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Interactive comment on "Changes in the timing and duration of the near-surface soil freeze/thaw status from 1956 to 2006 across China" *by* T. Zhang et al.

Anonymous Referee #1

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This paper provides an interesting overview of station-based soil freeze/thaw processes in China over recent 51 years. As such, it provides useful information, especially because this information is difficulty to obtain observationally. Overall it is a good paper, and below I provide some suggestions which may improve and strengthen the paper.

It isn't stated anywhere what type of data actually form the basis for this entire analysis. The paper only says "station data" of "near-surface freeze/thaw," and also refers to "ground-surface temperature" (GST). The "CMA, 2007" data citation is not in the reference list. If they are GST, measured in the uppermost layer (centimeters) of the ground, what types of sensors did they come from? What is the quality of these data? Were

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there sensor changes over 1956-2006? Were the data homogenized to remove certain (potential) artifacts such as station moves, location biases, etc.? Please comment on these issues.

The Data and Methods section describes SRTM data, but it then isn't mentioned what this is used for. I'm sure the metadata for the station observations include elevation–so I am unclear where SRTM elevations are employed.

This section 2 also mentions calculation of regression "trends" for latitude and altitude. Given that latitude and altitude probably don't change over time, how are you calculating trends for these variables?

Any mentions in the paper of "insignificant" trends and changes should be removed; a non-significant trend means there is no trend at all (it cannot be distinguished from "0"); this also includes most of the panels in figure 6. Similarly, it is also not necessary to state the p-values for all of the findings. Usually a significance threshold (like 95%) is chosen a priori, and then the results are reported as either significant, or not. But the magnitude of the p-value itself is not useful.

How were the various "breakpoints" in the time series determined? For example, in addition to providing trends pre- and post-1970 in the text, figures 2, 3, 4, and 5 all show separate trend lines from "the early 1990s" (it looks like 1992, but this is not actually stated anywhere in the paper) onwards–how was this break-point chosen? The lead author has a previous publication where an objective change-point analysis was applied to determine breakpoints (Frauenfeld, Zhang, & McCreight, 2007, IJoC). Could this be employed here? In many cases (figures 2-5), it looks like 1995 might also (if not more so) be an appropriate break point, so using an objective method may be advisable.

A more fundamental question pertains to which aspect of "climate change" the authors are attributing the observed freeze/thaw changes. The rapid and incredible urban expansion of Chinese cities is well-known. To what degree does, e.g., urbanization (and other land cover changes) factor into the findings? These surface changes are, of course, part of "climate change," so it would be useful if the authors could more precisely attribute the freeze/thaw status changes to certain aspects of climate change. E.g., you could categorize stations as rural, urban, or having transitioned from rural to urban, and then check to see if this accounts for some of the changes. Alternatively, you could explicitly state that it is not possible to distinguish between, e.g., greenhouse-gas warming and land use change, and that both effects are thought to contribute.

What is the explanation for some of the interdecadal variations, e.g., in regard to the "major increase in FD...after the 1970s?" In other recent work (Frauenfeld and Zhang, 2011, ERL), you suggested a strong role of the NAO in affecting soil freezing (or lack thereof) in Russia over this exact same time period–is there a similar explanation here? This is one important aspect that is currently missing from the paper: attribution of the soil freeze/thaw changes to 'something' beyond air temperature. Land surface and soil properties, vegetation, latent/sensible heat sources, snow cover (in the cold season), etc. could all be playing a role in GST variability, yet only air temperature is used. It seems a little simplistic to essentially conclude that when it is cold, the ground freezes, and when it is warm, the ground thaws.

A couple of minor, final points: why are the results of this study compared to Kansas, USA (are there some expected similarities)? Also, please carefully check the paper for grammatical errors. There may be some PDF conversion issue, but in many places, two words are merged together (e.g., "datewas" in line 9 of the abstract, also "utilizedto" on p. 3788 line 14, "atleast" p. 3790 line 25, p. 3792 lines 20-21, and many others). P. 3792 line7 contains a mistake ("the stations 140 stations"), "observede" on p. 3794 line 15, "effectively" on p. 3796 line 13 (should be effective), and line 26 on p. 3796 is missing a word after "land-atmosphere."

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