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Discussions

Interactive comment on "The Global Methane Budget: 2000–2012" *by* Marielle Saunois et al.

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Review of Saunois et al: The Global Methane Budget 2000-2012

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General Comments

This study by Saunois et al. is important, of high quality, and should be published. This excellent work is a major and powerful contribution to our understanding of the planet, and the way the Earth is changing under our anthropogenic impact.

Saunois et al. collate and discuss what is known about the global methane budget in the first 12 years of the century. The work is extremely comprehensive, and the comparative analysis of differing inputs and points of view is both penetrating and incisive: this is an essential synopsis of the 'state-of-the-art' of our knowledge of this very important greenhouse gas. The paper is clearly and logically written, and has been carefully

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presented: I only found two very minor typographical errors.

The paper fully reviews our understanding of the Global Methane Budget. It is a major and significant updating from the influential Kirschke et al (2013) paper. The significant points are: 1. The great discrepancy between estimates of methane emission assessed by Bottom-up (∼736±about 150Tg) and Top-down (∼558± about 20 Tg) methods. This gap is so great it falls outwith the very wide error bounds. 2. The discrepancy may come primarily from conflicting estimates of emissions from freshwater and geological sources. There is a degree of agreement about anthropogenic emissions. 3. Top-down studies point to the tropics – S. America, Africa and SE Asia - as the source of nearly two-thirds of emissions. 4. There is a major need to improve in situ methane observations at the local scale, especially in the tropics.

The paper covers the period up to 2012. Thus it ends before the extraordinary methane growth of 2014 and 2015: it is hoped that the next update of the work will have much to say on these years.

I have only one significant comment, which is that the discussion of the isotopic composition of atmospheric methane is given very short shrift, tucked away as it is in a minor sub-section of Section 4.1.3. This discussion is correct as it stands but is surely inadequate: isotopes offer extremely powerful insights into the budget and provide potent constraints on changing relative inputs. But I must declare an interest and a bias in this comment: I work on isotopes.

Overall, the paper, which will be very highly cited, is publishable with only the most minor amendments. But it would be nice at least to give isotopes their own sub-section!

Specific Comments

1. Introduction: This section provides a very useful general review. It is a telling comment that uncertainties in emissions reach 40-60% at local scale in the tropics. The comment that methane growth is tracking RCP8.5 is very worrying and could perhaps

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be added to the Abstract. The Introduction skates over the problem of how to achieve optimal use of satellite retrievals, which have in the past led to some perhaps misleading inferences.

2. Methodology. The paper broadly follows the established methodology of Kirschke et al. (2013). This is good. However for future updates it might be worth considering an attempt to split agricultural emissions into ruminants, rice, and agricultural waste/silage/manure and biomass burning categories, and to address urban waste fully independently from agricultural waste/silage/manure.

3. Methane sources and sinks. The discussion of inventories is excellent and very helpful as a general summary. The recent sharp increase in bottom-up source inventories, close to RCP8.5, is commented on. What is only lightly discussed (nor later in Sect. 4) is the isotopic implication and the clash with observations. Also, the ethane results (3.1.3) may merit a little more comment. The discussion of China's coal emissions is valuable. Perhaps R. Thompson et al's (JGR 2015) findings that methane emissions from China increased by 3% annually from 2000 to 2011 is worth citing? Shale gas / Fracking: Zavala-Araiza et al probably need more discussion as this is a very interesting paper and TD and BU estimates were close. One point is that the emissions were dominated by a few high-emitters, and that these came largely from the 'conventional' part of the extraction complex – it's not the fracking, but what happens after the fracking. . .Indeed, the paper could lead to an optimistic inference that these high emitters (which must surely be expensive) can easily be found and controlled. The increasing cost-consciousness brought on by falling prices may be driving leak reduction as much as regulatory controls. There is emerging evidence (e.g. Peischl et al.) that nowadays there isn't much difference between conventional and unconventional gas, at least as far as methane is concerned. Livestock. Assessing emissions is difficult: Africa cattle eat trees and are often water-limited. Indian cattle have experienced poor monsoons. Parts of S. America had severe droughts in this period. But in 2001-2012 Chinese cattle increased (as did melamine consumption). Waste. Urban waste in the

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Middle East and parts of Africa and rapidly urbanising Asia has had little attention. Our own work in Kuwait indicates it may be a significant source. Rice. The changes in rice area in China, and perhaps growth in non-conventional locations like Australia, will need attention in future. Biomass burning. This is a major topic – it is possible that in the isotopic balance, a decline in biomass burning has masked a rise in fossil fuel emissions. The discussion in the paper mostly addresses forest biomass burning, which is very important in SE Asia and S. America. However, my own anecdotal experience from 45 years of travelling and flying annually across the length of Africa is that the bulk of biomass burning in Africa is in C4 grasslands. Similarly, even in forest, peat burning merits more attention. Also, a significant part of the burning may be of seasonal grasses in clearings. It might be worth mentioning the CO record as it pertains to biomass burning Biofuel – this is a placeholder really.

