Atmos. Chem. Phys. Discuss., 10, C12769–C12771, 2011
www.atmos-chem-phys-discuss.net/10/C12769/2011/

Interactive comment on "Attributing and quantifying European carbon monoxide sources affecting the Eastern Mediterranean: a combined satellite, modelling, and synoptic analysis study" *by* R. Drori et al.

Anonymous Referee #2

Received and published: 25 January 2011

1 Introduction

The paper by Drori et al. studies the transport of CO to a site in the Eastern Mediterranean (EM). This is done by (coarse) tagged model simulations, trajectory analysis and comparison to observations (flasks and MOPITT). Although the study is in general well conducted, it lacks a sufficient analysis. The tagged model simulations, trajectory analysis and synoptic classes presented in the paper are interesting, but the shortcomings of these methods are not discussed. And the analysis assumes that the model

is able to reproduce the observations, which is not adequately demonstrated, as I will outline below.

2 Model validation

The paper starts by showing the results of the model (figures 2 and 3). It is noted that the model has a double dip in the seasonal cycle. One in June, and one in September October. Is this feature reproduced by the measurements? If we inspect the GMD flasks (figure 5) this conclusion is not substantiated. Yes, the model reproduces the seasonal cycle, which is driven by OH, and also seems to capture some synoptical events. But how much of the correlation is explained by the seasonal cycle? Is the model co-sampled with the flasks. Are the flasks sampled conditionally (e.g. pollution free)? This information is not present in the manuscript. The authors should at least check how well the model is able to reproduce synoptical scale features in the measurements (e.g. use de-seasonalized data). The paper is not about the seasonal cycle, but about the synoptical changes. The reader should be able to assess the quality of the model in this respect. And the conclusion states that the measurements show a double dip. This is not clear from figure 5.

Another piece of validation is the use of MOPITT data (figure 4). Again it is stated that the correlation is high, but it is my suspicion that these correlations are mostly driven by the seasonal cycle. Actually, I find the agreement rather poor. The MOPITT data clearly show positive outliers that are not reproduced by the model. Simply mentioning a correlation coefficient does not suffice here!

Figure 6 compares the MOPITT data to the GMD flasks in a monthly averaged fashion. Again it is claimed that both products agree well, based on the correlation. Yes, both products capture the seasonal cycle, but do they show a double dip, as stated in the appendix? Very strange, the correlation is quoted, but not the bias, because it is only a qualitative comparison. Why then do the authors use the correlation, which is a quantitative measure?

3 Conclusion

Since the paper hinges on the ability of the model to reproduce the synoptic scales, the authors should highlight the ability of the model to do so. They should concentrate on the synoptical scale variations, study situations in which different airmasses reach the EM, etc. Moreover, they should substantiate conclusion 3, which states that CO is enhanced in the EM in summer, both in the model and in the measurements. Also they should clarify the differences between MOPITT and the model and GMD measurements. One option would be to calculate the 900 hPa MOPITT measurements by combining the surface measurement with a modeled profile. I regret that I cannot be more positive about this paper at the moment. Later, I might upload a comment with more technical corrections.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 30893, 2010.