

## ***Interactive comment on “Top-down estimates of European CH<sub>4</sub> and N<sub>2</sub>O emissions based on four different inverse models” by P. Bergamaschi et al.***

### **Anonymous Referee #2**

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This paper presents four inverse model estimates of European methane and nitrous oxide emissions. It compares these estimates with those reported under the UNFCCC and from the EDGAR inventory. The authors find evidence for under-reporting of European CH<sub>4</sub> emissions, and N<sub>2</sub>O emissions that are largely consistent with the UNFCCC reports.

The paper is well written and relies on established models, observations and methods. Therefore, I think it can be published in ACP, following a few minor corrections.

Specific comments:

Abstract, line 1: I don't think it's appropriate to describe the models as “independent”. There are several shared components between the different inversions (e.g. three

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models use some variety of ECMWF met fields).

Page 15687, line 17: should this be “. . . such as European CH<sub>4</sub>, . . .”

Page 15691, line 9: were there any specific reasons why LMDZ-4DVAR “S2” inversions weren't performed? If so, could you provide reasons, otherwise, perhaps this should say that they “weren't available for this analysis”.

Page 15691, line 16: give a justification for the use of daytime/nighttime measurements.

Section 3.2 (and results/conclusions): There are many “subjective” choices that have been made throughout this section. For example, correlation length scales, a priori uncertainties, model representation errors, etc. In some cases these appear to differ markedly, for example, the apparent maximum representation error of 1ppm for TM5-4DVAR, but only 30ppb for TM3-STILT. It would seem to me that these choices could explain a relatively large fraction of the observed discrepancies between the models. Perhaps the discussion in the results or conclusions could be expanded slightly to highlight this, in addition to model systematic errors, etc (and suggest ways forward?)?

Page 15692, line 22: Why was this “relaxed” constraint not also needed for methane? Aren't there similar “hot-spots” for CH<sub>4</sub> (e.g. the polish coal mines that are mentioned elsewhere)?

Page 15693, line 28: I think this should be “off-diagonal”, rather than “extra-diagonal”.

Page 15694, line 3: these are only “estimates of transport errors”, which are actually unknown.

Page 15694, Line 6: where have the methyl chloroform observations been obtained from? A reference should be given.

Page 15699, line 5: “. . .due to fewer models BEING available”?

Page 15701, Line 17: “commonly” instead of “standardly”?

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