

Overall quality

I consider the overall quality of the manuscript as very good considering a suite of instruments deployed for the study and the use of appropriate data analysis tools. The authors have also carefully considered the limitations and uncertainty some specific part of the results in this work.

However, the authors need to ensure that sentences are concise and avoid unnecessary repetition and confusion in the statement formation.

General comments

This research investigates the sources of ultrafine particles (UFPs) in rural Payerne, Switzerland. It finds that secondary processes significantly elevate UFP concentrations, similar to urban levels. Primary particles mainly come from traffic and wood burning, while secondary particles arise from new particle formation (NPF) events, driven by sulfuric acid and stabilizing bases like DMA and ammonia. The study highlights the need for further measurements of precursor vapours to fully understand these processes. Considering the dearth of studies of UFP in rural locations, this study adds to the research enhancement in the field of particle formation.

I recommend the publishing of this work after the author addresses the specific comments and also clarifies the readability of the text. This MS needs English language revision, many sentences are poorly formed and fail to convey the correct meaning.

Specific comments

Line 20: non-volatile particles fraction should be written as non-volatile fraction of particles, to be corrected else where in the MS.

Line 23: Expand the acronym NPF. the acronyms need to be defined at their first use.

Line 24: What do you mean by cluster ions and nucleation mode. The size range needs to be defined in the abstract itself.

Line 30 “transport related” should be replaced by “traffic related”. Transport is a verb – a process of moving goods/people from one place to another.

Line 49-51: abundant references are available. The authors need to cite some of those.

Line 58: The authors talk about that different sources can contribute to different size classes of UFPs. Have the authors defined what are the important size classes of UFPs relevant here?

Line 89 Figure S1: Not clear at all. the military airport is not clearly visible, Image resolution needs to be improved. Also I think the authors meant fig S1(A)?

Line 113-114 : Can the authors give a range of overestimation in MLH expected. Using the word “slight overestimation” does not sound

Line 129: The word “particle” can be omitted from number size distribution, as here the authors are talking about the ions.

Line 131. What do the authors mean by “multiply charged particles”? is it a typo error?

Line 149: SMPS 3938 size range in the table S1 is mentioned as 6-110nm, here it is 3-110 nm.

Line 187 -188: regional events are also well defined in Dal Maso et al. The authors mentioned they followed Dal Maso classification and later modified to include regional & transported events as per Dada et al.. what was the modification used in the classification of regional events here and why?

The later lines in the same paragraph explain this better. However, that means Dal Maso et al classification was not modified as mentioned in the Lines 187-188 but Dada et al classification was adapted to fit the classification at the study site (as per the figure S3). The authors are suggested to modify the text to bring more clarity in the classification explanation as this an important section in the MS.

Section 3.4 : Coagulation sink was calculated using the combined size distribution method. The authors should include the main equation under this section or atleast in the supplementary information.

Line 200: Why was 50% appearance time method used “positive ions” only?

Line 281: “Traffic emissions are a major source of NO₂ indicated by the sharp increase of NO₂ during the morning and evening rush hour (Fig. 1, Fig. S7), although other sources such as residential wood burning and use of fertilizers in agriculture could affect the concentration”. The authors attribute the morning and evening peaks mainly to the traffic rush hours. First for a rural place would the evening rush hours be 18-20 hrs? do we have data for traffic rush hours? Have the authors checked the BC concentration during these hours? do they match with traffic hours? Could there be other nighttime chemistry playing a role for NO₂ here? Significant amount of NO₂ can reacts to form NO₃ and N₂O₅ during the course of a night, but their fate is an important determining factor to the overall fate of NO_x (=NO and NO₂)

Line 284 : Figure S8 B just shows the NO₂ concentration on weekdays between 8-17 hrs, which does not justify the high concentration during 18-20 hrs.

Line 285-286: Mixing layer height does not clearly explain the seasonal differences in NO₂ concentrations. The MLH is highest in spring, yet NO₂ concentration is higher as compared to summer season, so the authors need to explain other processes associated with seasonal variability of NO₂.

Line 308 “regional nature of SO₂ emissions”: Could the authors be more explicit in explaining this ? Since the airport emissions are not playing a significant role, then which sources are impacting the SO₂ emissions is missing from the discussion.

Line 363: Does the catalytic stripper separates the semi volatiles or non-volatiles? How efficient is this separation? is there any error estimates using this technique, since the results of secondary particles may itself be at high uncertainty as the authors have already mentioned. therefore, the result uncertainty needs to be calculated atleast based on the instrument/technique used.

Line 442: it points towards the higher significance of secondary particle contribution towards total number of particles. Therefore the fig S15, y axis should have secondary fraction. I suggest to show secondary fraction on the y-axis.

Line 459 "Volatility and size suggest a secondary processing" - A vague sentence. The authors can make it more clear and concise.

Line 518-519: In the previous lines the authors stated that airport is not making a big difference in the anthropogenic contribution at the study site. Now in these lines, the authors mention airport as one of the anthropogenic sources for higher GR in the larger sizes. it is a contradicting statement! authors need to clarify.

Line 490: when the wind is coming from the airport direction then also the BC concentration is not elevated! What could be the reason for this? BC concentration should normally go high when the wind is from a source of BC. Mixing of airmass before arrival at the site? This needs to be explained here.

Line 541 -542: An interesting observation! Can the authors comment what could be the reasons for a similar median ammonia concentration during NPF and non NPF days? why ammonia is not playing a limiting role despite the importance of a base, which could play an important role in NPF (stabilizing SA clusters) as per many previous NPF studies! Is it because of low ammonia threshold?

Line 557: "causal relationship"? please refine the sentence for better clarity.

Line 563-564: If the precursor vapours required for growth are lost (for e.g organics) how does it favour NPF? The manuscript does treat NPF as clustering + growth.

Line 604: Are the authors sure that the NPF events are supported by ammonia based on the fig 6D where ammonia conc. is same during the event and non-event day.

Line 647-648: "secondary particles dominate the overall particle size distribution, with a larger fraction at higher particle concentrations" could be streamlined for clarity.

Line 666: The conclusion section of the MS should also mention about the role of meteorology (wind direction) in driving NPF especially considering the location of the site surrounded by grasslands and agriculture lands.

Technical comments

Table S1, two times "nm" is mentioned in the SMPS row, delete one.

Line 86-88: sometimes "Km" is used for distance, sometimes its expanded form "kilometers", same for meters. Please follow consistency as per the journal guidelines.