Based on the one-year long of field measurement of inorganic ions and ammonia, this paper mainly reports the seasonal variations in concentrations and compositions of fine particulate inorganic ions, and the aerosol acidity in Wuhan, China. The aerosol acidity, i.e., pH of the $PM_{2.5}$, is the subject of this work, but I found many statements of this paper on the aerosol pH are not correct and main conclusions given by the authors do not stand up. Moreover, this field observation work lacks of QA/QC, making the data unreasonable. For an online semi-continuous measurement using a technique like MARGA in this work, a simultaneous collection of filter samples should be done to compare and correct the online results, which is an indispensible of QA/QC, because the online data are of significant uncertainty, as reported by many researchers. However, in the current work there is no any comparison by using filter collection or other measurement, and the data quality is poor (see my detailed comments below). Concentrations of inorganic ions are the main and the key data of this work, which are the basis of the conclusions given by the author, but I found the inorganic ion concentrations are too high and in many cases they exceeded the $PM_{2.5}$ mass, which are unreasonable (see the details below). Some explanations in this paper are subjective or self-contradictory (as exemplified below). In addition, there are many English grammar errors in this manuscript. Therefore, I do not think this paper could be accepted in the current format. Following is my detailed comments, which should be addressed when revising the manuscript.

1. Line 33-35, why aerosol pH showed a different trend at the lower AWC from that at the AWC higher than 15 ug/m^3?

2. Line 183-193, and Table1, inorganic ions account annually for 70+/-19% of $PM_{2.5}$, I think this ratio is too high, and unrealistic, because $PM_{2.5}$ not only consist of inorganic ions, but also consist of organic matter, black carbon and crustal materials. Probably the $PM_{2.5}$ mass was underestimated. Did author collect filter samples to validate the online MARGA data? In this study there was no work on QA/QC.

3. Line 196-198, these statements are not correct. Higher temperature in summer favors more ammonium nitrate aerosol decompose and evaporate into the gas phase as NH_3 and HNO_3, because NH_4NO_3 is volatile, thus resulting in the lower concentration of NH_4NO_3 aerosols. This is the reason why the calculated NOR is lower in summer. However, this does not mean that nitrate and ammonium formation rates are lower in summer.
4. Line 200-207, summer is rainy season for Wuhan, thus the lower SNA is also due to the stronger precipitation.
5. Line 214-217, the strong correlation between SO_4^{2-} and NH_4^+ does not necessarily means that$(NH_4)_2SO_4$ is the form of ammonium, in fact, many studies have pointed out that $(NH_4)HSO_4$ is the major chemical form rather than $(NH_4)_2SO_4$. Moreover, this statement is inconsistent with the main conclusion of this work. Because as reported by Liu et al. (Liu et al., 2017; Son et al., 2018). The pH of $(NH_4)_2\ SO_4$ aerosol is more than 4, while the pH of $PM_{2.5}$ in Wuhan estimated by the current work is very acidic, less than 3.7, thus NH_4HSO_4 seems more consistent with the aerosol pH values given by the authors.

6. Line 246-248, as I mentioned above, under such a lower pH=2.84 conditions, ammonium in $PM_{2.5}$ cannot be exist as $(NH_4)_2SO_4$.
7. Line 299-302, this statement is not correct. In fact, AWC is not only dependent on RH but also dependent on the mass of hygroscopic matter. Inorganic ions such as ammonium sulfate and ammonium nitrate are much less in summer, thus AWC are the lowest in July, August and September.

References:

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