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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The atmospheric cycling of radiomethane and the "fossil fraction" of the methane source" *by* K. R. Lassey et al.

K. R. Lassey et al.

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On behalf of the authorship, I appreciate the considered and positive comments of the two referees from which the manuscript will benefit. We propose to modify the manuscript to take account of those comments and to further improve the manuscript as described below. Note however, that both referees work from a pagination that is different from the "screen pagination" as published in ACPD. This paper also has a preceding "companion paper" (Lassey et al., 2006) and we are mindful that any changes in nomenclature need to consider both papers.

Response to Anonymous Referee #1

The comments by this referee are well taken, and we propose to implement most of them as detailed below.

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Interactive Discussion
Discussion Paper

1. The referee's "chief concern" – a concern also expressed by Referee #2 – "is the assumptions made about the constancy of the methane sources since the mid-1980s". We propose to explicitly address this issue by performing identical regression analyses over the shorter (10-yr) regression intervals 1986–1995 and 1991–2000, supplementing the main analysis over 1986–2000. Any systematic change in source through the latter 15-yr period should result in marked differences between the two 10-yr analyses. There are no such differences. However, the referee's concern raises the wider issue about sensitivity of our results to assumptions, and we have expanded the paper slightly (2 paragraphs plus a small table) to include not only the two 10-yr sub-intervals but to also address the sensitivity to the "time-lag distribution" (see Comment #14).

2. The referee asserts that "leaks from the enormous global natural system are somewhat under-discussed", apparently with the Russian gas system in mind, a system "which is now probably fairly tight, but may have bee leaky in [the Soviet era of] the late 1980s". We are unfamiliar with what the referee describes as "much literature on the improvement in the Russian gas system", noting that the paper by Reshetnikov et al. (2000) that the referee cites and with which we are familiar assesses fugitive emissions only during the Soviet era. We are aware that Dlugokencky et al. (1994a) conjectured that improvements to that gas system may have contributed strongly to the 1991 decline in the atmospheric accumulation rate – indeed some of the present authorship made similar conjectures: (Lassey et al., 1994). However, this explanation was not without its critics (Rudolph, 1994; Hogan and Harriss, 1994; Dlugokencky et al., 1994b), and with hindsight the 1991 anomaly sits in a context of marked interannual variability during the 1990s (e.g. Bousquet et al., 2006). Consequently, we prefer not to digress into conjectures about the Russian gas system, especially when our work does not corroborate a systematic change in the global source.

3. For similar reasons as in Comment #2, we prefer not to speculate on clathrate emissions and submarine landslides as possible reasons why the global methane source might have changed during 1986–2000 when (a) only systematic changes (not episodic

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if small) will affect our analysis, and (b) our analysis does not support a systematic change.

4. The referee challenges (without saying why) the assumption "that the source has not varied since the mid-1980s" (he/she should have stated "... not varied systematically ..."), but would have been happy with "... since the late 1980s [which] could make a big difference for the fossil fraction calculation". Our consideration of the sub-interval 1991–2000 suggests that these concerns are unfounded.

5. A reference is appended to justify the choice of global source and its δ^{13}C value.

6. The referee's discussion about the abbreviation "BSR" and its application in marine geophysics ("Bottom Simulating Reflector") is somewhat peripheral and barely relevant in the present context. That abbreviation is indeed defined before its first usage (page 5043). While we were unaware of that usage of "BSR", in deference to it we will change to "BR" (biospheric radiomethane) in both this and its companion paper.

7. We see no need to offer concluding comments on Russian gas leaks and Chinese coal emissions for reasons outlined in Comment #2.

8. A "brief table" listing terms and abbreviations is, in our view, unnecessarily space-consuming in such a short paper. Almost all of the mathematics is contained with the section on "Mathematical framework", which the non-mathematical reader is invited to skip. Key usages of mathematical entities outside that section either are or will be defined or cross-referenced.

Response to Anonymous Referee #2

We appreciated the referee's kind comments, and propose to implement his/her recommendations as follows.

9. We were remiss in not acknowledging the pioneering work of Ehhalt (1974) and propose to remedy this.

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10. The “relatively small” tests conducted by France and China in the late 1960s and 1970s is a quantitatively accurate descriptor when judged against those by the US and Soviet Union prior to the Partial Test Ban Treaty in 1963. Rath (1988) estimates post-treaty bomb yields as totalling about 50 Mt-TNT equivalent spread over more than 15 years, compared to the cumulative yield to 1963 of about 600 Mt-TNT, most concentrated in 1961–62. None of the post-treaty tests caused more than a minor blip on the $\Delta^{14}C(CO_2)$ curve of Hua and Barbetti (2004, Fig 1) which we will cite. The bomb ^{14}C datasets are also discussed in some detail by Lassey et al. (1996).

11. While we would be happy with the referee’s suggested rewording about the value of the bomb ^{14}C pulse, we have elected to remove this sentence and its reference in the interests in minimizing overlap with the companion paper.

12. We have taken account of this referee’s comments on the assumption of constant source, noting that Referee #1 also had similar reservations. We have expanded the relevant discussion and conducted some sensitivity tests that we will report. See comment #1. It should be borne in mind that our approach is insensitive to “random” inter-annual fluctuations—after all the atmospheric dataset is smoothed – but we do wish to address possible systematic changes.

Other proposed changes

13. The findings of this paper are affected by the realisation that the PWR dataset in Table 3 of the companion paper is in error. Unbeknown to us, it did not include electricity generation by Soviet-designed PWRs, which were designated WWERs (Water-cooled Water-moderated Energy Reactors) until the two categories were recently merged into a common category also termed “PWR”. We propose to incorporate the corrected dataset in which electrical generation is 12–15% higher after 1986. The only appreciable net effect is that fitted “NPR factors” are correspondingly smaller and the fossil fraction slightly larger (by $\sim$0.6% in magnitude).

14. We also propose to adopt the exact implementation of the lag-time distribution (with

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mean lag time, τ_{lag}, of 6 years) from the companion paper, rather than approximate it by a lag of exactly 6 years. As part of our sensitivity test, we will cite the latter approximant, as well as examine the consequences of varying τ_{lag} within its plausible range.

15. We propose to drop the inessential subscript "NPR" from ϕ.

16. We propose many additional minor textual improvements identified by the authorship, mainly with a view to trimming the text length and reducing overlap with the companion paper.

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