Atmos. Chem. Phys. Discuss., 14, C1411–C1413, 2014
www.atmos-chem-phys-discuss.net/14/C1411/2014/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Uncertainties in assessing the environmental impact of amine emissions from a CO_2 capture plant" *by* M. Karl et al.

M. Azzi (Referee)

merched.azzi@csiro.au

Received and published: 14 April 2014

It is commendable to see the application of a comprehensive photochemical modelling system coupled with a Fugacity Level-III multi-media model to investigate the air pollution (and associated partitioning of the depositional mass into lake water) externalities associated with the use of MEA to scrub CO2 from flue-gas exhaust in a large scale (hypothetic) carbon capture plant. The paper and supplementary material are well structured and are useful in that they include a detailed sensitivity analysis which addresses the uncertainties in the meteorological modelling, the chemical transformation pathways, the plume rise, wet and dry deposition and differences compared to earlier

modelling studies in the same region of Norway. This sensitivity analysis is useful in that it sets up a baseline for on-going research in the area of MEA CCP. The authors are encouraged to publish the paper subject to addressing some concerns expressed below. 1. The authors have used the EMEP model for the chemical transport simulations of a hypothetical CCP point source. Point source in-plume concentrations are sensitive to plume rise, chemistry, vertical diffusion and horizontal diffusion and thus all of these processes need to be considered for this source geometry. However, the state equation in Simpson (2012) does not include a horizontal diffusion term (as acknowledged in the Simpson paper). While it is acknowledged that horizontal diffusion in grid models can be dominated by numerical diffusion due to the finite differencing approaches used in advection schemes (and the treatment of the point source as a volume source), horizontal diffusion under convective conditions can dominate numerical diffusion, particularly for the small grid horizontal grid spacing used in this study. 2. The sensitivity of ground-level concentrations to plume rise are likely to be partially an artefact of the coarse model spacing in the vertical. This is noted by the authors who comment that a refinement of the vertical structure of the EMEP is currently underway. In lieu of providing the results of additional simulations with a finer vertical structure for the paper it is suggested that the authors also highlight the impact of the relatively coarse vertical resolution in Section 3.4 where plume rise sensitivity is discussed. Another alternative is to use Gaussian plume modelling to investigate the sensitivity of near-source concentrations to the choice of plume rise algorithm. 3. Further to the plume rise discussion, the authors use equation (1) to estimate the wind speed at stack/plume height give the wind speed at the first model level. The use of (1) should be restricted to $-Z/L < O(2)$ for unstable conditions and $Z/L > 1$ for stable conditions. Can the authors confirm that (1) is restricted only to applicable limits? 4. In section 2.2 the authors discuss the emissions of SOx (SO2 + SO4) and NOx (NO + NO2) and refer the reader to Table 1 where the CCP emissions of NOx are documented. It is noted that NO2 emissions are present and make up about 3% of the NOx on a molar basis. However, the use of a caustic solution in the CCP would reduce concentrations

of NO2 in the flue-gas to trace amounts. Can the authors provide justification of why this was not considered when modelling the emissions of NOx from the CCP? 5. There is some focus on the sensitivity of peak MEA+DEYA concentrations in the model cell which contains the CCP facility. In discussing this sensitivity the authors should also note that the peak concentrations will be a function of the cell volume and may be the subject of errors due overshoot/undershoot of the numerical advection scheme which is not able to accurately resolve single cell emissions close to the point of emission. In fact, given the small sensitivity of the primary emissions to chemistry close to the stack, did the authors give consideration to using a Gaussian plume or similar model for resolving the near-field concentrations? 6. In nesting from 50 km to 2 km, the model grid spacing decreased by a factor 5 for each grid nest. A more typical reduction is a factor of 3 in order to minimise aliasing errors. How do the authors justify the larger factor? 7. In section 3.2 the authors provide a comparison of NO, NOx, O3 and Ox. Note that contemporary observations of NO2 and actually the sum of NO2 + HNO3 + PAN etc. and thus the modelled 'NO2' should consist of the same summation. This is particularly important for aged air masses. Can the authors confirm that the comparison of observed and modelled 'NO2' was done this way?

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 8633, 2014.