The authors mainly dealt with the effects of the lockdown measures due to COVID-19 pandemic on gaseous pollutants and fine aerosol particles in North China Plain. The topic is timely and of interest for the research community, and fits into the scope of the journal. The authors also investigated the changes in major inorganic chemical composition and some metal constituents of particles which has been rarely done so far. The MS indicates valuable results and conclusions which seem worth publishing on one hand. On the other hand, it is difficult to assess their real value since some important information, firm explanations and background discussions are largely missing. They should definitely be complemented. There are also several smaller discrepancies or other issues listed below which are to be improved or corrected for. The present reviewer can arrive at the final suggestion only after all these are clarified and added in a careful and substantial manner.

Major comments

1. One of the key methods applied to deconvolute the effects of meteorology on the atmospheric concentrations is the random forest approach. The method and its conditions of validity are not described and virtually not discussed (Sect. 2.2). Several important questions can be formulated in the reader with regard to this. For instance, is the investigated time interval sufficient for training the learning method and for its testing as well, in particular when the data sets were shared in a respective ratio of 90%/10% between these phases (lines 86 and 122). Are there any constrains of the method as far as the number of available data and retained variables are concerned? What are the uncertainties or limitations of the modelled results? Are there possibilities to verify or validate the modelled outcomes and were they performed?

2. If I understand it correctly, the deweathered results can still contain the effects of changes from other natural or anthropogenic sources of the investigated chemical species or some substantial dependency of the variables on larger (e.g. seasonal or annual) time scales. Cf. also line 116. How was the possible interannual variability considered? The authors are requested to explained and discussed these items. The related interpretations of the atmospheric concentration changes and their quantifications are not convincing.

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3. A large part of the results concerns secondary aerosol components. Their formation can depend sensitively on the solar radiation, which is missing from the set of meteorological variables. The uncertainties related to this missing factor should be discussed separately. Similarly, O_3 as a main representative of secondary pollutants is missing from the list, although it would be very exciting to see its modelled changes. At many urban or polluted locations, O_3 concentrations increased or stayed constant during the restriction intervals. The authors may want to explain why O_3 is not among the investigated gases, and to discuss the enhancement from the aspects of their results. (It is noted that the MS could gain from citing some earlier references in the field and published in this journal.)

4. Many parts of the MS are too descriptive/fuzzy or too long (e.g. Sect. 1, 3 pages) and more importantly, real interpretations and discussion are often missing. This should be improved, e.g., metal components and secondary inorganic aerosol constituents are to be discussed from the point of view of their regional sources/source sectors.

5. Atmospheric concentrations can usually be described by log-normal distribution and, therefore, median descriptive statistics is preferred to mean (which is called average in the text). Furthermore, the authors should rethink their rounding off strategies all over the MS; e.g. the value of 63.5 (line 157) implies a relative uncertainty <1%, which is unusual in analytical chemistry or should be explained separately; or similarly, a value of 93.5% is questionable (line 163). Are the differences between some corresponding data pairs of the results, e.g. 0.33 and 0.28 (line 285) significant?

Minor comments are listed as examples

6. Unusual citing practice in line 33.
7. Which transport sector is specifically meant?
8. Abbreviation $PM_{2.5}$ expresses the particles in the specified size fraction, while it is used in the sense of $PM_{2.5}$ mass in many places of the text (e.g. lines 44, 62, 101).
9. Check the order of the words in lines 52–53.

10. Unusual expression: neutralized the decreases (line 60).

11. The distribution of semi-volatile components between gaseous and condensed phases is completely missing from the explanations in line 68.

12. Change "knowledge was critical… in the future" to "knowledge is critical… in the future" in line 75.

13. Change "might take more sense" to "more sensible" in line 84.

14. What is meant by: standard sample. Is it a sample collected for off-line analyses, and if yes, what were its collection conditions? What does the expression "agreed well" specifically mean? All in lines 111–114.

15. Meteorological parameters were not resolved at their first occurrence (in lines 120–121).

16. Check the order of words in line 191.

17. Check Figures S1–S3 in lines 185, 188–189, etc. and similar typos.

18. Captions of the figures are not self-explanatory or descriptive enough.

19. The reference list is often deficient in required details.

20. The language and grammar should be improved.