Natural Sources Wetlands, lakes, ponds and streams. Saunois et al point to the discrepancy between bottom-up and top-down estimates of wetland and freshwater emissions. This tallies with my personal anecdotal experience that freshwater bodies deeper than a couple of metres emit little methane. Ebullition is dissolved on rising, or is captured by methanotrophy. There is much need for better studies of freshwater emissions from open lakes and streams. To some extent, the scientific funding system may be a problem here: funding bodies do not like null results and there is always an incentive to claim bigger and more impressive methane emissions from whatever source is being investigated. "My burp is bigger than theirs – give me a grant!").

Land surface models do not in general currently differentiate isotopically between C3 wetland systems (as in boreal muskeg) and C4 wetland vegetation (e.g. papyrus, some C3/C4 phragmites). The uncertainties are huge and need attention. This poor knowledge of freshwater sources is arguably the largest single barrier to a proper understanding of the global methane budget. In particular the fluxes from lakes, ponds and streams need sharp critical evaluation.

A significant puzzle is that in some El Nino events, methane emission often seems to

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rise, while land surface models usually find that it should plummet. Part of the reason may be temperature: Q10 in land surface models is not well constrained and could be a major source of uncertainty. Also, hydrology can be important in large tropical wetlands. In the start of an El Nino event, ground water may be well-charged from the previous season. Thus even a small run off can flood wetlands, albeit not to the extent in a 'normal' season. Later, after evaporation and transpiration, the wetland groundwater level becomes depleted. Thus the fall-off in methane emission should show a hysteresis – it should lag the El Nino.

Geological Sources. Here too there may have been a tendency to aggrandisment of fluxes. Locally, large seeps etc may occur, but how significant are they? Our own group's work in Kuwait suggests the use of mobile CRDS instruments may help constrain regional seepage sources, for example around oil and gas fields. Termites: these insect cows channel emissions from their 'gardens' on the water table via the chimneys of termite mounds, so that egress can bypass methanotrophy.The Saunois paper depends heavily on one study by Sanderson. Perhaps it would also be worth going back to some of the earlier work by Pat Zimmerman and Stan Tyler? Wild animals. These are perhaps a larger factor than estimated. There are enormous numbers of deer still in hiding – SE Asian forest has large populations of small deer, as does North America. How do reindeer fit in? – semi domestic. Camels? Incidentally as sources to amuse, we have found that elephant dung is not significant, but maybe in Venezuela one might consider the hoatzin (an avian ruminant, the stinkbird, the last ruminant of the dinosaur clade). Oceanic – This is a very useful revision of the Cicerone and Oremland 'placeholder' flux that has survived in inventories for nearly 30 years. Maybe cite Westbrook et al (GRL 2009) – methane plumes do not reach surface. Hydrates – maybe cite Fisher et al 2011, showing Arctic hydrate emissions are small. Vegetation. Plants are powerful channels for methane escaping past the jaws of hungry methanotrophs. In wetlands, cotton grass seems to do this; in the tropics tree transpiration brings methane up from anaerobic soil methanogens. Pangala's work is appropriate here. What is interesting as an aside is that SCIAMACHY was used to bolster the plant

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methane story – it illustrates the risk of simplistic interpretation of retrievals, especially from some regions with near 100% thick wet season cumulo-nimbus cloud cover in daytime.

OH oxidation. This is among the very largest unknowns in the global budget. The discussion is appropriate: perhaps it could be expanded a little, given the significance of the uncertainty. Stratospheric loss. Again, this is a large factor and may be changing with the changing incidence of tropical clouds pushing up the tropical tropopause as global warming expands the tropics. The Brewer-Dobson circulation could be mentioned, and the impact of the polar vortex in bringing down depleted isotopically heavy air to the Arctic? Soil methanotrophy. The work cited is old and derivative. Maybe in the next update some of the more recent boreal/Arctic findings could be included. Lifetime. (includes soil and Cl as well as OH). Maybe recapitulate on the difference between different definitions of "lifetime". Dlugokencky's point about the 9.3 yr equilibration time is powerful (mentioned in 4.1.1).

