**Atmospheric
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Comment

Interactive comment on "Characterization of photochemical pollution at different elevations in mountainous areas in Hong Kong" *by* H. Guo et al.

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Disclaimer: This peer review was written as an assignment for an Atmospheric Chemistry course. I've never written one before, so I would appreciate feedback on my technique. I would also like to apologise in advance for any mistakes or assumptions I've made, of which I've sure there are many.

This paper outlines measurements of O3 levels at varying altitudes, and the possible causes of the concentration differences found. It concluded that there were higher levels of O3 at higher altitudes due to a variety of causes. These include: â $\check{A}$ ć the effects of NO titration- lower [NO] at higher altitudes lowers the removal rates of O3 â $\check{A}$ ć vertical meteorological conditions- the height of the inversion layer changes the [O3] at higher altitudes â $\check{A}$ ć valley-mountain breezes transporting newly emitted pollutants

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from urban areas to higher altitudes during daylight hours âẮc regional transport- the differing wind patterns at various altitudes bringing in changing concentrations of O3 from nearby urban areas It was also found that the O3 rate was dependant on both VOC and NOx concentration levels at higher altitudes, rather than just [VOC]. Firstly I feel like the method for determining the concentrations should be moved to an appendix. While it is important to include this methodology, it has been done before and is not new science. Including it in the main article detracts from the purpose of the study, which was focussed on reasons for the variation in concentration. It seemed like the paper could effectively be read from section 3.2, which starts with a summary of the previous findings and then begins an analysis of the causes. The Abstract contains a good and succinct summary of previous research and findings. It would have been interesting to read about the author's opinions on these findings, as they are often contradictory. A clear statement of intent would have been appreciated, as the paper's goal wasn't entirely clear. They state that it was the first time pollutant concentrations at varying altitudes had been investigated in mountainous regions, but don't present forth a hypothesis on what the causes might be, which would be an improvement. However it does give examples of previous findings, and confirms that their own match these. The Methodology attempts to describe the layout of the site in several paragraphs. This is hard to visualise and the information could be better conveyed on a map in the appendices. A map of China is included, but it doesn't show any of the pertinent information. The map also didn't show height variations, which is obviously extremely relevant to this study and would have been helpful. They did provide a map of the wind movements, which will be useful for anyone looking to apply their own hypothesis to this data in the future. It was also mentioned that the distribution of urban land cover was replaced in their Weather Research and Forecasting model with the most current data, which I found impressive. In several cases the graphs would benefit from more explanation or notes as they are hard to follow. There was also some statistical analysis it would have been useful to include- for example the [O3] could be plotted against varying wind speeds on different days, to substantiate the claim that regional transport

effects the concentration. These kind of results are easily plotted from the data obtained and would increase the original science presented in the paper exponentially. The details of the measurement techniques are good, giving details of the equipment model numbers and statistical precision limits in terms of σ. However, the Methodology fell down when discussing simulations (sections 2.3 and 2.4) which weren't explained in enough detail. In particular, including more information on the Moving box scenario would be relevant. When analysing findings they compared their own suggestions with previous papers, which was good. They could have made it clearer that their results matched previous studies, as this was a success and was barely mentioned! Apart from this lack of clarity, the analysis was clear and understandable. In terms of the science, I had several concerns about the method used. The measurements were only taken over a period of 9/6/10 to 11/29/10. I worry that this isn't enough time to get a reliable average- if the O3 concentrations vary by season or if there was a particular outlier during this time period it wouldn't be picked up on. I also find it worrisome that the meteorological parameters used in the analysis- including temperature, relative humidity and wind speeds- were taken from a weather station. It would have been quite simple to take these measurements at the site itself, which would stop small fluctuations in temperature or wind going unnoticed, when they might affect the results. I would also point out that the instruments may not be at the same temperature as the environment, and this will have gone unmeasured. The VOC samples were 'collected at 2-h intervals from 7:00am to 7:00pm per day'. It should be noted that these sample times mean that there is no measurement taken at midday- when the concentration should be highest. It is also concerning that the O3 samples were taken more frequently than VOCs, and the number of samples taken at each altitude was also different- varying from 201 to 183. It seems strange to add in an unnecessary bias to the data by changing the sample number in this way, when it would have been more logical to keep all possible variables constant in the measurements. This makes statistical analysis of the data harder to process, and also means that the results will have different confidence levels – so they cannot be compared as easily. The presentation of the paper was good with

very few mistakes. The spelling and grammar were generally good- although I found wrongly worded sentence in section 2.4, and a spelling error in the Conclusion. The paper repeatedly uses the term 'O3 episode day', but this isn't defined until the results on page 5. The acronym 'LT' was also used throughout and not explained until page 6. It would benefit from another proof-read. An equation was given in section 3.2.1 which wasn't given in integer proportions- it gave molecules in ratios of 1: 0.63: 0.32: 0.23. I found this odd, especially as a rate constant was quoted. This might lead to confusion in calculations when the k value is used. The graphs were good- the sizing made it hard to see all the details but this isn't necessarily a problem if reading on a computer, when it can be zoomed. It might be pertinent to include the day and night boundary layer mixing heights on the graphs as a reference. In some places references are placed in brackets rather than the footnotes. They also didn't provide references for several models that were mentioned, although perhaps interested parties reading the paper would be familiar with all of these. The paper in general presented a few new ideas, but it was mainly a confirmation of existing findings. Section 3.3, for example, discusses the correlation between O3 and VOC/NOx* which are well known mechanisms. I would suggest that including more fresh hypotheses would make this paper a more useful addition to previous research, however it does provide important data which can be analysed further to provide solid evidence for their claims. To summarise, this is a solid beginning on which further work is needed to make it a useful addition to the field. A lot of the data is now available and can be further analysed in regards to the different factors to find correlations. Whilst the Methodology is very detailed, the Analysis needs to be taken further before the paper can be considered an original finding.

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