

Interactive comment on “Summer-temperature evolution on the Kamchatka Peninsula, Russian Far East, during the past 20,000 years” by Vera D. Meyer et al.

Anonymous Referee #2

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The manuscript “Summer-temperature evolution on the Kamchatka Peninsula, Russian Far East, during the past 20,000 years” presents a terrestrial MAT temperature reconstruction based on branched glycerol dialkyl glycerol tetraethers analyzed in a radiocarbon-dated marine sediment core from the NW Pacific margin. The interpretation of the GDGTbr records as reflecting terrestrial temperatures is well defended, and independently-dated records of this nature are sorely needed for the NW Pacific. The paper would be improved by a more rigorous handling of some of the more quantitative aspects of comparison with other regional/global records: specifically improved evaluation/presentation of the errors in the temperature reconstruction, as well as the chronology of the core. A more quantitative treatment of the comparison of the Kam-

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chatkan MAT temperature record to the Greenland ice-core NGRIP d18O record would also strengthen the paper. This is particularly important given the strong statements the authors make w/re the North Atlantic ‘obviously’ driving NW Pacific climate. Detailed suggestions are below.

Abstract Line 12: Rather than ‘western continental margin off Kamchatka/marginal Northwest Pacific’ suggest ‘western continental margin off Kamchatka in the Northwest Pacific’

1. Introduction Line 32-37: Long, awkward sentence. Suggest reworking for clarity.

Line 37: Unsure why this sentence begins with ‘Particularly’ in context of previous sentence.

On line 40: Statement that majority of sea surface temperature records from the sub-arctic NW Pacific and marginal seas mirror N. Atlantic climate oscillations needs to be qualified. I am assuming that this claim pertains to millennial scale N. Atlantic climate oscillations as recorded from ice core d18O? If so, Caissie et al. 2010 reference is for a surface ocean temperature record of multi-millennial scale resolution with very limited chronological control that bears little resemblance in structure to NGRIP d18O outside of a crude transition from apparently full glacial to interglacial conditions between 12-11 ka. Of the 6 records from the ‘NW Pacific and marginal seas’ presented by Max et al., 2010, while the nearly all the color b* records resemble deglacial NGRIP d18O in structure, only one of the attendant SST records (the NW Pacific core SO201-12-KL that is also the subject of this paper) looks anything like NGRIP d18O at millennial scales. I can’t comment on the Meyer et al. reference as it’s unpublished.

If correct, this rather sweeping assertion that NW Pacific SST mirrors N. Atlantic climate has fairly important implications vis-a-vis the following suggestion that N Atlantic teleconnections control deglacial temperature development in the N. Pacific. However this assertion seems poorly defended by the references offered in the text at this point and at the very least needs further qualification/elaboration.

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Line 44: In the marine environment you only address records from the broader NW Pacific, but then for terrestrial records you include records from the Alaskan portion of Beringia. Why is there no discussion of the well-dated marine records of climate from the NE Pacific, or alternatively, why are the terrestrial Alaskan records being included in this discussion?

2. Regional Setting Line 71: Suggest replacing 'which are' with a comma.

Line 75: Why is Jet capitalized? Should it be 'westerly jet stream'? Similarly 'Jetstream' in line 78 should also be two lowercase words.

Line 82: Add comma after 'ranges'.

Line 84: Would read better as 'Mean temperatures averaged for the entire Peninsula range from...'. Alternatively, place a comma after 'Peninsula'.

3. Materials and Methods 3.1 Core material and chronology

Although you reference Max et al. (2012) for details of chronology, as it's highly pertinent to this paper it would be nice to have some basic information offered on the core length and the number of radiocarbon dates that constrain accumulation. Similarly, as these results are the subject of another paper, a mention of the mean sedimentation rates in the Holocene and deglacial sections of the core (properly cited) would be useful to the reader.

3.2 Lipid Extraction Line 101: No need for a comma after n-hexane.

3.4 Temperature determination

I'm unclear from this how the BIT-index controls for GDGT's from fresh water environments?

