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Comment on acp-2021-93

Anonymous Referee #2

Referee comment on "Measurement report: Effect of wind shear on PM₁₀ concentration vertical structure in the urban boundary layer in a complex terrain" by Piotr Sekuła et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-93-RC2>, 2021

Here the authors present data collected from surface stations and balloon measurements in and around the city of Krakow, Poland. Through comparison of PM₁₀ measurement patterns, along with model output, the authors attempt to explain observed differences in observed PM₁₀ vertical profiles through variability in atmospheric dynamics, including wind shear strength. The use of commercial balloon measurements in this way seems to be an interesting and useful source of atmospheric data, and I think it deserves recognition and dissemination. It is also clear that the authors have compiled a great deal of observational data and model output in their exploration of pollution in this region, and it is likely to be useful for many locations with similar meteorological and topographical features. That said, I have a number of concerns regarding the quality and types of analyses being performed that I would like to see addressed before publication.

General Comments

Overall Organization - The total quantity of information, visualizations, and explanation is quite high relative to the scope of data presented. However, in its current form it seems inefficient and scattered to my eye. A great deal of written space is dedicated to relatively straightforward topics (e.g. regional topography, various model configurations), while other analytical choices of great importance (e.g. vertical profile categorization and statistical analysis of potential influences) are only described briefly in passing. I would expect to see significant streamlining and focusing of the primary narrative in this paper before I would consider it ready for publication.

Clarity of Text and Visualizations - Figure quality varies greatly, and some in particular do not seem to directly support the claims made about them. Phrasing and word choice is sometimes awkward and difficult to parse. As one example, "wind direction change" is sometimes used in a way that makes it hard to tell if the authors are referring to a change over time, or a change across the vertical profile (wind shear). More attention to clarity and precision would be appreciated throughout.

Analysis Choices - Several key steps taken in the analysis of observations seem to lack reproducibility and objectivity, raising concerns in my mind regarding the robustness and validity of the conclusions drawn from them. In particular, the selection and sorting of vertical profiles, as well as the subsequent evaluation of associated meteorological conditions deserves more description and potentially some rethinking in approach. Specifically:

- The authors state that they use a "subjective method of fitting the linear curve to each vertical profile" based on "R squared coefficient, the angle of the straight line and residual values classification". They further clarify that objective classification methodologies could not be used "due to differences in flight heights and the PM10 measurement altitudes". With all of the classification approaches available, I find this assertion difficult to accept, especially if the classification is being performed on quantitative metrics like the ones listed. Without a clearer, more objective approach I find it hard to consider this procedure sufficiently robust and reproducible.
- Following this subjective sorting, some comparison of local conditions for each profile is performed and described. However, the figures and metrics presented to support the final conclusions are not sufficiently clear and convincing, to my eye. Figure 6, in particular, would seem in the text to be a key figure in supporting final conclusions, but after several readings I still cannot figure out what it is supposed to be showing. Figure 7 highlights a particular day chosen as an example, but the information is scattered, unclear, and leaves many questions unanswered regarding the representativeness of the chosen day and the reasons for not somehow showing the characteristics of ALL observed days, rather than one or two. Throughout the final pages of text, I kept looking for the conclusive evidence that succinctly and unambiguously showed the connection (with uncertainties and other statistical metrics) between the patterns being studied, but I was not able to find this. If such evidence is in there, it should be made more clear. If not, it must be added.
- Finally, I would like to suggest that the order of operations being performed here (first manually sort vertical profiles, then try to analyze atmospheric conditions) is making the authors' (and reader's) task more difficult than it ought to be. If the main thesis is that strong wind shear drives key differences in surface and vertical profile PM10 patterns, why not first sort days based on this unambiguous metric (strength of wind shear) and then compare PM10 levels, boundary layer depth, vertical profiles, or any other dependent property based on that? Either way, the connection between pollution results and their potential drivers needs to be explored in a clearer, more defensible manner.

Minor Comments

- References: Several references are in a notation unfamiliar to me, for example (Air... 2020). Is this a placeholder? I think these should be standardized and revised, unless I am missing something.
- Figure 1: The maps are useful, but their distance from corresponding tables makes it hard to interpret their labels. This may be better resolved at a later stage of typesetting, but some effort to line these up in some way would be helpful.
- Flight frequency details: What were the patterns of balloon flights over the course of each day? How much of the vertical profile patterns observed can be attributed to regular diurnal cycles? If the "frequency of flights depended on meteorological conditions" is it possible that some sampling bias has been introduced based on these limitations, and if so how is it being addressed?

- Figures 5 and 7: These panel plots appear to contain a great deal of information, but they are overpacked and underexplained. It's unclear in their current form what they are trying to show, on the whole, and some presentation details significantly impede clarity. For example, the humidity color bars run from 0 to 100%, but the data appear to be uniformly in one small sliver of that space, making the entire plot uniformly green. Both of these figures need rethinking.
- Figure 6: As mentioned, this figure is very confusing and it's unclear what the takeaway message is supposed to be. Furthermore, panel (b) appears to never be described in the caption.