Atmos. Chem. Phys. Discuss., 14, C2492–C2493, 2014
www.atmos-chem-phys-discuss.net/14/C2492/2014/


Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "Methane and carbon dioxide fluxes and their regional scalability for the European Arctic wetlands during the MAMM project in summer 2012" *by* S. J. O'Shea et al.

**Anonymous Referee #2**

Received and published: 19 May 2014

The manuscript presents flux estimates for methane and carbon dioxide derived from different methods on different scales. The core novelty of this study is the determination of regional-scale greenhouse gas fluxes based on airborne measurements, which are then compared to eddy tower fluxes and chamber fluxes within the flux footprint, and which are then used to assess the skill of two land surface models, i.e. JULES and Hybrid8 for a study area Northern Fennoscandia.

As the airborne fluxes are the main new feature of this study and they are not published elsewhere before, it is particularly important that the methodology to obtain these fluxes is well documented, in order to allow an assessment of their representativeness and



reproducibility. However, the description of the flight design is vague and sometimes unclear. The authors write (p8463 l16) they performed transect flights at an altitude range of 100 m to 2000 m. What exactly were the flight levels for the different transects and how constant was the flight altitude during one transect? Later in the manuscript (p8468 l15), they write that the range for the transect flights was 70 to 1287 m and 103 to 1382 respectively. If I interpret this information correctly, this is a very unorthodox flight experiment design, as the aircraft apparently changed its altitude and its east-west position simultaneously. Hence, it is not possible to distinguish whether the observed changes are due to the movement in the horizontal direction or in the vertical direction. In addition, there is a possibility that the observed changes are due to non-stationarity of the PBL. Maybe I am misinterpreting the authors' description but then it needs to be clarified in the manuscript.

The mass-balance approach relies on a series of assumptions and these need to be verified through measurements whenever possible. These are well-mixed conditions (vertical profiles), stationarity (always flights along the same track in both directions, preferably around midday), zero entrainment flux (stable boundary layer height, check by several soundings in between), and horizontally homogenous wind field (flights at several levels). A much more detailed description of the flight experiment design is necessary to be able assess whether the airborne measurement strategy is valid. As it is now, too many questions remain unanswered. In general, this study lacks focus and therefore I would recommend to focus much more on the description, analysis and interpretation of the airborne measurements. In my opinion, major revisions are required and the revised manuscript will need another round of reviews.

Just one technicality: Figures 1 and 6 are much too small.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 8455, 2014.