Review Comment

This manuscript presents a fairly detailed modeling analysis of a haze even in North China in winter. It discusses the chemical composition, transport, and formation mechanism of the haze episode. Using the coupled meteorology-aerosol simulation with WRF-Chem, the paper also estimates the effect of aerosol feedback on meteorology. It concludes that BC contributes to 50% of the overall effect of aerosol feedback on PBL and surface PM2.5 concentrations. This paper confirms many findings from previous analysis of wintertime hazes in North China, including the importance of secondary inorganic aerosols, regional transport, and meteorological conditions. My main concerns are with the validity of using CO as a proxy of regional PM2.5 transport and with the uncertainty in BC simulation. Minor issues are with the presentation clarity and some technical details. I recommend publication after these issues are properly addressed.

Major comments:
1). In Section 4.3, the authors use CO to indicate the source regions of PM2.5 in Beijing. They did a sensitivity analysis by turning off CO emissions in Beijing and used the relative change in CO concentrations to denote the impact of surrounding regions on PM2.5 pollution in Beijing. Although they showed that CO and PM2.5 were highly correlated, CO has a much longer lifetime (~ 3 months) than PM2.5 in winter and also undergoes different loss mechanisms (e.g. CO is not water soluble or lost through deposition). As such the sensitivity simulation using CO may not be appropriate for the sensitivity of PM2.5. Why didn't the authors choose to conduct the sensitivity simulation using PM2.5 directly? They have a model at their disposal. They can turn off primary sources of PM2.5 as well as the emissions of its gaseous precursors over Beijing, the same approach as they did for the CO sensitivity simulation, to evaluate the impact of surrounding areas on Beijing.

2). A major conclusion of this paper is that BC is responsible for 50% of the aerosol feedback on meteorology which in turn influence surface concentrations of PM2.5. That conclusion is obviously dependent on the ability of the WRF-Chem model to simulate BC concentrations correctly as well as its relative contribution to the overall PM2.5 composition. Figure 7 clearly shows that the model overestimates BC concentrations at the surface, for example by about a factor of two during the severe haze days (i.e. 18-19 January). Because the model underestimates OC and sulfate at the same time, this means the model has a significant overestimation of the fractional contribution of BC in total PM2.5. These two factors in combination suggest that the simulated absorbing effect of BC on meteorology in this winter episode should be significantly overestimated because of (1) overestimate in BC absolution concentration and (2) underestimate the role of scattering aerosols. This is an important issue that needs to be acknowledged at a minimum, given the emphasis of this manuscript on the simulated role of BC. But the authors did not discuss or even mention any uncertainty of this point. This is a major shortcoming of this paper and should be addressed before acceptation by ACP.

Minor Comments

1) pg 22784, l3: add “the time period” before “from 2001 to 2001”.
2) Section 4.1: are the analysis presented in this section based on model simulations or observations? It’s not clear to me.
3) Pg 22791, l7: upwards should be northward, since the discussion is on a 2-D surface pattern.
4) Pg 22791, l8-l10: the discussion of the high pressure is confusing. First, there is no clear indication of existence of a high pressure on Figure 4. Figure 4 shows only winds, not pressure fields. Second, the authors suggested high pressure would act to disperse pollution and low pressure leads to pollution accumulation. This is contradictory to the common understanding that high pressure is not conducive for pollution dispersion because of the subsidence and stability, and low pressure (e.g. cyclones) usually acts to reduce pollution.
5) Pg 22793, l15-17: Remove the sentence “However, few modeling studies have ….”. There have been quit some modeling studies on secondary aerosols during winter haze in China.
6) Pg 22793, line 24-26: Are these factors from model or observations? If from models, how do they compare with observed factors?
7) Pg 22797, line 23: add “and” before as a result
8) Pg 22798, line 4: increase of temperature inversion should be changed to decrease of temperature gradient from surface to aloft, because Figure 10e shows only the difference in temperature between the two runs, not temperature profile.
9) Pg 22798, line 15-17: below Beijing should be changed to south of Beijing.