I am happy to see a review about the indeed very likely underappreciated process of anaerobic methane oxidation in peatlands. The authors present a good overview about a large part of literature on anaerobic methane oxidation in other habitats, and the few works, to a large part by themselves, on AMO in peatlands. This is not their fault, but means that central parts of the article rely on a very thin body of literature and involve lots of extrapolation and speculation. In some subchapters (3.2, organisms responsible) this goes too far, and this part could better be removed or partially transferred to other subchapters (I agree with the other reviewers on the structural problems, see also my detailed comments below). Overall I see significant potential for shortening (also figure 3, parts of chapter 2.3), but in some instances also extensions: for placing the topic into the context of the ecosystem discussed, aerobic methane oxidation and its significance as a methane sink should at least be mentioned in the introduction. I have also indicated few more relevant studies on AOM in other freshwater systems.
The manuscript should also be carefully checked for the instances of artificially built contradictions ('Although,…'; 'In contrast,…').
After addressing the raised issues, I hope that this paper will help giving this important research topic the attention it deserves!
My detailed comments:

Chapter 2.1
p. 7948 l. 8 ff: Should read 'SR outcompetes methanogenesis *for most substrates* based on…
l. 18 f: These tracer studies provided a link to the process, not the organisms
l. 26: techniques do not suggest
p. 7950 l. 13-15: Why despite? Not a contradiction
l. 17 f: The process described by Ettwig et al 2009 is not coupled to SR and therefore not comparable
l. 20: a constructed contradiction; a much bigger body of literature shows that these electron donors are favourable substrates for SRB, not just in freshwater environments; and they do often stimulate SR rates and SRB (e.g. Scholten et al 2002 FEMS ME, Schönheit et al 1982 Arch Micr), but not the SRB involved in AOM.

Chapter 2.2
Several more lake studies that demonstrate AOM: Lake Plußsee (sulfate is e- acceptor, Eller et al 2005, AEM), Lake Rotsee (e- acceptor unclear, Schubert et al. 2010, Aquatic Sciences DOI: 10.1007/s00027-010-0148-5), Lake Lugano (e- acceptor unclear, Niemann et al 2009 GCA Supplement 1 73: A942)
p. 7951 l. 25 – 27: Example for shortening potential; volume of reactors is unnecessary information
p.7952 l. 16: replace nitric oxide by nitrite
l. 18: The results are not inconsistent, because the geochemical settings are different.
l. 20/21: remove 'thus agreeing with…' and the according citations. Another landfill with AOM likely coupled to denitrification is described by van Breukelen et al (J. Contaminant Hydrology 2003 & 2004).
l. 23-25: See previous, 'in contrast' is not appropriate.
p. 7953 l. 4: Why although? Why is man-made in contrast to being a source of CH_4?

Chapter 2.3
p.7954, l. 20: should read 'low net CH_4 production rates due to AOM'? Revise whole sentence, it is difficult to understand and inconsistent; e.g. how can rates serve as a process?

p. 27: Mention here as well the stimulating role of NO3- for denitrifiers as competitors for potentially methanogenic substrates
p. 7955 l. 4 ff: Please mention that the described methodological limitations in methane production and consumption assays could for a large part be overcome by using stable isotope labelled substrates.
l. 6: The same (CH4 limitation) is true for aerobic methanotrophs, see e.g. Hornibrook et al 2009 (Biogeosciences).
l. 10: Why but? Message of sentence unclear.
l. 16-18 and rest of chapter: These are studies on cultured methanogens! Psychrophilic methanogens do exist (e.g. Nozhevnikova et al 2003, AEM 69 (3): 1832), and it can be expected that the communities in a particular habitat are adapted to the range of in situ temperatures they experience. No AOM at 70 C is hardly a surprise, and as long as no different temperature optima for AOM and methanogenesis have been demonstrated, this speculation is not convincing and should be left out.

Chapter 3.1
p.7956, l. 13-18: Too long and redundant, largely discussed in introduction.
l. 21 f: 'Because know mechanisms of AOM…' Which ones? Do the authors mean metals as cofactors or enzyme components, or e- acceptors (Mn and Fe)?
p. 7958 l. 7: Why although?
l. 10-12: Oremland 2010 is not the original article on AMO linked to reduction of N-oxides, and the mechanism is less elusive than mentioned here, compare p. 7952, l. 13-16. In the context of the many speculations in this article, please discuss the possibility that these oxygenic organisms might switch between aerobic and anaerobic lifestyle.

p. 7959
l. 9: 'For these reasons…' – Which ones?
Rest of the chapter: Even though very little understood at present, the initial evidence for AMO coupled to Fe and Mn reduction (Beal et al 2009, Science) in marine sediments should be mentioned in this context. Otherwise, the section should be substantially shortened, especially the speculations on p. 7960 l. 7-17.
p. 7960 l. 18: Again this is not a contradiction; the studied systems are different.

Chapter 3.2: Organisms responsible
In my opinion this chapter is the weakest and should be removed or at least significantly shortened. Over 2 pages the authors reiterate, with several mistakes (e.g. p.7961, l. 14 ff. – ANME 1 are not Methanosarcinales, but most affiliated to Methanomicrobiales), what is known or being speculated for other ecosystems, with the main links to peat ecosystems that the organisms have not been found there.
Furthermore many considerations seem The studies of Zehnder & Brock (1979) and Scheller (2010) only deal with the reversal of enzyme function, not net processes carried out by methanogens. Why would a fermentative SRB (which at that moment does not reduce sulfate) in syntrophy with a methanogen 'leave the door open for AOM linked to SR' (p.7962, l. 10f)? With the argument 'it is unclear why an FeIII reducing organism would utilize CH4 in peat soil where the pool of other organic compounds is often so vast' (p. 7962 l. 22f) the whole article could be declared in vain – why would any organisms prefer the difficult-to-activate methane over easier organic substrates? The authors cite 5 articles dealing with anammox in marine ecosystems, but none stating that anammox bacteria do NOT use a promiscuous monooxygenase enzyme for ammonium activation (e.g. Strous et al 2006, Nature), which indeed would have the capacity to oxidize methane.

p. 7963 l. 4-6: There is such evidence, not for anammox, as mentioned above, but for AMO linked to denitrification, which employs a methane monooxygenase in an anaerobic environment (Ettwig et al 2010, cited above).

Chapter 4

Similarly, this chapter is very verbose and weak, with the conclusions essentially based on a single previous study with circumstantial evidence by the same authors. The other rate measurement reported (Raghoebarsing 2006, p.7965, l. 6 ff) has nothing to do with in situ process rates. I would suggest discussing the lack of knowledge and the methodological difficulties of inferring estimates of global budgets from laboratory experiments or single field observations in the final outlook chapter.

Figure 3:

In my opinion figures in reviews should be of general/conceptual nature, and not just show single experiments. Additionally, no experimental details for the data in figure 3 are provided; it should therefore be removed.