

Interactive comment on "Characterization of ions at Alpine waterfalls" by

P. Kolarž et al.

General recommendation

The authors investigated ion concentrations close to Alpine waterfalls. Evaporating water will produce negative ions in the air and as a consequence leave positive charges behind in the water that hasn't yet evaporated. Usually these positive ions deposit on the ground. Therefore, large negative ion concentrations are seen at the waterfalls and during the rainfall. What makes this study to stand out is that several waterfalls have been studied and the instruments cover a large size range. Based on the manuscript the main motivation and aim of this study is not clear.

I recommend publication in ACP. Nevertheless, there are some issues to be resolved, mainly related to some corrections and providing more details, which help understanding the methods and the results.

1 General comment

1.1. What is the main motivation and aim of this study? Where would you apply the results from this study? Add this information to the introduction and conclusions.

1.2. Ion mobility versus size: Why did you choose to use Tammet's mass diameter instead of Millikan-Stokes mobility diameter as you mentioned in the text? Nowadays, in air ion researcher community Millikan mobility diameter is commonly used (see e.g. Mäkelä et al. JCP 1996, Hirsikko et al. ACP 2011). This can cause some difference in results when comparing results with for example Laakso et al. 2007.

You mention that you converted ion mobility to radius (!) in the manuscript (p. 25299, line 16). Unify terminology. Make sure that all sizes that you report are actually the same size, diameter to be exact (Tammet diameter?), for all instruments (CDI and SMPS).

1.3. Background in measured signal due to high relative humidity: Typically, the electrical mobility spectrometers are very sensitive to high relative humidity which causes high noise levels to cover the real measured signal. Explain in more details why this not a problem here.

p. 25302, lines 4-7: How did you solve the problem with WF's droplets (water spray) soaking the electrodes of the CDI, which caused current leakages in the electrode system and amplifying electronics?

Especially, concentration calibration with very high concentration (up to 50000 ions cm⁻³) should have been included into this study. This is also related to specific comment 2.3.

1.3 Basic characterization of all 5 measurement sites and WF's is missing: Chapter 3.1, especially p. 25302, lines 24-25: It would be crucial to add a summary table with all the measurement

sites listed including the control sites. Where, when (how many days total of good data) and what (quantities) was measured on each site? What kind of WF is at the site (height of cascade etc.)? Add this information to the manuscript.

In the abstract you state that "During a 3-year field campaign..." but there is no time series presented in the paper. This is very confusing.

1.4 Seasonality in ion concentrations: How would you assume that the seasonality affects your results? You were not able to measure at all sites at the same time. What kind of seasonal behaviour the WF's have? How would this affect the ion concentrations? This is why you actually would have needed to measure the long time series from one selected site.

2 Specific comments

2.1 p. 25298, lines 23-24: You have to let reader know where you found the numbers you report in the introduction. Add citations. E.g. Hirsikko et al. paper in ACP 2011 and reference therein might be very useful to you.

2.2 p. 25299, lines 14-15: What does the Aplin, 2008 citation really mean? In atmosphere the charged particles are not single charged. Especially the ones you measure with the Grimm SMPS (>20 nm). Is this citation related to mobility-to-diameter or voltage-to-mobility conversion?

What kind of inversion algorithm did you use for the Grimm SMPS data when measuring naturally charged ions? I assume that you took only the diffusional losses into account in the inversion – not e.g. charging efficiency. Add this information to the manuscript.

2.3 p. 25031, lines 8-11: How was the CDI's with different cut-off sizes intercompared or calibrated? Please if there exist some study refer to it. Now the sentence that states: "It is expected that simultaneous measurements of all three instruments on places where no WF-related ions are present should show approximate air ion concentrations." This doesn't sound convincing.

Please, move the CDI1-2 intercomparison results from p. 25304, lines 25-> into chapter 2.1.

2.4 p. 25309, line 8: Did you actually measure 0.2 nm ions? I suppose this is in Tammet diameters? Those are really small ions. How did you measure those? You haven't mentioned ions diameter of 0.2 nm earlier in the paper.

3 Technical corrections

Chapter 2.1: Please add manufacturer and instrument model information always when introducing instruments (e.g. for Grimm SMPS, CPC, DMA (length), and anemometer).

Chapter 2.1: What is the time resolution of the instruments?

p. 25301, line 13: What does RPM stand for?

p. 25303, line 1: Specify. What is 'maximal ionization'?

p. 25305, line 4-5: What does 'testing' include?

p. 25318, Fig. 6: Revise the caption. I suppose you mean <2 nm ion concentrations? Three-year summer average? See p. 25305, lines 6-8.

p. 25306, line 4: What does ELPI stand for? Refer to some paper that has technical description of the ELPI.

p. 25312, Table 1.: What is 'closest range'?