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Interactive comment on "The consumption of atmospheric methane by soil in a simulated future climate" *by* C. L. Curry

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I thank the referee for his/her comments on the paper. I agree with points 1, 2, 3 and 6, and have made the requested changes in the revised manuscript. The following is in response to the two substantive issues raised in Referee 2's report.

4. As noted by the referee, the aggregated Holdridge life zone (AHLZ) scheme adopted in this and the previous paper does not include croplands and grasslands as specific classes. This is likely because these two types of land cover, which are extensive, overlap with several different AHLZ classes. E.g., comparison of the AHLZ map with the HYDE 5' resolution dataset for 1990 (http://www.mnp.nl/en/themasites/hyde/index.html), shows that AHLZ classes 5, 6, 10

and 12 (cool temperate forest & scrub, and subtropical/tropical forests) overlap with large regions where the crop fraction is greater than $\sim$60%. The same is true for the 1990 HYDE pasture map (usually considered a subset of grasslands), although the overlap is with different AHLZ classes. A note will be added in Table 2 detailing this correspondence.

Certain other land cover classifications in the literature— e.g., the Leemans (1990) scheme used in the recent compilation of Dutaur & Verchot (2007)—do include grasslands and croplands as separate categories. While the choice of a land cover classification scheme is subjective, I felt it made sense to use the same scheme as in Paper I for purposes of comparison.

5. The referee notes that "The paper does not include any comparisons of modeled CH4 uptake rates with field data." Model validation was carried out in Paper I, and was therefore not covered in the present work. However, the referee points to an apparent model underestimate of the CH_4 uptake rate in deciduous forest soils, compared to two observational studies. Indeed, the uptake in cool and warm temperate forests given in Table 2, which ranges from 0.4 to 0.7 mg CH_4 $m^{-2}\,d^{-1}$, does fall well below the range of these two studies, $1.7-6.9$ mg CH_4 $m^{-2}\,d^{-1}$.

However, it is dangerous to directly compare model results, especially from a coarse resolution GCM, with two isolated studies of short duration at point scale. The comprehensive meta-analysis of Dutaur & Verchot (2007) (hereafter DV07), which compiled results from over 120 published papers, indicates a much larger range of CH_4 uptake in temperate forest soils (N=92): 5.82 $\pm$ 5.61 kg CH_4 ha^{-1} y^{-1}, or 1.54 $\pm$ 1.52 mg CH_4 m^{-2} d^{-1} ($\pm 1\sigma$; see their Table 2). This standard deviation is 2.6 times larger than that of uptake measured in non-forest soils, despite a comparable number of observations in the two categories. Hence, there is—unfortunately—no compelling lower bound from observations on the absolute value of CH_4 uptake in temperate forest soil ecosystems.

It may be slightly more meaningful to compare the overall range of uptake values and

ratios between different biomes in the model versus observations. This comparison suggests that the model has a smaller range of uptake over different ecosystem types than in the real world. E.g., the ratio of areal uptake in forest ecosystems (temperate and non-temperate) to all other ecosystems is 1.6 in the model compared to 2.6 in observations. Within the forest classification, there is also some suggestion that model uptake in tropical/subtropical regions is larger than in observations. However, this is difficult to verify due to the use of different land cover classifications in our work versus DV07: i.e., it is unclear how much of the area termed “subtropical" in the AHLZ scheme lies within each of the areas termed “tropical" and “temperate" in DV07.

Thus, in response to the referee’s suggestion, “...at least some discussion comparing modeled values with measurements would give readers more confidence in model predictions," I have added a few sentences to Sec. 4 of the revised manuscript explaining the observational constraints, such as they are, and focusing in particular on the ratio of forest to non-forest CH_4 uptake rates along with the relevant caveats.

These conclusions do not, in my view, support a serious re-evaluation of the model parameterizations or optimized parameters at this time. However, I am mindful that both model parameters were calibrated against time series data at a grassland site in the continental U.S., as detailed in Paper I. Therefore, it may be valuable to seek out similar long-term measurements at a forest soil location for a reevaluation of these fiducial parameters. At present, however, I am not aware of such a dataset.

References

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