In this and previous (3.3) section, in many locations in the text I'm unclear on what precipitates the use of the abbreviation GDGT as opposed to brGDGT? I would have guessed it was branched versus all Glycerol Dialyl Glycerol Tetraethers, but in some

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cases (if I'm not mistaken) GDGT appears to be used interchangeably with brGDGT. Really this comment could extend to entire manuscript.

Line 145: pluralize 'sample'. Also no need for 'present'.

4. Results 4.1 Concentrations and fractional abundance of brGDGT

It would seem to me that Figure 3 (and the discussion of it) should precede Figure 2 in the text.

4.2 Temperature development over the past 20 ka Line 178: This is a very narrow definition of the late Holocene (1 ka BP), and it only affords you one data point in the record of 12KL to compare to the dozen or so available for the 2 ka window afforded the glacial (18-20 ka BP). Perhaps consider broadening the definition of 'late Holocene' and when presenting a surface temperature include a standard deviation that encompasses both analytical/calibration error as well as observed variability over that time interval. Also, here and every future instance, why is approximately abbreviated?

While I don't doubt that the glacial temperatures are statistically 'the same' as those in the late Holocene, this could easily be presented quantitatively.

Line 179: In this and every instance throughout the paper, when giving a temperature, present that value in the context of its uncertainty.

Line 179-182: This pair of sentences is awkward and read poorly. The discussion of the single warm data point at 16 ka reads like a stream of consciousness as opposed to a well-digested scientific observation.

Line 181: If you're going to present ages down to the century scale, you need to include estimates of temporal error that reflect the chronological control of the core.

Line 187: What is the average mid-Holocene thermal maximum temperature between 8.0-4.0 ka BP (with errors)? Perhaps present the average temperature in that window, then give the highest temperature reached and the age (again, with errors) that that

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peak temperature is observed.

Line 188: How do you determine when the cooling trend is initiated? It would seem that the cooling arguably begins closer to 5 ka, but then again if you're interpreting at this level (and you probably shouldn't) you could argue the cooling stops by 3 ka.

Line 189: This last sentence needs to be quantitative. Also, when calculating variance, remember to use equivalent temporal windows for the Holocene and deglacial and smooth the record to a constant resolution.

5. Discussion

5.1 Sources of brGDGT and implications for CBT/MBT'-derived temperatures Line 219: Either here or in the methods section some very basic discussion of how to interpret the BIT-values should be given.

Line 221: Eminent might not be the right word choice for this sentence. Perhaps 'Marine settings where terrigenous input is low are particularly sensitive to bias from in-situ production, thus non-soil derived brGDGTs potentially have a considerable effect on the temperature...'

Line 225: Again, would suggest minor reworking. Perhaps 'Ti/Ca-ratios reflect the proportion of terrigenous and marine derived inorganic components of the sediment, and can be used as an estimator of terrigenous input'.

Line 226: 'With relatively high values at 15.5 and 12 ka BP, and minima at 14 and 11 ka BP' is an incomplete sentence. Also, again, if presenting chronologies at the centennial scale really need to give errors on those ages.

Line 244: 'Mai' should be 'May'.

5.2 Temperature evolution over the past 20 ka 5.2.1 The LGM (20-18 ka) – warm summers and the regional context

What definition of LGM are you using? Should give a reference. Clark et al., 2009 is

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the most widely used citation that I'm aware of and they define global LGM as ending at 19 ka.

Line 251: While you could say there was a 'cooling tendency' from MIS-3 into the LGM, since time moves forward when comparing the LGM to the Holocene it would be better to say 'Generally cooler LGM temperatures are thought to result from...'

Line 257: What does 'BLB' stand for?

