

We would like to thank the referee for pointing out a number of weaknesses in our manuscript, as summarized in “General comments” by the referee. After addressing the issues raised by the reviewer, we think that the paper will improve considerably.

We will address the critics i) by stating more explicitly the scientific goals of the paper, ii) by bringing up more clearly the new scientific findings resulting from this work, iii) by extending our analysis of new particle formation and growth to the resulting cloud condensation nuclei production, and iv) by enhancing the comparison of our results to other studies made in China and elsewhere. In practice, we will rewrite most of the introduction section and add a new subsection “3.2.2 New particle formation and growth and subsequent CCN production” after the subsection 3.2.1. Our responses to the detailed comments are added below after each comment.

General comments

The paper presents 4.5 months of measurements from a site out of the city of Nanjing. This period is not very long and not very representative.

The paper does not contain real scientific conclusions. It is another analysis of new particle formation events, giving all the relevant parameters but why? What is this important for? If the authors do not state this clearly and understandable I cannot recommend this paper for publication. Taking a data set and processing this with a standard program is not sufficient. Thus, at first a clear motivation is needed. Secondly, the analysis needs to be extended: e.g., taking a longer data set (at least one year), estimating the effect on e.g. CCN-concentration, optically relevant parameters or other climatically relevant parameters.

Another option might be to state what it really new in this paper. To my opinion all the methods and programs have been applied to various data sets before and this is just one more set of number which is not automatically new science.

Thus, I recommend not publishing the paper in its current status, a publication requires major revisions and a second step of review to ensure a sufficient scientific quality of the journal ACP.

Comments in detail

Introduction:

This introduction is very general. The authors mention the direct and indirect effect of aerosols but they do not show the relation of these effect to the presented study, which focuses on new particle formation (NPF). NPF creates small particles, but under certain conditions they grow to larger sizes and might be relevant for the optical processes as well as the formation of clouds. This connection is not mentioned here. However, in China the conditions for particle growth are frequently in a way that favors relatively

rapid growth. This was at least shown in one case study (Wiedensohler et al., 2009), but there are probably more papers showing this connection.

Page 22339, Line 17ff:‘

However, so far only few comprehensive studies have been published (e.g. Shen et al., 2011). Instead, many projects have had rather campaign character, presenting only a few weeks of observations...’

There are few studies published. Number size distribution measurements at PKU in Beijing started in 2004 and were the subject of several publications (later you cite Wu et al., 2007, but it should be mentioned here too. To my opinion, 4.5 months is also not very long and representative and close to campaign length. It covers more or less only one season and no general conclusions are possible.

The introduction does not motivate the presented paper. What is it relevant for? Health, radiation balance, visibility, cloud formation, air quality, health? There are many potential aspects, but they should be addressed here. Without a real motivation this paper should not be published.

We will rewrite most of the text in “Introduction”. More specifically, we will i) discuss in some detail the connection between atmospheric new particle formation, resulting cloud condensation nuclei (CCN) production, and climate, ii) state more explicitly the scientific goals of our paper, and iii) bring up how our investigation add to previous studies on this subject made in China. With these changes, we think that the motivation of this paper will become much clearer than it currently is.

3. Results and discussion

The general description of meteorological conditions and aerosol characteristics is appropriate und well written. One can easily follow and it is very compact. Many of the features have been also observed at other sites in connection to NPF

Page 22347, line 6

The measurements were taken more or less during the winter months only, therefore one cannot say if there is a winter break or not. Furthermore, I recommend here to compare more with other studies from China. In the Beijing region the winter is cold and dry but cleaner as the summer, thus NPF is there more frequent in winter time or the events show completely different characteristics between winter and summer. But speculation about differences throughout the year is not possible with a 4.5 month-data set only.

We will make a new subsection “3.2.2 New particle formation and growth and subsequent CCN production” and add there a more detailed comparison of our results with other studies. We agree that our data covers essentially one season only. We will remove the sentence on page 22347 (lines 6-7) and other speculation that might be interpreted as an attempt to say something about seasonal cycles of new particle formation at our measurement site.

Page 22347, line 8 ff.

I do not understand the difference between Figure 5a and 5b. I think they both display more or less the origin of incoming air during nucleation events?! Or what means a retroplume exactly? In Fig 5a, SE-directions are completely missing why in Fig 5b the nucleation probability is between 20 and 40% for this direction. Thus, there was nucleation observed? Please explain this!

Fig. 5a shows the retroplume, i.e. a model description of the origin of the air masses (at 100m above ground); and Fig. 5b shows the actual wind directions at the site (at ground level). Fig. 5a shows where the air masses were 24h before the event, while Fig. 5b shows wind directions during the event. Both figures describe the incoming air mass, but they are not describing exactly the same thing. 5a is a regional view of the movement of air masses and 5b shows how this translates into observations on site.

Our revision will highlight the difference between the figures more clearly and point out the different information the figures contain.

4. Summary and conclusions

Page 22356, line 23

..NPF was observed on 26 days...20%...

What frequencies were found at other sites, such as Beijing, North China Plain, Shanghai for the winter months? Is this comparable? Are there any measurements for other months of the year available?

As stated earlier, we will add a new subsection (3.2.2) with more detailed comparison of our results with other studies. The main conclusions from this comparison will be added to “4. Summary and conclusions”.

Again, the data are not representative and more data should be considered. I fully agree with the authors that measurements apart from those in Europe are required and in particular China is an important location to obtain detailed measurements within the Megacities but also outside. These regions are not well characterized yet and they may also change their properties due to the rapid development of the country and the economic growth.

Thus, basically I like the type of measurements, the location and instrumentation but they should be extended and analyzed in more detail with regard to climate effects.

We thank the referee once again. We feel that our response and modifications address the main concerns of the reviewer.