Atmos. Chem. Phys. Discuss., 10, C4358–C4363, 2010
www.atmos-chem-phys-discuss.net/10/C4358/2010/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Forecasts and assimilation experiments of the Antarctic Ozone Hole 2008" *by* J. Flemming et al.

Anonymous Referee #2

Received and published: 22 June 2010

General comments

This paper describes the set-up and results, with respect to stratospheric ozone, of a new pre-operational system developed at ECMWF. It evaluates the capacity of this system to assimilate and forecast the Antarctic "Ozone hole" event, using the year 2008 as a test case. As reported in the conclusion, "The focus was put on the impact of three different ways to describe stratospheric ozone chemistry and on the impact of the initialization with ozone analyses produced by assimilation of satellite data".

The 4D-VAR data assimilation method is recalled very briefly, probably because it is already well described in the litterature and was used here in a standard manner. The selection of the observational data (section 2.3) is a useful report and could be helpful for other developers of chemical data assimilation systems. The resulting ozone analy-

ses are shown to represent very well the Aura MLS dataset, which dominates the other observational information brought into the system. This result is reassuring but not original: the assimilation system is simply shown to work as well as in the operational Numerical Weather Prediction (NWP) system. The predominance of Aura-MLS over the other assimilated instruments is technically interesting but not surprising, in view of the large profile density and high vertical resolution delivered by Aura-MLS level 2 products.

With respect to the model component, three different approaches were tested: the linearized scheme already used in the operational NWP system (IFS); a relaxation to a climatology (IFS-TM5); and an explicit chemistry solver which attempts to simulate the heterogeneous processes on PSC particles (IFS-MOZART). This comparison was apparently expected to deliver the most interesting results: an insight into polar ozone depletion processes, or at least into the ability of coupled chemical weather system to deliver realistic medium-range forecasts. It turns out that the results are negative, with all three versions failing to forecast the onset of the ozone hole.

Unfortunately, we think that these negative results are not an original contribution to the field. The climatological approach (IFS-TM5) can be viewed only as a worst-case reference, because it defeats the whole purpose of atmospheric modelling for chemical weather applications. Previous publications (Cariolle and Teyssedre, 2007; Kinnison et al., 2007 - see also specific comments below for p.9180) had reported the failure of the two chemical modules to simulate the heterogeneous chemistry processes which cause polar ozone depletion. The IFS-MOZART model (Flemming et al., 2009) is an interesting approach to combine the transport algorithm of IFS with the explicit chemical scheme of MOZART, but can not be expected to fix the shortcoming of MOZART with respect to polar stratospheric ozone depletion. These previously published difficulties are not discussed further, nor even recalled in the present paper - a very disturbing omission.

This study also focuses on the "chemical predictability" of stratospheric ozone but does not try to define strictly the concept, which is simply tackled with some ad-hoc results (figure 7, bottom). The impact on the chemical forecasts of the initialization with chemical analyses, i.e. the persistance of the information brought by chemical assimilation, is shown but not evaluated quantitatively. Additional 15-day forecasts with ozone as a passive tracer, at least, would have helped to clarify the impact of the initialization with ozone analyses.

In summary, this paper is written with a good style, the figures are clear and the model and experiment set-up contain valuable information. It could have led to an interesting study if it had been based on chemical modules able to simulate correctly polar ozone depletion. But in view of the inadequacy of these chemical modules, the absence of improvement with respect to stratospheric ozone already delivered by the operational NWP system (IFS), and the superficial treatment of the central concept of chemical predictability, this study is not sufficiently original and significant to allow publication in Atmospheric Chemistry and Physics. Since it is useful to report the status and present performance of an important project, we suggest the authors to submit instead this manuscript to the companion journal "Geophysical Model Development", as was done already for the model description (Flemming et al., 2009).

