Atmos. Chem. Phys. Discuss., 11, C13607–C13609, 2011
www.atmos-chem-phys-discuss.net/11/C13607/2011/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The isotopic record of Northern Hemisphere atmospheric carbon monoxide since 1950, implications for the CO budget" *by* Z. Wang et al.

Anonymous Referee #1

Received and published: 23 December 2011

This interesting paper studies CO from the NEEM firn borehole. Part of the study is published elsewhere and this paper concentrates on the CO isotopic composition. The main outcome is a CO emission scenario for the period 1950-2008 that shows that fossil emissions probably have peaked in the 1970s and declined afterwards. Better car-technology and Diesel vehicles are held responsible for the CO decline. While CO2 emissions kept increasing into the 1990s and 2000s, CO emissions must have declined according to the authors. The main driver for this scenario appears to be the $\delta^{18}O$ signal, which was heavier in the old firn (and thus in the 1950-1980 atmosphere). Since fossil fuel is $\delta^{18}O$ enriched, a decrease in this source is required to explain the lighter signature in the current atmosphere. It is also inferred (Petrenko et al., 2011)

that the atmospheric CO levels maximized around 1980.

While this message seems quite straightforward, some complicated science is behind the firn-analysis. First of all, the seasonal cycle penetrates into the firn layer and the uppermost data points cannot be used for the analysis. In section 4.1 a rather complicated section is devoted to the air diffusion inverse and forward model. In its current form it does not shed much light on the problem at hand. I would suggest to shorten this section and refer to the references. For the isotopic part, I see e.g. a reference to Buizert et al. 2011. In section 4.2 it is argued that the inverse model does not work with seasonal cycles. Although not totally clear to me, figure 2 presents the effect of seasonal cycles in the firn. I suggest to add for clarity the assumed seasonality in the atmosphere as a reference. Also the caption of this figure is hard to read. What is a "constant atmospheric trend", for instance? In the end, it is not totally clear how the barely visible green stars show the isotopic values corrected from (?) the effect of seasonality. This deserves better explanation.

In section 4.3 the authors discuss the best estimated trend in figure 4. They discuss here the depth (40m), which refers, I guess to figure 5.

The language of the paper is often difficult to follow, so a thorough rewriting is required, preferably by a native speaker. An example is in paragraph 4.5, "They also undergo the longest age mixing". As a non specialist, you are totally lost!

But my major concern is with the results. The authors often claim a proper validation, while my impression is that the results are not really good. For instance, the $\delta^{13}C$ results presented in the appendix show that none of the scenarios matches the observad maximum around 2000. One could argue that the source mix might have changed, but this would involve a very unlike scenario. Also, the main result in figure 7 is not very convincing. 60 ppbv CO from fossil fuel seems way to high compared to the MOZART-4 model (only 40 ppbv). Also the employed biomass burning scenario is way higher than GFED-3, although the resulting contribution (roughly 10 ppbv) seems reasonable.

All in all, the authors did not convince me that the final scenario really is the true story. There are to many open ends (e.g. $\delta^{13}C$) and the modeling seems not very accurate.

1 Minor points

- Figure 1 does not add anything and can be removed
- Table 1 misses the unit for CO
- Page 30643: CO removal by OH has been constant over the past 50 years. This is inaccurately phrased: OH is assumed constant I guess. CO removal scales with the CO concentration, which was clearly not constant
- Section 5 is named discussion, while it contains results
- Figure 9: please indicate that the symbols refer to Pb, I guess

---

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 30627, 2011.