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Climate of the Past Discussions

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.

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Dear reviewer, we appreciate your helpful suggestions to improve our manuscript very much. Please, see below how we would like to revise our manuscript.

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

Indeed, the sentence reads better without the slash. We will adopt your suggestion.

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

We suggest to rewrite the parapraph as follows: "Kamchatka is one of the least studied areas of Beringia. Since the 30 available terrestrial climate archives, such as peat sections or lake sediments, do not reach beyond 12 ka BP (e.g. Dirksen et al., 2013, 2015; Nazarova et al., 2013a; Hoff et al. 2015; Klimaschewski et al., 2015; Self et al., 2015; Solovieva et al., 2015). The climatic conditions during the LGM and the deglaciation are poorly understood."

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

"Particularly" is not needed and will be deleted.

On line 40: Statement that majority of sea surface temperature records from the subarctic 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 reference as it's unpublished.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.

We agree that the statement appears not well defended as the reference list is incom-

plete. Therefore, we decided to include more references, also from the NE Pacific (Barron et al., 2003; Praetorius and Mix, 2014; Praetorius et al., 2015). Together with the references listed, the current state of the art will be well represented and the idea that the atmospheric teleconnections with the N-Atlantic were important for deglacial climate change in the N-Pacific realm, will be much better defended. The "NW-Pacific" in line 41 will be replaced by "North Pacific". The Meyer et al., submitted is now published (Meyer et al., 2016).

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?

See comment on line 40. We will include references for the SST development in the NE Pacific.

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

Will be done.

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

You are right, "Jet" does not need to be capitalized. This will be changed in both lines.

Line 82: Add comma after 'ranges'.

Will be done.

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

That's true. We will write: "Mean temperatures averaged for the entire 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.

This information will be included into the paragraph: "Age control is based on accelerator mass spectrometry (AMS) radiocarbon dating of planktic foraminifera (Neogloboquadrina pachyderma sin; 9 dates in total) as well as on correlations of high-resolution spectrophotometric (color b*) and X-ray fluorescence (XRF) data of different sediment cores from the NW Pacific, the Bering Sea and the Sea of Okhotsk (Max et al., 2012). The correlation allowed to transfer AMS results from core to core, which provided 10 more age control points for site 12KL (Max et al., 2012). Based on the age model by Max et al. (2012) Holocene, deglacial and glacial sedimentation rates are 39, 79 and 59 cm/ka, respectively, allowing to investigate climate change on multicentennial to millennial timescales (Max et al., 2012). The core has a length of 11.78 m representing the past 20 ka (Dullo et al., 2009; Max et al., 2012). It was sampled in 10 cm steps providing an average resolution of approximately 200 years between samples (Meyer et al., 2016).

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

Will be deleted

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

We will add the following sentence to line 144: "The higher BIT index values the larger the relative contributions from terrestrial soil or fresh-water sources."

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

In section 3 "GDGT" is used for the total GDGT distribution comprising isoprenoid and branched GDGT. "brGDGT" is used when only brGDGT are intended to be addressed. If specific brGDGTs are mentioned (e.g. GDGT III) the "br" is not indicated in order to keep consistency with the common nomenclature in the literature. In order to clarify that the total GDGT pool is addressed, we may implement a sentence saying that isoprenoid and branched GDGT were isolated from the sediments and then abbreviate with GDGTtotal or GDGTiso+br.

In section 4. "GDGT" is indeed exchangeable with "brGDGT". Initially, we abbreviated "branched GDGT" by "GDGT". Later this was specified by "brGDGT" and all "GDGT"s were supposed to be replaced. It seems that some of them managed to hide from the change. We will correct this in all instances throughout the manuscript. Thanks for pointing out.

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

Will be done.

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.

We agree that there is reason to let Figure 3 precede Figure 2, because in sections 4 and 5 fractional abundances are discussed before the temperature evolution. However, in section 4 and 5 the concentrations of ΣbrGDGT and the BIT-index are discussed prior to the fractional abundances (and the concentrations and BIT-index are shown in Figure 2). This order is required by the logic of section 4.1 where the low BIT index is the reason for a detailed discussion about the sources of brGDGT in core 12KL. The

fractional abundances are used to evaluate the sources. In order to keep a consistent organization throughout sections 4 and 5 we decided to introduce the results for concentrations (given in Figure 2) prior to the results for fractional abundances (Figure 3). This requires Figure 2 to precede Figure 3.

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.

That is good point, thanks. We will broaden the window of the late Holocene to 1-3 ka BP. Temperature is on average 7.3 $\pm$ 5.3°C during this interval, the same as between 18 and 20 ka BP.

Also, here and every future instance, why is approximately abbreviated?

