.01	-0.64
	LINsf	0.64	0.41	1.37	0.36	0.53	0.43	0.06
	RCSsf	0.21	0.05	1.32	0.46	0.56	-0.05	-0.73
	ADSsf	0.69	0.47	1.58	0.06	0.50	0.51	0.20
	1901-2010 Calibration					1850-1900 Validation		
	series entered	r	r2	DW	LINr	r	RE	CE
DB	LINres	0.55	0.31	1.37	0.50	0.50	0.52	0.21
	RCSres	0.64	0.40	1.59	0.40	0.48	0.50	0.18
	LINSF	0.54	0.29	1.35	0.38	0.43	0.40	0.00
	RCSsf	0.65	0.43	1.64	0.35	0.47	0.48	0.15
	ADSsf	0.65	0.42	1.59	0.30	0.47	0.34	-0.09

Fig. 4.

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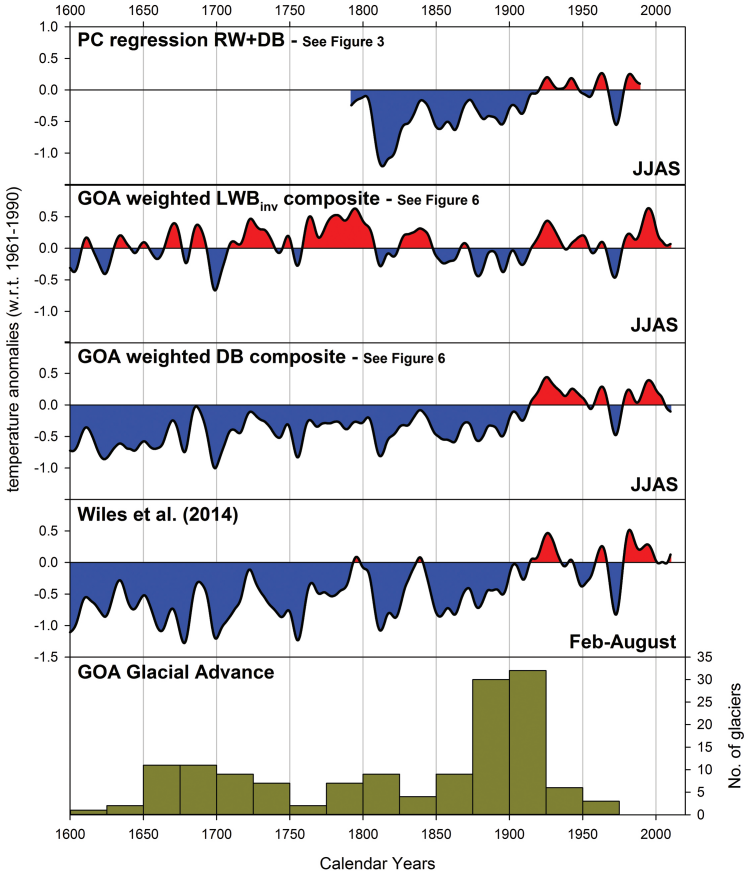

Fig. 5.

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