General comment

A valuable long term dataset is described as a basis for an interesting long term trend study on ozone and its precursors, which can be used to track how emission changes and some abatement strategies are effecting air quality in Hong Kong, and how best to deal with air quality issues into the future. However, much more detail on the scientific approaches and applied methods (measurement techniques, modelling and data analysis/filtering) are needed as detailed below. Once these issues have been adequately addressed, this study can be accepted for publication in ACP.

Specific comments

More details of the measurement techniques, in particularly the QA/QC methodologies and approaches for the GC analysis is needed (stating computer programme used for auto-calibration is not good enough). I find a detection limit of 2 pptv using this technique quite low, especially for an hourly measurement.

Different speciated sets of VOCs have been targeted in previous studies, such as 30 C2 –C10 in previous studies at MK and HT (Lyu et al., 2016). Why have you focused on a different speciation of 21 species in the present study? Given the extensive annual VOC information available at other sites (local and regional) it would be useful to compare seasonally averaged measurements at as many different sites as possible (in a Table?).

Difficult to separate the background and regional effects on observed ozone formation from the local chemical production and more detail into how the authors have done this is needed. What are the sensitivities in the model to these (potentially high) uncertainties in initial concentrations of ozone precursor?.

Set the overall air quality picture in HK in perspective - How do the observed precursor trends and ratios (e.g. NO2/NOx) compare to other high population industrial urban areas of the world on a year by year seasonal basis? There is a wide range of comparable data in the literature.

Difficult to relate specific plots with specific trends discussed in the main text.

Confusing in parts and difficult to read/follow.

Missing reactivity – This study is linked to the measurement of 21 VOC precursors, which may be the main source of ozone in the region in the inventories (show this?). However, how much primary reactivity is potentially missing (e.g. long chain/branched alkanes and alkenes?), on a year by year basis? Some discussion on the missing reactivity and how this may affect the presented results is warranted.

Technical comments

Fig 1 – “cities” of regions? A more detailed map/maps of the local and regional environs including distance to downtown HK would be useful. What are the prevailing wind directions in each season?

Page 5 (modelling) - Reference for TUV photolysis modelling needed – model sensitivity to cloud and haze days? Loss of radicals to aerosol sensitivity studies? MCM is not an “explicit” mechanism. It is described as “semi-explicit” – more complex and complete chemistry than CB05.

Why was a simple BLH/deposition sensitivity run not performed?- sensitivity?

Did the model runs include a “spin up” time to get the radical intermediates into steady state? i.e. run for 2-4 consecutive days and then taking the data for the last 24 hours? This could significantly effect the model O3 coming from the model secondary chemistry and the RIR calculations if the intermediates are not in steady state.

The reasoning behind the removal of “background” and “local” concentrations to give an “observed” value are vague and questionable. Their influence will be highly variable with respect to the meteorology and time of the year. What are these concentrations and how were they derived (by wind direction? hourly, monthly, seasonally?). Again, I would like to see the seasonal concentrations of VOC measurements at other regional and local sites compared and contrasted to the current data set in some detail and how sensitivity to this calculation effects the deconvolution of local photochemical ozone formation. The authors state that the “NO_2 emitted from the inland PRD is slightly more likely (?) to arrive at TC in winter and spring than in summer and fall, the differences in travel time among the seasons are relatively small and it is difficult to be precise with seasonal average estimates of NO_2 lifetime and travel time”. Therefore, the errors and limitations in understanding the effect of regional/background NO2 on the observed O3 are high here (which the authors acknowledge) – what are the uncertainties associated with this and what is the sensitivity to the model runs?

Eq 7 – delta kro2+no[RO2][NO]?

Fig 3. Not sure how significantly different the inter-annual trends of the TVOC in spring and autumn are?

Fig 4. Why are the locally produced simulated and filtered trends so different in most seasons?

Page 10 – “The different inter-annual trends of NO_X and TVOCs in spring/autumn from those in summer/winter were probably because marine air significantly diluted air pollution in summer while continental air masses remarkably burdened air pollution in winter, which concealed the decreased local emissions of NO_X and TVOCs in summer and winter (Wang et al., 2009)” – do you have winder sector data to show evidence for this?

Define and show NO titration reaction as a separate Equation.

Further investigation found that temperature and solar radiation in summer indeed increased in these years ($p<0.05$)” Show this data.

Page 11 – Section 3.2 repeats itself somewhat

Page 12 – the MCM is not “explicit”. Should also be ($p<0.05$)?

“locally-produced O_3 (filtered) values clearly showed similar trends to locally-produced O_3 (simulated) in spring, autumn and winter ($p=0.07$, 0.09 and 0.93, respectively)” - where is the plot showing these trends?

“Unlike in spring, though the observed and locally-produced O_3 (filtered) displayed increasing trends in summer (0.70±0.34 ppbv/yr and 0.66±0.41 ppbv/yr, respectively; $p<0.05$),” – these

trends are not the same as shown in Fig 4?

Page 13 – "the total solar radiation (0.24±0.16 $MJ \cdot m^{-2} yr^{-1}$, $p<0.01$) and temperature (0.095±0.034 $^{o}C/yr$, $p<0.05$) in summer significantly increased during the past 10 years" – odd units! Solar radiation should be given as irradiance in W m-2. Temperature in K. Be consistent as to how you write units – K/y or K y-1. Why has the solar radiation increased in the summer over 10 years? Less haze?

Fig 9. Why is the data not presented as monthly averages or even by season – which would be clearer and possibly give more information?

Page 20 (contribution of VOC groups) – I would have liked to see the year by year seasonal trends of the individual TVOC groups plotted – i.e. aromatics, alkanes, alkenes and BVOC. This would give a more comprehensive overview of the TVOC trend and how it evolved/is evolving.

Page 21. Basic detail of the HK government emissions reduction plan need to be outlined (with the reference website placed in the References section).

"photolysis rates of alkanes" – I am sure alkanes do not photolyse in the atmosphere!