Will be written out everywhere.

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.

See comment on line 178

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

This will be done with all our MATifs values. As for literature data it will be added if the uncertainty estimates are given by the source of the data.

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.

We will rewrite the section: “With the beginning of the deglaciation at 18 ka, temperature drops by about 1.5 $\pm$ 5°C. It remains colder than during the LGM until 14.6 ka BP where the temperature abruptly jumps back to the LGM-level.”

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.

We suggest to implement a discussion of age-model uncertainties into section 3.1 where the achievement of age control is described. “Max et al. (2012) converted radiocarbon ages into calibrated calendar ages using the calibration software Calib Rev 6.0 (Stuiver and Reimer, 1993) with the Intcal09 atmospheric calibration curve (Reimer et al., 2009). A constant reservoir age of 900 years was assumed for the entire time-interval covered by the core (Max et al., 2012). The uncertainty of AMS dating was smaller than $\pm$ 100 years (Max et al., 2012). Another important issue are changes in reservoir ages of the surface ocean, in particular during the last deglaciation (Sarnthein et. al., 2015). However, recent studies suggest that reservoir ages of the Bering Sea and the N-Pacific varied by less than 200 years during the last deglaciation (Lund et al., 2011; Kühn et al., 2014) and are within the range of reservoir ages originally assumed by Max et al. (2012).”

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 peak temperature is observed.

The sentences in lines 186-189 will be replaced by: “After the abrupt temperature increase into the Preboreal temperature increases progressively rises culminating into a Mid-Holocene Thermal Maximum between 8.0 and approximately 4.0 ka BP (average temperature 7.7 $\pm$ 5.2°C).”

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.

As the term Holocene thermal maximum already implies that temperature is lower in the periods preceding and following the HTM, the last sentence will be deleted.

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.

Actually, the last sentence is trivial since it is well known that the Holocene climate variability is generally rather constant compared to the one of the deglaciation. Furthermore, the main findings of this paper are that LGM summers were similarly warm as during the Holocene and that the Greenland like millennial-scale oscillations characterize the deglaciation. So, the sentence in line 189 is not important for the paper and we will delete it.

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.

See comment on “3.4. Temperature determination”

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: : :’

We will implement the sentence you suggest.

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’.

That reads better, thanks.

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.

Indeed, the sentence will be completed by: . . .Ti/Ca indicates relatively high contributions of terrigenous material relative to marine components at 15.5 and 12 ka BP and relatively low terrigenous contributions at 14 and 11 ka BP.

For chronology see comment on line 181.

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

Thanks, will be changed.

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

We are referring the definition of Mix et al. (2001) according to which the LGM lasted from 18-24 ka BP.

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: : :'

That is a good point, thank you. We will rearrange the sentence as you suggest.

Line 257: What does 'BLB' stand for?

BLB stands for "Bering Land Bridge". This is defined in the introduction (line 29). We will repeat the definition to make the text easier to understand.

Line 260: No need to hyphenate 'insect-data'. Also suggest rewording to 'Markovo,

and ElGygytgyn and Jack London lakes'

Will be changed.

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?

The alkenone temperature record from site 12KL is excluded from the LGM discussion since it does not reach beyond 16 ka BP (see Max et al., 2012). For the discussion on millennial scale oscillations we did not show the UK'37 as it is in line with the TEXL86 from core 12KL (Meyer et al., 2016). Since the general trend is the same in both records it appears more reasonable to represent the SST evolution of the NW Pacific by the TEX which spans the entire LGM-Holocene transition. Differences between UK'37 and TEXL86 are discussed in Meyer et al., 2016 (in the paper referenced as "Meyer et al., submitted") and are attributed to different blooming seasons of coccolithophores and archaea.

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

You are right. Thanks. This will be changed.

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.

Since the Meyer et al (submitted) is now accepted (Meyer et al., 2016), nothing will be

changed.

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 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.

We will "delete" clear and "strong" and "undoubtedly" As for age uncertainties, see comment on line 181.

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'.

We will delete the sentences in lines 409-413 as the presence of an 8.2 event on Kamchatka is debatable on the basis of the existing records and it is not apparent in MATifs.

6. Summary and Conclusions Line 415-419: This introduction to the conclusions reads awkwardly.

We will delete lines 417-419 and also the listing (i; ii) in the following.

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

Will be replaced by "may".

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.

"Obvious" will be replaced by "seem to be linked".

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.

In order to increase the contrast between land and ocean we will add a grey shading to the Holocene land masses.

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.

See comment on section 5.2.2 Controls on MATifs

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.

As discussed above (see comment on the result section), we would rather leave the order as is.

We will increase the density of the age scale in Figure 3.

References:

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