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Transport of Po Valley aerosol pollution to the northwestern Alps.
Part 2: Long-term impact on air quality

Introduction

The manuscript presents a comprehensive analysis of meteorological (standard weather parameters, ceilometer) and air quality (PM10 characterization) measurements over a three-year period in the Aosta Valley in north-western Italy. The study convincingly demonstrates that a non-negligible fraction of the particulate matter concentration observed at receptor sites in the valley is advected from the neighboring Po plain, occasionally providing a decisive contribution to the exceedance of concentration limit values. The study also shows that advected aerosols differ from the locally emitted ones in their physical-chemical properties. Results from a chemical transport modelling chain are evaluated, demonstrating that the effects of advection from the Po plain can be represented only qualitatively from state-of-the art operational model chains.

Recommendation

Methodology and results are scientifically sound. The manuscript is written in good English and figures are generally of adequate quality. Suggestions for a few minor modifications should be taken into account before publication can be recommended. Details are provided below. Please note that the assessment is based mostly on the meteorological aspects of the study, reflecting the expertise of the reviewer.

Comments

1. The superficial reader might conclude that pollution in the Aosta valley mostly depends on advection from the Po plain. While this is true to some extent, one should also consider that easterly advection is most common in summer (Figure 3), that is, when PM10 concentrations in Aosta are at their yearly minimum (Figure 8). Based on these contrasting statements, another superficial reader (with an opposite bias) would conclude that easterly advection actually cleans the air. The latter is obviously a flawed argument, still it shows that it is important to carefully delimit the message of the study.
   A balanced perspective on the whole issue could be the following: easterly advection from the Po valley, which is most frequent in summer but possible in all seasons, may combine with adverse weather conditions (low-level inversions) locally worsened by the valley topography, greatly enhancing pollution levels. This is especially true in the coldest part of the year, when emissions are highest.
   I feel that, in the introductory and concluding remarks, the authors could do a better job in explaining these subtleties.
2. The abstract is very long. ACP doesn't seem to set length limits, but it is in the interest of the authors to provide a more concise formulation. Please refrain from using references to published literature in the abstract.
3. Page 1, lines 17-18. "maximises" → "is highest", "minimizes" → "is lowest".
4. Page 2, line 27. There is a double blank space before "WMO".
5. Page 3, Figure 1. Please align image borders (merely for aesthetic reasons).
6. Page 3, line 15. Remove "the main of which are".
7. Page 4, line 31. Thermally-driven winds include a night-time component as well (drainage flows along slopes and downvalley winds), which can ventilate the urban atmosphere and reduce pollutant loads. Are nocturnal breezes not observed in Aosta?

8. Page 4, line 32. Strictly speaking, foehn is not necessarily a rain-shadow wind. Even if the (quite inaccurate) textbook picture of Foehn emphasizes the impact of upstream latent heat release due to condensation, Foehn is in most cases connected only to low-level blocked flow upstream of the orographic obstacle. Foehn may develop even if there is no upstream precipitation. Warming mostly occurs through the adiabatic descent of unblocked air from levels above the mountain tops (see for instance this DOI: 10.1127/0941-2948/2012/0398).
9. Page 7, table 1. Footnotes b and c. Unclear: is this the average data availabilty, or are data regularly available with this periodicity?
10. Page 8, line 7. I am not sure that COSMO-I2 is a "high-resolution" model. Its grid spacing is certainly in line with that of other state-of-the-art operational limited-area-models. However, it is barely sufficient to appropriately resolve thermally-driven circulations. See for instance this DOI: 10.1175/MWR-D-14-00002.1. Please consider also comment 21 below.
11. Page 9, lines 26-30. How sensitive are the TSM model results to the ad-hoc method of weighting the concentration fields on the number of trajectories through each cell? How si the weighting performed, exactly? Does it only reduce noise in the spatial fields, as stated, or does it also affect their magnitude?
12. Page 10, line 20. "...was chosen based on the Q/Qexp ratio". Please clarify.
13. Page 13, table 2. It seems to me that the criteria outlined here are not mutually exclusive. For instance, would a 13-hour period with wind speed exceeding 4 m/s be classified as "downwind" or as "foehn"? I also have a few concerns about the terminology.
    (A) "Upwind" and "downwind" typically identify position relative to the wind and to a specific location (e.g. "when the flow is southerly, the city is downwind of the mountains"). In this context, it might be more appropriate to speak of "Channeled synoptic flow from the west/east". In addition, it would be appropriate to specify wind direction ranges for the two classes.
    (B) "Breezes" have by definition wind speed > 1.5 m/s (Beaufort scale). This would exclude wind calms at night . It might be better to speak of "Diurnal wind system".
    (C) "Stability" refers to the thermal structure of the atmosphere. Because winds are emphasized here, it might be appropriate to speak of "Wind calm".
    (D) The directional range for the Foehn class probably deserves being explained.
    (E) Please use wind "direction" instead of "provenance". "Direction" refers conventionally to the direction the wind comes from.
14. Page 14, figure 4. In summer, about 70% of the days feature breeze systems, but aerosol-layer days are only about 50% (Figure 3). What about the rest? Are easterly breezes occasionally "clean"?
15. Page 15, line 14. It would be useful to explain where the Divedro Valley is. The great majority of readers wouldn't have a clue otherwise.
16. Page 16, line 15. Please remove the comma between "simple" and "forecasting".
17. Page 19, line 4. Reference to Teixeira et al 2016. The interested reader wouldn't find much in this editorial. More appropriate references could be those available at these DOIs: 10.3389/feart.2015.00077 and https://doi.org/10.3390/atmos9030102.
18. Page 25, line 5. The dashed continuous line in Figure 12a is very hard to see. Consider replacing with a colored line, or with a filled area in the background.
19. Page 25, Figure 13. Text is hard to read on the colored background. Consider using a different color map, or adding white background to the location labels.
20. Pages 30-32. There is some redundancy in Figures 16-17-18-19. It is unclear why results from two locations are represented in the figure. In fact, the differences between the two sites are never discussed in detail in the text. It would be possible to remove the figure panels referring to Donnas without great loss of information.

21. Page 33, lines 2-3. “This is a clear indication that the external contribution (boundary conditions) is not optimally parametrised in the model”. I am really not sure that I agree on this statement. If I understand correctly, FARM uses information from the whole COSMO-I2 domain, which completely includes the Po valley. Therefore, the problem does not lie in “external contributions” not being represented in the lateral boundary conditions. There might be problems with the lower boundary conditions (emission inventories), but I doubt these can be wrong by a factor of 4 over the whole Po valley. I am not even sure that model parameterisations matter here.
    I would argue that the relatively coarse resolution of the weather model (2.8 km, marginally enough to resolve the valley) and of the transport model (only 16 vertical levels) play an important role. Please consider the topography cross-sections in Figure 20. Aosta seems to lie in a basin, likely non-existent in reality and introduced in the model by terrain smoothing. If the topography profile descended continuously from Aosta to Donnas and the Po Plain (as in reality), it would be possible to resolve the horizontal advection of aerosol-laden air from much lower altitudes on the plain, likely removing a large fraction of the negative concentration bias.
22. Page 33, line 10. “The reason for discrepancies is therefore likely in the emissions”. Why not in deficiencies of the transport modelling, as I argued above?
23. Page 46, line 18: There seems to be a problem with the reference to Thunis et al, 2012 (the third author).