4. Observations Satellites. This is a very helpful discussion. The visually very impressive maps and sweeping conclusions from satellite studies have perhaps exerted an influence on the appreciation of the global budget, that glosses over the problems of bias, clouds and aerosols, and the problems of Arctic cover. Satellites are extremely important and powerful in their inputs, but the retrievals need to be evaluated in light of their uncertainties and inherent biases. That said, satellite results are vital in understanding the lightly-monitored tropics. The point that 'satellite' based inversions include from-the-ground priors is usefully made. Other atmospheric observations. The IASI and TCCON discussions are good. Perhaps more could be said about the incoming use of mobile CRDS and also drones for low-altitude work. This is a major area of advance. First, I must admit bias, but the isotopic section squeezed into the bottom of 4.1.3 is surely the weakest part of the paper. The discussion is OK but too brief. Isotopes are very powerful as source discriminators, and indeed for the insight they give into sinks too. Methane measurements provide four sources of information: mole

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fraction, C-isotopic ratio, D/H, and back trajectory of the air mass. Just using mole fraction and trajectory is a 2D view: in fact the full 3D picture is now becoming adequate to support inverse modelling, and hopefully 4D information will soon be available. The global budget will not be solved until the full range of isotopic information is used. Thus this treatment is very inadequate and would benefit from a significant upgrade in the next generation of the GCP work, 2-3 years from now. C-14: Lassey's 2007a paper is important, and is a major insight, but it only goes to 2000, and perhaps the weight placed on it in the conclusion is too substantial. Inversions The point that inversions use B-U or T-D priors is well made, as is the problem of the large corrections placed on satellite CH4 results (which are, strictly, not 'data' but interpretations). Perhaps there could be a little more discussions of the weaknesses of chemical transport models.

5. Methane budget The study points at the biggest hole in the budget: inland water emission (i.e. open water that is either free-running or more than, say, 1m deep and say a 10 m2 in area). Bottom-up, natural methane is 50% of sources, Top-down, natural sources are about 60%. That's a big difference relevant to the actions of policy-makers. It needs to be investigated. Also, the year-to-year variability of tropical wetland emissions has been studied by various authors – variability is not discussed much in this paper but perhaps variability deserves better, and could be given more attention in a later GCP report? Geological emissions are also a parameter needing re-study. As noted above, the 14C insight mentioned is pre-2000. Interestingly, TD and BU inventories do agree reasonably for anthropogenic emissions. But as the study indicates, that probably needs caution.

Regional Budgets. A very useful part of the study is the emphasis on emissions from the tropics – Africa, S. America and tropical Asia. These regions are poorly instrumented and in the wet season are covered by dense thick clouds, hard to see through. The focus on these regions is an important and valuable part of the study. The study shows that there is a major discrepancy between N. American B-U and T-D Inventories – perhaps that's in part an example of the bias of science funding regimes encour-

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aging ‘discovery’ of larger sources. and discouraging contrary studies that set out to test hypotheses of low emissions. The result that China’s emissions may have been overstated is enormously interesting. Possibly this is linked to the poor quality coal that is being burnt in some cases – high silicate content, so tonnages may be exaggerated as a lot of the mass may be shale. Thompson et al (JGR 2015) is relevant to China’s emissions.

The finding that southern African wetlands are important is interesting. Hitherto the enormous upper Congo wetlands have been largely neglected, but the Chambesi and Luapula swamps are enormous, as are those in the upper Zambesi, and parts of Angola and DRC. The Mweru and Bangweulu wetlands in particular could benefit from study: as someone who has been near the source of the Chambesi, a personal comment is that Northern Hemisphere scientists tend to forget this region.

6. Future developments. The focus is clear – we need better information on freshwater emissions, biomass burning, and better information in the tropics. Personally I would add better isotopic coverage to this list: they seem much neglected, almost forgotten, in this GCP study but perhaps that’s a matter for the future: the next update in a couple of years’ time could address them.

7. Conclusions These are sensible. The study usefully points to the latitudinal source breakdown - 2/3 tropical, 1/3 temperate, minor Arctic, and the need for better in situ tropical measurement. The importance of studying the variability of tropical wetland emissions is perhaps in need of more emphasis. Inventories suffer in general from assumptions that year-on-year changes are small, and that seasonal changes can be ignored. Both these weaknesses need to be addressed.

Overall, this is an immensely valuable study, of major importance, that should be published with the most minor of changes.

Minor Comments 1. Kirschke et al. 2013 is referred to as K13 in reference call-outs. This is unsettling: it would be better to give the full name. 2. Page 9 has a typo in line

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4 - Artic. 3. Page 45 has a typo in line 9 – resultas. 4. In the acknowledgements, spell out P. Bous. 5. Page 67 UNFCCC typo – ational.

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