Rev 1

P4 L120-121: The percentages of data filtered for CH4 are given here only; the data amounts filtered for N2O (17, 15 and 9%; Review Response P7) should also be included.
Review Response P10, relating to comment on P869 L24: The authors have provided the information requested by the reviewer, but this should actually be included in the text (at P10 L327 of the revised manuscript).
Review Response P12, relating to the comment on P874 L16: The sentence regarding isotope measurements (P14 L460-462), even with the newly added reference to Rigby et al. (2012) should not be included in the conclusions of this study – no new information was provided in the paper at all to support the use of isotope measurements. The authors could rather refer to this by concluding that they could only poorly resolve source processes, thus highlighting the limitations of concentration-only measurements, and they may then tentatively suggest the utility of isotope measurements – although no simulations were performed to show that this would help in the present case.
Regarding natural N2O emissions: It is unclear whether the technique of multiplying Saikawa et al. natural soil emission estimates by the proportion of natural land is valid, even considering the limitation of the Saikawa et al. estimate – because natural N2O emission can still occur as a `baseline` from agricultural lands ie. Some of the N2O from these lands is due to fertiliser and thus anthropogenic, and some would occur regardless. Similarly, deposition of N (eg. From NH3) on natural soils means that not all emissions from natural lands are natural emissions.
The way the authors have dealt with this, ie. To multiply the estimate by the proportion of natural land, is adequate because it is close to impossible to estimate natural and anthropogenic soil emissions more accurately with present information. However the authors should be more careful in highlighting this problem in both their prior, and in their posterior estimates of natural vs. anthropogenic soil emissions.

Rev 2

The authors have thoroughly replied to the issues raised in the first review and have made the corresponding changes in the paper. Therefore, I recommend this paper for publication after a few minor issues have been addressed.

L8: state what the prior estimate is based on

L12: insert "annual mean" before "N2O emissions"

L25: missing hyphen "100-year"

L118-120: while it is wise to remove data with very strong local influence, the explanation for doing so should be improved. The authors state that the data were removed because these data were "more likely to be affected by local processes due to the more stagnant air". The main problem with these data, however, is that due to deficiencies in the model, i.e., accuracy of atmospheric transport, spatial and temporal resolution of both the transport model and the fluxes, these data cannot be represented by the model. This, and the fact that the influence on observations at these times is very strong, would result in large errors in the model, which would lead to errors in the retrieved fluxes.

L122-123: there appears to be a verb and preposition missing in this sentence: "…the measurement uncertainty was described by the variability…"

L191: missing hyphen “30-day”

L193: replace “that air” by “from which air”

L196: this sentence could be made clearer, suggest: “…originate from the southern outer domain boundary”

L197: replace “formed by” by “comprised of”

L312-314: suggest changing this sentence to “A difference in natural emissions…” since the posterior estimated fluxes include the natural fluxes (it is the total flux that is estimated). The current formulation suggests that the posterior flux estimates exclude natural fluxes.

L322-323: perhaps the difference between the Bergamaschi et al. 2014 results and this study is due to the different observation dataset. In Bergamaschi et al., the new UK stations were not included.

L383: insert “ratio” after “signal-to-noise”

----- Rev 3

Overall the authors have addressed thoroughly most of the comments which I had rai

However, I have the following remaining comments:

It is not clear to me, why the authors consider the difference between their posteriori CH4 emissions for the UK and the priori 'statistically significant'

anthropogenic prior: 2.42 +/- 0.48 Tg CH4 yr-1 (Reported CH4 emissions +/- 20%)
natural : according Table 3: 7-9 % of total emissions, e.g. ~0.2 Tg CH4 yr-1 (but I wo

total posteriori CH4 emissions 2.09 (1.65–2.67) Tg CH4 yr-1

i.e. the total prior is just at the upper end of the total posterior, but still within the giv also the uncertainty of the prior should be taken into account.

Table 1/2: 'A priori values used': Only the inventories are listed but not the values (i.e

Table 3/4: would be useful to include here also the absolute CH4 / N2O emissions UK

'Wetlands and rice' : I assume there is no significant rice cultivation in the UK / Irelan

update reference [Bergamaschi et al., ACPD, 2014] to [Bergamaschi et al., ACP, 2015