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Interactive comment on "Probing the fate of soil-derived core and intact polar GDGTs in aquatic environments" *by* F. Peterse et al.

Anonymous Referee #2

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This article attempts to assess the stability of core and intact GDGTs in aquatic environments using 6 month soil incubations. It also analyses the changes in distribution of the different GDGTs to examine possible differential degradation velocities. While the subject of the article is of interest to the community I find that major flaws in the experimental setup and misleading interpretation of the results call for a major re-writing of the manuscript, please find detailed comments below:

Abstract Line 4-7 as already pointed out by rev. 1 your experiment does not allow you to asses suitability of brGDGTs as terrestrial tracers, it only allows you to determine degradation (or lack of thereof) in waterlocked soil conditions and then only for 152 days which is too short as shown by many previous studies (some of them unquoted, such as Kim et al. 2010, Huguet et al. 2008).

Line 10 There is nothing in your setup that allows you to measure production, especially since it is too short compared to previously estimated decadal turning times (e.g. Weijers et al. 2010). I would rather say that the lack of changes indicates there is no significant changes in brGDGTs in the water column that may have settled in your soil, pointing to a lack of water column production and a cessation of soil production when water locked.

Lines 13-15. That you see an increase in isoGDGTs does not necessarily indicate production. As you did not filter incubation water you may be getting additional setttling form your water to the soil. Which brings me to a crucial point you treat the soil as an inert part of your experiment when in fact it plays a major role in both signal storage/preservation and transport. While sandy loam is quite sandy it still contains clays that will certainly store lipids very efficiently and may provide a matrix preservation effect, which has already been shown by Huguet et al. 2008 . Moreover when transporting your soil to the river the structure and grain size distribution will be crucial as bigger particles will be transported first and allow faster degradation (but of course also settle first probably not reaching the coastal area). Lines 20-21. It may also show that the archaea living in the water column have a different distribution or that those in soil and/or that some iso-GDGTs degrade faster than others or are released from soil matrix faster.

1. Introduction: Line 29-pg11572. Here you should introduce papaers by Smith et al. and Fietz et al. That talk about the adequacy of the BIT.

Lines 20-21-pg11572. Well not sure about that Harvey and white worked both bacterial lipids and presence of fossil intact lipids has already been shown...certainly needs to be toned down.

Line 5-7 –pg 11573 well the fact that there is little primary productions does not necessarily hinder heterotrophs growth so I would be careful here.

2. Material and Methods:

Line 10- I fail to see how you obtained enough material filtering 10 L of water when in all studies much higher amounts need to be sampled.

Also when filtering with a 0.7um filter you are really collecting SPM but not free living organisms which has been shown to grossly underestimate IPL presence (Ingalls et al. 2011). As you are comparing your values with the soil in the incubations (and not with 0.7 filters of the incubation water) in the soil samples will then contain all fractions that have reached the soil during your experiment and not only SPM. Meaning your experiments have two major flaws you did not estimate the abundance of lipids accurately in your water samples and you are not comparing equal fractions when analyzing water SPM and soil sample.

Also on experimental setup you never say how many water samples you analyzed? Lines 18-20 I wonder how well this mimics an in situ process? In a natural environment very small soild particles will be carried into a river and then down the river at a very high water/sediment ratio with constant movement (not sure the shaker mimics that) and with varying rates of exposure to sun and oxygen.

Line 10-Pg 11576-You cannot calculate a deviation with 2 samples.

3. Results and discussion

3.1.BrGDGTs

Lines 7-9 I think the large spread between the 2 replicates hints at a heterogeneous soil sample and this should be discussed further and taken into account when interpreting the results.

Line 10-13-pg 11577. As they may be protected by a matrix and really a msall fraction of the soild would be exposed to the light (in contrast to SPM floating in a river) I don't think you can say they are not photo degraded.

Line 24-pg 11577. There has been a lot of debate on the liability of IPLs and this should be reflected here, but overall I think you cannot make the connection with higher

production so easily.

Lines 25-30-pg 11578 Your study is not comparable to that of Huguet et al. 2013, first they did not change the natural soil structure and conditions, secondly they used a much longer time span and third they did not see changes in abundance.

Line 6-pg79-remove but

Lines 7-10. I think my main concerns here are a)not all nutrients in soil will be released by adding water (and certainly the type of water will be crucial in that process), b) the different masses will contain nutrients (yes the distilled water too), and c)if you have no alive brGDGT producing organisms (as you seem to hint from your no production conclusion) the nutrient concentrations are not relevant.

3.2-iso

Lines26-2 pg 80: I strongly disagree with this statement firstly as pointed out before 0.7 will only give you SPM lipids and not free living organisms. Secondly 100 mL is very low a sample and could easily not be representative. And finally it is not relevant what concentration you had in soils but rather how it increased over time.

Lines 6-13 pg80: This is highly speculative as a) you cannot prove that the increase in crenarchaeol comes from production, b)It has been demonstrated that Thaumarchaeota favour low ammonia concentrations (e.g. Martens-Habbena et al. 2008), c)you did not measure nutrients in the water before or after.

Lines 15-19: I don't think you can deduce that from your data.

Lines 24-30-pg80: Nothing new here, as you pointed out in your introduction intact lipids degrade faster than core lipids however in all cases lipids even with a phosphor head group will take long to degrade (see previous degradation experiments!) especially in a water logged soil where potential degrading organisms may not be doing too well.

Lines 3-5 pg 81: You have no evidence to support this statement either measure intact lipids or remove sentence.

3.3-Distribution

Line 14-pg. 81: I would say worryingly, as if the biggest change is your control then there is a factor you are not considering or it is not the right control. I would certainly make a much bigger effort to explain this.

Line 26-81: I must agree with rev. 1 not detected does not mean not present.

Lines 1-4-pg82: with your data you can certainly not talk about rates of production.

Line 10 &15-pg 82: You cannot talk about production.

Lines 4-6-pg 83: Yet I would argue that hardly any production takes place, especially in soils and that the differences steam from the IPLs and CLP present in the water.

Lines 6-10-pg 83: highly speculative you have no data and there are no supporting publications on this.

Lines 14-21-pg 83: Well to have methanogens your soil should have been anoxic (which I doubt form a sandy loam), if your soil had been anoxic and water logged to start with your experimental setup would have been better and your result likely very different.

3.4. Comparing

Lines 4-11-Pg 84 To be honest with a river that is 150 km long there is no doubt that your soil and water samples are not representative, especially since you already have replicates that at heterogeneous so I think you should take this final section and use it to reinterpret your results in a more sensible and temperate manner.

4.Conclusions I think this section needs to be modified fully after major changes in the rest of the manuscript.

Interactive comment on Biogeosciences Discuss., 11, 11569, 2014.