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Interactive comment on "Impact of crop field burning and mountains on heavy haze in the North China Plain: A case study" *by* X. Long et al.

Anonymous Referee #2

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This paper conducts a numerical study to investigate the impact of crop field burning and topography on haze pollution in the North China Plain. This is an interesting study and potentially will be useful for air quality management in this region. However, there are several important points need to be appropriately addressed before it can be accepted for publishing in Atmospheric Chemistry and Physics.

Main comments:

1) The impacts of biomass burning and topography on air quality in the North China Plain certainly are interesting topics for policy makers. However, because modeling results are sensitive from case to case, from policy perspectives, such kind of study should be conducted for a longer period. For the crop filed burning, this paper only conducted a case study for one week in October. According to Figure 2 of this paper

and other previous studies in China, the most intensive period of biomass burning in the eastern China is June. It's a little bit strange that the authors selected a case in October. In fact, from the results presented in Figure 5, it is quite clear that in this case the crop field burning activities didn't play an important role on air quality in both NNCP and SNCP if it was compared with an overall broad peak of anthropogenic pollution. For suck kind of non-typical case, I don't know whether it is meaningful to make many statistics to compare the relative contributions in several tables (e.g. Tables 3-6). With such a short-term case, I would like suggest conducting more in-depth analysis to understand specific scientific questions other than a calculation of numbers.

2) In the second part of this paper, the authors conducted an interesting numerical experiment by removing topography in specific regions in WRF-Chem model. However, such kind of treatment may cause some inconsistency in the initial conditions of meteorological parameters and the terrain data, which need more spin-up time because WRF uses a terrain-following vertical coordinate. However, according to the modeling description of this paper, the spin-up time for the WRF-Chem simulation is only 12 hours. Since the authors aim to give a quantitative understanding of the topographic effects, a longer spin-up time, for example several days, is needed. In addition, same as the Comment #1, as a case study for several days, the quantitative results here will have large uncertainty for policy makers. I would like suggest giving a more in-depth discussion by touching some scientific questions related to mountain, such as the impact of mountain-valley breezes on the accumulation of air pollutants etc.

3) The authors didn't give appropriate literature review for the both topics of biomass burning and topographic effect. In the model description part, the authors gave too many (more than 15) unnecessary references related to some common model schemes in WRF-Chem with some of them published several decades ago. However, in the main results part (Sect. 4), only two references (Cao et al., 2008 & Huang et al., 2012) are cited in the first paragraph but there is a lack of some comparisons of the results and conclusions with previous works done by other scientists for similar topics

in the same regions or in other regions of the world.

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