

Interactive comment on “Spatial distribution of pingos in Northern Asia” by G. Grosse and B. M. Jones

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GENERAL COMMENTS:

This is an impressive, well written paper that undertakes analysis of the factors determining pingo development in Northern Asia. The methods are sound, and possible errors and uncertainties are carefully outlined and assessed. The novel approach of the paper is that it applies GIS techniques to evaluate the factors that determine pingo development. This has enabled a more quantitative regional analysis than has hitherto been possible. In many regards it is perhaps surprising that this has not been accomplished previously. The authors should be congratulated for undertaking the significant amount of work that has clearly gone into developing the database that underpins the paper.

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My only real misgiving about the paper is that the conclusions are not overly substantive. In the whole, the authors simply confirm previous findings by other authors (e.g. “In general, our findings are in good agreement with previous research on environmental conditions required for pingo presence.” Conclusions: Page 1798 (lines 11-12)). I do therefore have concerns that the paper does not tell us a great deal about pingos that is new or novel. Nevertheless, the paper does provide a very useful regional overview of pingos in Northern Asia. It is also the first time that many of the relationships between pingos and the factors that influence their distribution have been quantified in a systematic way over a large spatial area and I believe that this alone probably warrants publication.

The paper should be condensed by shortening the introduction, removing some figures and removing unnecessary references (see specific comments below).

The paper assumes that all landforms recorded in the database are pingos. At no point in the paper do the authors discuss the possibility that any of the landforms could be non-pingo frost mounds (e.g. lithalsas). As the majority of the landforms are <8 m high, it is possible that at least some of these landforms could be lithalsas (or some form of mineral-cored palsa) rather than pingos. I acknowledge that without ground-truthing this is very difficult to establish, but do suggest that it probably warrants a short discussion.

Ideally I would like to have seen more Russian pingo literature included in the references, but I do understand that accessing this literature can be challenging.

I would like to applaud the authors for their statement that they intend to make their database available (via the web) at some point in the future. I believe that making it available would strengthen the impact of this paper (ideally I think it would have been good to have published them simultaneously).

SPECIFIC COMMENTS:

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1 Introduction –

The introduction is very long and includes far too many references. I suggest that the authors look at condensing the introduction accordingly. In particular, I felt that the text between page 1784 (line 7) and page 1786 (line 7) could be condensed significantly without affecting the overall paper. This would also allow a significant reduction in the number of references that the paper cites (currently ~90). The authors should retain the final paragraph of the introduction, however, as it introduces the paper nicely.

I think that the introduction (and perhaps the paper in general) might benefit from the inclusion of a (slightly) more detailed description of the internal structure of pingos. I do appreciate that there are limited examples of exposure of pingo internal structures. However, internal structure (e.g. type of ground-ice, thickness of overburden) is important, as it will strongly influence the response of pingos to climate forcing via changes in active layer thickness.

Page 1784 (lines 26-28). If retained, three references appear to be spelt incorrectly or differ from spelling in reference list (Mueller 1959, Lagerbaeck and Kodhe 1985 / Lamborinchen 2000). Advise that authors check these references.

Page 1785 (lines 5-7). If retained, the sentence “Some studies describe. . . .conditions and processes” requires an example reference.

2 Study Area -

Page 1786 (lines 25-26) the reporting of latitude and longitude should be standardised (i.e. minutes as well as degrees should be reported).

4 Results –

Page 1792 (line 2-13) – See comments referring to figures 6-11 below.

Page 1792 (line 2-13) – Authors should standardise the format of latitude and longitude throughout the paper. They are reported here as decimal degrees, but earlier in the

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paper (page 1786) as degrees/decimal minutes.

Page 1793 (lines 12-13) – “Only 236 pingos are higher than 8 m (8% of pingos with height data”. This is an important statement, as most descriptions of pingos concentrate on the very large-scale landforms (those over 30 m).

Page 1793 (lines 19-23) – I suspect that if large pingos, with elevations greater than 50 m, were observed then they would have been mapped when the original ‘base’ maps were produced.

