Review Klinge et al.

This manuscript by Klinge et al. appears to be quite poorly written, and the research clearly needs significant improvements to qualify publication at Biogeosciences. There is a clear lack of novelty – all hypotheses tested in this study appeared to be well-established science. The authors did not explain what additional insights that answering this set of hypotheses/research objectives that we can gain. Reading the abstract, I was not convinced that this study adds new knowledge to the science community. Moreover, throughout the text, the authors made many statements without proper citations to support their statement. Significant improvements are needed in the Introduction section to link background more clearly with the (hopefully revised) hypotheses. Section 2 and 3 appears to be in good shape, but strong justifications are needed for some assumptions that the authors make. Among those, one important assumption is the role of biophysical factors in affecting vegetation dynamics between the 1980s and current period. The authors essentially assumed these factors are static, which in reality, they are not. Moreover, it appeared to me that all hypotheses proposed in Introduction are contingent to the assumptions they made in these two sections. In a way, they've proved their hypotheses. Further, more citations are needed to support some statements that the authors made, which I tried to indicate in my specific comments. Furthermore, some contents in these two sections should be revised/deleted as there appeared to be un-necessary for the main text of a manuscript (e.g. Figure 2). Reading up to the end of the Method section, it appeared to me that this is not written as a proper scientific paper – it appears to be more likely a report in many aspects. Therefore, I suggest significant improvement is needed to warrant its publication. Below I outline some specific comments.

Specific comments:

L24: what do you mean by landscape unit? I assume you meant vegetation type, but it wasn't introduced beforehand. Moreover, why different vegetation type and site (which possibly include site-specific soil, climate and disturbance history) showed minor difference in vegetation biomass? Is it a technical issue preventing you from showing a difference? Or is it something embedded in vegetation structure/landscape heterogeneity?

L24 – 25: Some explanatory texts to illustrate the why's would be helpful – currently this is a plain description of the result. Later in your method you indicated that "NDVI should be applicable in the study area" (L83 – 84).

L26: A quantitative definition of forest edge is needed here. All the subsequent estimate of forest biomass really depends on your definition of forest edge, especially given that "interior" and "edge" biomass are different.

L29: Is the range for the forest biomass a minimum and maximum or a confidence range? Please define it.

L33-36: Can you rank the relative importance of these factors based on more advanced statistical analysis (e.g. AIC)?

L37-40: Unclear how you did the modelling – but I take this as just a statistical interpolation without any mechanistic processes involved. Essentially you assumed factors affecting vegetation distribution are static in historic and current time period. That's a huge assumption, and tonnes of literature are out there dis-proving this assumption. Up to this point (which reaches the finishing line of the Abstract), I am afraid I do not see any new insights that this study reveals. In fact, the results described here is merely a technical report rather than scientific discovery. Yet, the title is misleading – it implies a major scientific discovery.

L41: Surely there are a lot more to say (e.g. implications/limitations) than this one sentence!

L72 – 73: Is there a citation to support your statement?

L84: That's quite a weak statement. What's the range in crown closure? What is the evidence for applicability of NDVI to Mongolian boreal forests? The next sentence was a simple description of who did what – there is no evidence in your description.

L88: But relating NDVI with climate only gave you inference of observational relationships. There is really no process-based extrapolatable power in these relationships.

L103 – 109: These hypotheses are not really well-linked to the background appeared before. Plus, what novelty do you have in these hypotheses? Aren't these obvious already? At least I wasn't convinced that there are novel insights to be revealed by the current set of hypotheses.

L144-145: This description on vegetation pattern already proved some of your hypotheses, no?

Figure 2 really shouldn't be a main text figure.

Section 3.1: This dataset is valuable.

L176: Why using the mean of the two methods? You must demonstrate the performance of these two methods – citing a paper without explaining the appropriateness of these methods to your data is not the way to convince your readers. Also, it remains unclear how belowground biomass was estimated. More details are needed.

L192-193: Here it seems that you proved your 1st and 3rd hypotheses too. Why making the assumption that fire was the only factor affecting forest cover in this period? In L152-155, you've indicated that there are logging activities in the region. More importantly, you are essentially assuming static vegetation distribution in these two distant periods. Clearly there are so many factors affecting vegetation biomass and dynamics over this period (regrowth, climate, CO_2)!

Figure 3: really poor quality figure. Font size is small and some texts are blurry.

L209-212: You already knew forest coverage in 1986 is higher than current period – hence the potential forest area must be larger than existing forest area. That's your hypothesis 4 proved, is it not?

L212 – 213: I really am having trouble with relating forest area in 1986 with your predictive variables. You can't assume forest in 1986 was un-disturbed. Fire is part of nature. Additionally, all your ground-based measurements were performed in ~2018. You are assuming vegetation remained unchanged, whereas there are so many factors that already led to changes. Just to name one, CO_2 concentration in the air – the CO_2 fertilization effect.

L214 – 216: What about CO_2? There are more advanced modellings (e.g. Maxent) available out there in the literature than this simple approach. Strong justification for the current method is needed. Also, downscaling climate data from coarse resolution to fine resolution means that you have so many small grids with essentially the same climate data. Is this the reason why you didn't see climate effect on vegetation biomass?

L262: up to this point, I don't see any quantitative definition of forest edge/interior. Given that the comparison of edge and interior was a major result in the Abstract, and given that this comparison really depends on the definition of edge and interior, I think there is really no need to read further beyond this point. There are many technical flaws in the method sections, and I don't see any novelty in the current way the author describe their key results (in the Abstract) and hypotheses. Hence, I suggest significant revision to the manuscript before it can be considered for publication at Biogeosciences. I really expect a much better quality manuscript than its current form.