Line 260: No need to hyphenate 'insect-data'. Also suggest rewording to 'Markovo, and EIGgytgyn and Jack London lakes'

5.2.2. Controls on MATifs

In this section you identify a possible seasonal bias in alkenone-based SST reconstructions towards warmer temperatures and dismiss them in favor of TEXL86 reconstructions. You then discuss the results of the TEXL86 reconstruction for site 12KL currently submitted for review. However there is no discussion of the already-published alkenone-based SST record for 12KL of Max et al., (2012), nor is there a presentation of this record alongside the TEXL86 record from the same site in Figure 2. For the period of overlap, it would appear that at least at this location the alkenone SST's are several degrees colder than the TEXL86 temperature reconstruction. Why would this be?

Line 266: Need to clarify that you're discussing warm Siberian summers during LGM

Line 288: As the paper Meyer et al., (submitted) has yet to pass through peer review, probably best to state that the relatively warm SST's at site 12KL may be explained by stronger-than-present influence of the Alaskan Stream.

5.2.3 The deglaciation (18 ka-10 ka BP)

Define/defend the use of the words 'strong' and 'clear' when describing the resemblance between the N-Atlantic d18O and 12KL MAT. Can you calculate covariance

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between the normalized/equivalently smoothed NGRIP d18O and 12KL MAT? To my eye they appear quite different: the Y-D is greatly compressed in the Kamchatka MAT record, the trend from the LGM to HS1 in 12KL is completely absent in NGRIP. A climate oscillation in HS1 apparently comparable in magnitude and duration to the regional expression of the Y-D (although a warming as opposed to cooling event) at 16 ka with no analogue in NGRIP is discarded from interpretation. I'm not arguing that there are similarities, but to say it's obvious or 'undoubtable' that the North Atlantic is driving NW Pacific climate via atmospheric teleconnection is a strong claim that needs to be quantitatively defensible. If this can't be done in the context of this paper, perhaps dial the tone of the text down a bit.

Also, as stated earlier in the text, when comparing 12KL to NGRIP at centennial scales chronological uncertainties in 12KL need to be addressed and stated.

5.2.4 The Holocene

The statement at line 411: "Hence it seems that the atmospheric linkage (with the N-Atlantic) that determined climate variability during the deglaciation likely persisted into the Holocene where it acted as an important driver for long-term climate changes as well as abrupt, short-lived climate events." seems poorly defended by the visual similarity between NGRIP d18O and Kamchatka MAT in Figure 2. To my eye the Holocene in the MAT record appears more variable, while the mid-Holocene thermal maximum and neoglacial cooling described for the NW Pacific region are absent in NGRIP. Quantitatively evaluating the covariance between these records would be challenging at best as the current chronology for 12KL is virtually unconstrained in the Holocene.

If this statement remains in the discussion/conclusions, at the very least some discussion of what is meant by 'long-term climate changes' versus 'abrupt, short-lived climatic events'.

6. Summary and Conclusions

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Line 415-419: This introduction to the conclusions reads awkwardly.

Line 420: Perhaps replace 'likely' with 'may' or 'could' as there is no evaluation of statistical certainty of this hypothesis.

Line 433: Again, the use of the word 'obvious' to describe the role of N-Atlantic climate in driving the NW Pacific seems somewhere between bombastic and unfounded. There are some similarities in deglacial climate, there are differences, and as yet these remain poorly quantified in the manuscript.

Figures

Figure 1: Could some kind of shading be used to more clearly denote Holocene landmasses? With apparently identical solid lines used to denote boundaries of continents, ocean currents, and rivers it's a bit difficult to visually parse.

Figure 2: As this figure includes the TEXL86 SST record from Site 12KL to be published in Meyer et al., submitted, it should probably also include the deglacial alkenone SST record from site 12KL published in Max et al., 2012.

Figure 3: As mentioned in my comments on the results section, I think this figure should be reversed with Figure 2 in its presentation order in the text. Also, instead of giving ages at 4 depths in the core, could a secondary axis with appropriately dilated/compressed ticks be added for age alongside the depth scale? If this isn't possible, would almost suggest it would be better to present results versus time than versus depth to facilitate comparison to Figure 2.

Interactive comment on Clim. Past Discuss., doi:10.5194/cp-2016-21, 2016.

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