Page 1794 (lines 21-23) (Figure 16) – I suggest that this statement be made less uncertain. I would place more emphasis on the fact that the area shown in figure 16 with a MAGT below -11 is negligible. Perhaps reword to: “There is a sharp decline in pingo numbers for MAGT above -3 °C and below -11 °C, most likely due to the limited area in the study region with ground temperatures below -11 °C (Figure 16).

Page 1796 (line 17) – Authors should quantify the “..small number of pingos;”. Was it <10%, <5% or <1%?

Page 1796 (section 4.6: Distribution in relation to lakes and streams) - I do wonder if the authors could make more of their dataset. For section 4.6 I wonder if there are more sophisticated, yet accessible ‘off the shelf’ methods, for analysing, for example, the proximity of landforms to lakes and streams (e.g. ARCGIS ‘proximity analysis’) ? Related to this, I was wondering if there was a statistical way of ranking the importance of the factors that influence pingo development? For future publications it might be worth investigating the use of Principal Components Analysis (PCA). I have seen this applied to ecological datasets (e.g. which environmental factors determine diatom populations in lake systems). It might be totally inappropriate for application to the current topic, but I would suggest at least investigating whether it might be of any use.

Pages 1797-1798 (section 4.7): I really like the idea that pingos could be used as remote indicators of the future state of Arctic permafrost, but I do wonder whether the

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signal would be as clear as the authors suggest. The depth that ground-ice is found at in pingos means that they are not necessarily sensitive indicators of active layer change, due to insulation by overburden. As a result, changes in near surface ground and air temperatures may not lead to rapid pingo degradation, so pingos may not be as useful as future permafrost indicators in the way that other ground-ice mounds (palsas or lithalsas) might. This section could benefit from a more in-depth discussion of the internal structure and ground-ice composition of pingos derived from literature sources, to back up the authors' argument.

5 Discussion –

Page 1799 (lines 2-15) – I am a little unclear how the authors can use their data to “assess the temporal dynamics of thermokarst lakes and basins over longer Holocene time-scales”. Dating pingos is fraught with difficulties and I can only see that this goal could be achieved by accessing the internal structure of numerous pingos and recovering samples for radiocarbon or luminescence dating.

I suspect that stating “..a detailed pingo dataset could help to refine long-term carbon budgets in permafrost regions” might be overstating the usefulness of the dataset a little. The authors should remove this statement or back it up.

Page 1799 (line 10) ‘absolutely’ is spelt incorrectly.

Page 1799 (line 23) should read “. . .large regions with pingos present. . .” rather than “presence of pingos”

Page 1800 – The authors have provided an excellent section that realistically discusses the limitations of the database.

Page 1802 (line 2) – should read “. . .can safely assume. . .”

Page 1802 (lines 14-18) – authors should be specific about the number of examples where multiple pingos were combined into a single pingo location.

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Figures –

Are figures 6-11 really necessary? They are not discussed in the text in any great detail (only on page 1792 lines 2-13, and a brief mention on page 1797, line 3). As the paper concentrates on the macro- rather than meso-scale I do wonder whether these figures can be removed entirely. A compromise might be to only have one or two sub-region ‘case studies’ and analyse them in depth (i.e. pick the ones that show something novel, unusual (e.g. the “dense pingo occurrence of the Central Yakutian region”), or contributes to the findings of the paper considerably. Another possibility would be to include figures 6-11 as supplementary info if this is possible?

If the authors do choose to retain figures 6-11 then I suggest that they show the boundaries of the subregions on one of the regional figures (e.g. figure 2), so that readers can easily identify where they are located.

If 6-11 are retained, then the ‘Lakes’ label in the legend should be renamed ‘Lakes and rivers’. It might also help figures 6-11 if the geology layer were transparently draped over the topography.

Figure 12 – It is interesting that there are two very distinct populations in the bar chart associated with figure 12. This may warrant more discussion in the text of the paper?

Figure 20 – In the hard-copy I made of the paper, the hatched area of high pingo density was very hard to make out in this figure. Suggest that the authors might want to edit this figure to address this problem.

Interactive comment on The Cryosphere Discuss., 4, 1781, 2010.

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