Specific comments

RE: temperature dependence of GEM production/emission:

The conclusion regarding temperature dependence of GEM production/emission is one of the major conclusions of the manuscript; however, I still do not believe compelling evidence has been provided to support this as a major conclusion of the work. It appears that the idea of higher temperatures resulting in higher GEM fluxes has been reached following visual observation of the data, but there is a great deal of scatter in the data, and this makes the conclusion potentially uncertain, and definitely less straightforward that has been presented in the manuscript. If this conclusion is to be included, it should certainly be qualified, and the shortcomings/uncertainty identified, as well as the process which lead to the identification of this "relationship".

The discussion of results is much more clear, in general; however, on pg. 8 line 15 - 16, the authors include the statement that: "*Low temperatures are required for the occurrence of AMDE (<-4 C)...*", and this condition represents the entirety of their data set. As a result, I do not believe this is a particularly compelling argument for the temperature relationship proposed in the manuscript.

In addition, the highest fluxes of GEM are certainly observed when temperatures are > -20 °C; however, this data is quite scattered, with many instances of 0 ng/m^2/h GEM fluxes being observed at T > -20 C. This would, then, not imply that the relationship between GEM flux and temperature is as simple as higher temperatures resulting in greater GEM production and emission, as may be implied from the conclusions as presented (ie/ pg 11, line 4 - 5 "Furthermore, the data indicate that that heating of the snow surface influences formation of GEM and reemission of GEM"). As a result, I would suggest further "softening" of this conclusion, and acknowledgement of the lack of a straighforward or definite relationship between these factors.

The statement about these GEM flux vs. temperature results that is presented in the abstract (pg 1, line 16 - 18: "*The measurements also indicate GEM emission is increasing with increasing temperature...*") is misleading, as it implies some manner of mathematical relationship could be derived, while the presented results simply show some instances of higher GEM fluxes when temperatures were above the ~ -20 °C threshold, coincident with many zero GEM flux measurements under those same temperature conditions. This statement should be revised alongside the conclusions.

Finally, when comparing the CO_2 fluxes vs. temperature in Fig. 9c, with the GEM fluxes vs. temperature in Fig. 9b, the authors state that there is no temperature dependence on CO_2 fluxes (pg. 8 line 12 – 14); however, simple observation of these two figures does not present a compelling case for the statement of a relationship in Fig. 9b vs. no relationship in 9c. If the authors are making the argument for relationships based strictly on visual observation, it appears that CO_2 fluxes may decrease with increasing temperature, where the highest depositional fluxes

of CO_2 appear to occur at T > -20 °C. If a more rigorous approach was taken to determine the presence of a relationship between temperature and GEM flux (vs. no relationship between CO_2 flux and temperature), this should be presented in the manuscript; however, if simple visual observation was employed, as appears to be the case with the manuscript in its current state, then the conclusions regarding the occurrence of a relationship in the GEM vs. temperature data (Fig. 9b) and no relationship in the CO_2 vs. temperature data (Fig. 9c) may be the result of observer bias, and should be reanalysed and/or not included as a major conclusion of the work.