Review comments – “Snow and albedo climate change impacts across the United States Northern Great Plains by Fassnacht et al.

This paper analyzes trends in physical climate variables as well as estimated (derived) albedo for 20 stations across the Northern Great Plains with (nearly) serially complete records for the 60-year period 1951 through 2010. The most robust trends were increases in daily minimum temperature and days with precipitation. Other variables, including albedo, had less consistent trends.

While the paper does not represent breakthrough science, it does make a modest contribution to the regional understanding of climate change in the cold regions of the U.S., and eventually should be publishable. However, I think the authors need to be sent back to do more analysis, and improve the presentation. With respect to the latter, I find the figures very hard to follow. While their attempt to include multiple variables and the spatial location of stations on single plots is clever, I also find it nearly impossible to digest – information overload. The way many authors have presented this kind of information that works well is with bubble diagrams, which provides a much better sense of the combination of spatial structure, trend direction, and magnitude. It does require separate plots for each variable. My suggestion is to replace Figures 1-3 with such plots (multi-panel of course). There are various ways of doing this; one is to use the size of the bubble to reflect the strength of the trend, with color (typically red and blue) indicating the trend magnitude, and solid vs open circles for statistically significant vs not.

My major technical concern is that the paper doesn’t investigate the cause for spatial anomalies. The Sterling vs. Kimball comparison is interesting, but the authors don’t offer any explanation as to why the trends are so different. My suspicion is that changes in station location, conditions, and/or instrumentation may have played a role. Presumably the 20 stations are in the NCDC Cooperative Observer network. There is a metadata archive for these stations, which the authors should review carefully. It is not necessarily the case that stations have been in the same location even if the station number hasn’t changed. I have seen presentations by Kelly Redmond that have highlighted horrors in these station records where the station has moved, but the same station ID was retained (his examples are in the West, where station moves often mean changes in elevation, and hence spurious temperature trends – that won’t be so much the case in the Great Plains, but other local factors may well be responsible for some of the apparent trends). In the case of precipitation, and the snow part in particular, minor changes in station location can easily change wind patterns, and hence snow undercatch, and even if the station location is unchanged, construction of buildings, growing or removal of trees, and so on can have a major effect. Also, there is the issue of time of observation. Nothing is said in the paper about observation time, which has changed in many cases and can introduce spurious trends. NCDC has a time of observation file for all of these stations (the authors may want to talk to Pasha Groisman who is an expert on these matters).

Conspicuously missing from the methods section is any discussion of how the observations were taken. In part, this should include a summary of gauge type and time of observation (and any changes therein), but also how solid precipitation was recorded. Many (perhaps all) of these stations are manually observed once per day, and with snow in particular, how is this collected? Is the presence of snow via manual observation of snow depth, which then is annotated and the

total (liquid) precipitation total ascribed to snow rather than rain over the previous 24 hours? And whatever the protocol is, has it remained the same over the 60-year period? Again, I think a discussion with Pasha Groisman would be worthwhile. Incidentally, I am surprised that I don't see any reference to his work on similar topics.

I also have to wonder why the authors didn't use stations in the Hydroclimatic Network (HCN), for which some quality control has been done. Some of these stations no doubt are in HCN, but others may well not be, and if so why not? Were they not included (in HCN) because of quality control issues.

Finally, although not conclusive, changes in snow albedo, if present, would be important. The authors note the limitations of the USACE decay algorithm, and I understand that there is no viable alternative over such a long period. Given that the computed albedos are a function of several measured variables, I think the authors should analyze trends in the contributing variables, and then show which trends are most responsible for the observed trends (this is different from the partitioning of variance, which they do discuss). It may also be that in the case of stations with no significant trend, this is resulting from cancellation of trends in the driving variables, and this should be noted as well.