Specific comments

p.9175, l.14: forecasting of stratospheric ozone depletion, at least at the time scales studied here, is not important for assessment nor for monitoring purposes.

p.9176, l.25: "... reactive gases such as...": are these 5 species a precise list of the chemical species actually assimilated for GEMS, or is the list actually longer? For the assimilation experiments used here: was ozone the only assimilated species, or were there other species assimilated simultaneously? If yes, what species?

p.9177, l.7: is the NRT provision of boundary conditions for RAQ forecasts already in place or is it just an important application for the future?

p.9178, l.1: 1-8: this discussion about predictability is quite vague. You should at least provide bibliographical reference(s) for "meteorological predictability" and propose a definition for "chemical predictability" (see general comments).

p.9178, l.10-11: the ozone lifetime reported here, and explained in classical textbooks, is based on 2D models with comprehensive chemistry - not on the Chapman cycle alone.

p.9178, l.13: Eskes et al. (2002) obtain a predictability range of 4 to 5 days - in what altitude range? For the whole total column? If yes, this information is not relevant for present systems which attempt to provide realistic information about the shape of the vertical profile.

p.9178, l.27: This sentence is false in many cases. The initialization of the stratospheric ozone fields is in fact *not* important for CTM runs which last much longer than the longest ozone lifetime encountered in the atmosphere, e.g. 1 year or more.

p.9179, l.6: While the ozone hole size below 220DU is a classical diagnostic, it would be useful to provide a bibliographical reference where the value of this diagnostic is discussed.

p.9180, l.8: Cariolle and Teyssedre (2007) show 3 forms of this parameterization, all failing to deliver sufficient ozone depletion at 100 hPa (their fig.13). It would still be useful to state which form (i.e. what version of the parameterization) is used here.

p.9180, l.16-19: Kinnison et al. (2007) show (their fig.16) that the ozone hole is not reproduced by MOZART-3 when driven by ECMWF fields and discuss the possible causes. It seems completely in the scope of this paper to push this discussion further, or at the very least to recall it (see general comments).

p.9182, l.15: Aura-MLS retrievals have a sufficiently high vertical resolution to be described as profiles rather than partial columns. The processing of these observations by the assimilation system is actually another question : from table 1, it appears that the profiles are transformed into 16 partial columns prior to assimilation. Why is this done? Could this result in some loss of information about the profile shape?

p.9184, l.18-29: As I understand it, analyses departures from the assimilated observations (fig.3) are primarily a verification tool to check that the assimilation system worked correctly. In this study, several instruments are assimilated and the analyses agree much better with one of them (MLS). While this allows to discuss the OMI-MLS and SCIA-MLS biases, the fact remains that OMI and SCIA observations could not be assimilated as well as MLS. The possible causes should be discussed.

p.9185, l. 12: where does this maximum bias of 3

p.9186, l. 10: Please adapt the CTM description to the topic under study. Wild-fire emissions are completely irrelevant here.

p.9187, l.9-13: This kind of quick-look visual check is not acceptable in a refereed journal. If humidity is relevant to your forecasts of the ozone hole, it must be evaluated and discussed in a statistically meaningful way.

p.9188, l.13-16: The forecast runs present huge biases w.r.t. observations (figure 6). Please mention that this is discussed in the next section.

p.9188, l.23 until end of paragraph: this attempt to justify the failure of IFS-MOZART with inadequate wind fields makes no sense, in view of the failure of MOZART itself (see general comments and comments for p.9180).

p.9191, l.17: MLS does contain the information necessary about the shape of the ozone profile, including in the ozone depletion altitude range. It has also been shown that MLS is the biggest contributor to the analyses. So the only issue here is with respect to the tropospheric part of the profile. Unless the pre-processing of Aura-MLS into 16 partial columns led to some loss of important information...

Technical corrections

Table 1: Two different datasets were used for MLS - identify the period for each

Table 2: Column "FC Length" seems to be in hours? But from figure 7, the lengths of FC15 experiments seem to be 15 days? Please clarify

Figure 3: the two periods must be labelled more clearly

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 9173, 2010.