Referee comment on

"*Variations in the chemical composition of the submicron aerosol and in the sources of the organic fraction at a regional background site of the Po Valley (Italy)*"

by M. Bressi et al.

This manuscript reports results obtained with an Aerodyne Aerosol Chemical Speciation Monitor (ACSM) during a long-term measurement period (1 year) in the Po Valley (Italy). The authors investigated the chemical composition of non-refractory submicron PM (NR-PM$_1$) with a time-resolution of 30 minutes and identified of the main components of the organic fraction. In addition, the parallel multiple off-line analyses were carried out to assess the performance of the ACSM in the determination of PM chemical species regulated by Air Quality Directives. This work is meaningful, and the workload is very large. The observed results are representative because they have been measured for almost one year. It is completely within the scope of Atmospheric Chemistry and Physics. However, the results presented here are a little superficial, and a more deeply analyses on these observation results is necessary. Therefore, I recommend its publication after a major revision.

Specific Comments

Abstract

Although the abstract has summarized the main contents of the text, it is still lack of some points to attract readers. I think the authors have done a lot of works, but they do not have an in-depth analysis on their results. For example, the author did not find the BBOA in summer. While, they only showed the average results in this part and main text. Then some interesting information was averaged and masked. Therefore, I believe other more meaningful results could be found after a more in-depth analysis.

Introduction

Please add a simple introduction on the previous ACSM and AMS research results in this area. Then discuss the innovation of your research is more appropriate.

About the article structure.

Section 3 (results) contains : (1) discussion on consistency of ACSM measurements; (2) discussion on organic apportionment quality control. Meanwhile, some content looks like a introduction of analysis method. I think these contents are more like a discussion part not results. On the contrary, the author showed a lot of experimental results in section 4 (discussion). Therefore, I suggest that the author reconsider the article structure carefully and some discussion content could be put in the supplementary material.

About the language: Some sentences are not easy to understand. While high quality language expression is very important for a high quality article. Therefore, please improve

the language after this modification.

At the beginning of section 4.3, the variations of NR-PM$_1$ chemical composition and OA factors' contributions as a function of total NR-PM$_1$ mass are examined. However, these analysis is not deep enough. According to the results mentioned above, the contribution of every NR-PM$_1$ and OA species to the total is different in four seasons. It means that the pollution characteristics in four seasons may be different. Therefore, I strongly recommend the author to discuss the variation of every species with the change of total NR-PM$_1$ mass for different seasons. Then some common features for four seasons and some unique characteristics for one season could be found. These results are more interesting than that provided now.
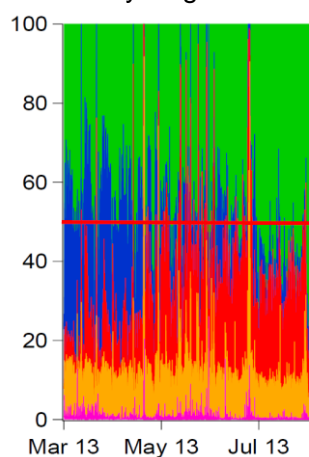
Figure 3:
(1) "Left, bottom, top right, bottom right" is not clear enough. Figure 3a, b, c, d is better. I suggest the author do the modification in this and other figures.
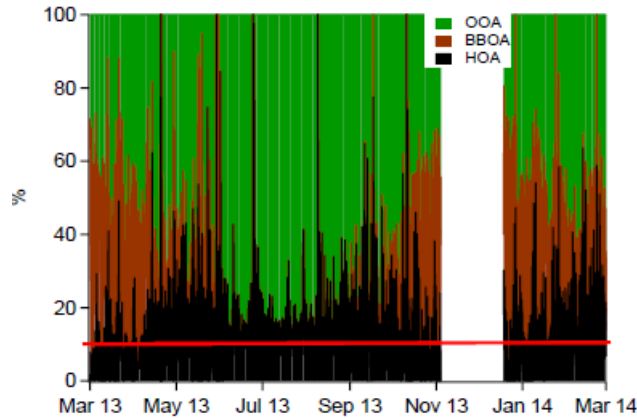(2) This figure looks not every clear.
For example:
The following figure is a part of the relative chemical composition of all NR-PM1 species. I found that the contribution of organic in spring (March-April-May) is lower than 50% (red line) in most of time, and frequently lower than 40%. However, the average contribution reached to 52% in the main text. It looks very strange.
In addition, according to the relative chemical composition figure, it seems that the contribution of organic was 0 some time. However, the mass concentration of organic looks always higher than 0.

The same phenomenon also appeared in Figure 5. Please see the following figure:

It can be found that the contribution of HOA is almost always higher than 10% (red line), even frequently higher than 30%. However, the average contribution was only 11%.

Other comments:

Abstract.

Line 31-33. A detail comparison with other study results is not necessary in abstract. I don't recommend to cite the detailed data results.

Introduction.

Line 50 Please provide the annual $PM_{2.5}$ mean concentration value. That could make this sentence more convincing.

Line 69 "2008/50/EC" is a reference or other standard ? It is not easy to understand.

Section 2.1 I suggest the author add a sampling site map in main text or supplement material.

Line 205 Please check the format.

Line 219 What is the reason for the poor correlation and results difference of chloride ?

Section 3.2.3 Although the author explained the reasons for HOA was not well correlated with BC, CO and NOx, I still feel confused. What are the main sources of BC, CO, and NOx in the study site ? It looks BBOA has a very big impact on them. In addition, chloride is also an important tracer for BBOA, did you have an analysis of their relationship ?

Line 376-378 Pleases add the values of the highest NR-PM_1 levels reported at rural and urban ....

Line 385-386 Please add "(Fig. 3)" to the back of this sentence.

Line 393 Did you measured the BLH in different seasons ? A comparison on measurement results is more useful.

Figure 1 and 2. These figures are very difficult to see clearly due to the too small words. It is necessary to enlarge the font size in the two figures.

Figure 6. It is not very clear and the font is too small. I suggest that all the figures are divided into two lines. Then the figures will not too narrow.