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Interactive comment on "Tropospheric chemistry in the integrated forecasting system of ECMWF" *by* J. Flemming et al.

N. H. Savage (Referee)

nicholas.savage@metoffice.gov.uk

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Overall

This is generally a very good manuscript which presents an important step forward in tropospheric chemistry modelling by having a fully online chemistry scheme in one of the world leading NWP models. The importance of this work is further increased by the fact that it is already being used to deliver the Copernicus Atmospheric Monitoring Service. In general the methods used to parametrise tropospheric chemistry in the model are sufficiently well described and presented clearly. It is also important because as well as showing the improved skill of the new model, it also demonstrates that online modelling is much cheaper computationally in this case. A wide variety of data sources are used to evaluate the model including satellite observations, aircraft and surface

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data.

With some minor corrections and clarifications (see below) I consider this paper to be suitable for publication in GMD.

Title Please include the version of the IFS used in the title of the paper as required by GMD (CY40r1)

Section 2.2 How large an impact does the correction of negative MMRs have on the budget of transported species?

Section 2.3 Are the biogenic emissions calculated from MEGAN offline and read from a file or is MEGAN used online?

Section 2.4.3

Please note that Price and Rind (1994) derived a correction factor for cloud top height as the resolution of the model is decreased. They found that as resolution decreases the global lightning frequencies decrease exponentially and corrected for this with a calibration factor. Was this calibration factor used in this implementation?

Section 2.5.1 Please give more details of the heterogeneous chemistry of SO2 - what reactions are included, how is the pH calculated and how is the relationship of pH and reaction rate calculated? For the heterogeneous conversion of N2O5 into nitric acid, how is the surface area of aerosols calculated - does it account for hygroscopic growth? Which aerosols are used - the prognostic scheme in the IFS or a climatology? Is the surface area of water droplets based on a calculated size distribution or is there an assumed size distribution used in the cloud scheme?

Section 2.5.4 Please explain in more detail what data is used to prescribe surface methane concentrations - are they based on observations or a model (and give a reference). Spatially is a single number used everywhere, is a zonal mean or a 2D fields? Temporally is there a seasonal cycle or is it the same all through the year? (I note that it is mentioned later that monthly zonal mean concentrations are prescribed, but still no

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reference, and it would be better to say that here)

Section 3.1 One model resolution is given here a spectral truncation and the other in lat-lon spacing. It would be useful here if the approximate resolution of both is given in km to make it easier to compare the resolution of the two models.

Section 3.2.1 How is the model sampled for comparisons with MOZAIC data?

"Only the rural Airbase O3 observations have been selected for the evaluation of the diurnal cycle". Please clarify - two plots for surface ozone over Europe are shown - a seasonal and a diurnal cycle. I assume that both of these analyses were carried out using all EMEP data and the Airbase observations at rural sites. If this is correct, this would be better phrased as "For evaluation over Europe, the EMEP observations and the rural Airbase O3 observations were used."

Section 3.3 Does the lower bias in C-IFS imply that the Cariolle scheme is performing better in the lower stratosphere than the MOZAIC chemistry? If so, please comment on this and implications for future choices of upper boundary conditions for ozone - if the Cariolle scheme is cheaper and better, why are you planning to add a detailed stratospheric chemistry scheme to CB05?

Section 3.4 I would say that the model reproduces well only the location of the global maxima (the manuscript correctly identifies the commonly observed underestimation of CO in the NH later on).

Section 3.5 The low bias in the outflow regions may also be related to insufficient production of NOx reservoir species such as PAN and alkyl nitrates.

The overestimation of NO2 in the biomass burning region coupled with the underestimation at this time suggest that the emissions modelling may be the issue here rather than the fire count.

Section 3.6 Is the underestimation of winter HCHO in Eastern US possibly linked to the ozone bias here as well? Is there some important winter time chemistry missing in

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both CB05 and MOZAIC?

Section 3.8 I found the following sentence confusing. "The additional resources allocated to the IFS are however mostly latent as the coupled MOZART model and the coupler software could not be made faster by using more resources." I think what is implied here is that in order to get enough memory, the coupled MOZART model needed to be run using a large number of CPUs. However, this is inefficient because there is insufficient parallelism in the coupled model to exploit this large CPU count. Please clarify.

The more complex chemistry schemes presumably require more resources to run because of both the costs of tracer transport and the extra chemistry. It would be helpful to indicate how the additional costs are spread between these two aspects.

Section 4 If a consistent chemistry scheme were used in the C-IFS to that of the coupled framework, this would help resolve whether the improvements in SO2 for example are due to difference in the diffusion schemes as suggested in the paper.

Interactive comment on Geosci. Model Dev. Discuss., 7, 7733, 2014.