

Interactive comment on “Ramped thermal analysis for isolating biologically meaningful soil organic matter fractions with distinct residence times” by Jonathan Sanderman and A. Stuart Grandy

Anonymous Referee #1

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General comments: soil-2019-44-manuscript-version1-1 The combination of ramped thermal oxidation with ^{14}C determination is a relatively novel approach to study organic matter fractions of different turnover times. Therefore, it is worth to be published in an international journal. Overall, I have problems with the presentation of approach and discussion of data. In my view the authors failed to relate their study to existing knowledge from thermal decomposition studies. This could be overcome by revising the text and involving more literature. Following this it should be stated clearly which results confirm former findings and which really add to the existing knowledge. Specific comments: Page:1 Ins 5 ff. I miss state of the art and objective in the beginning of abstract. What does "preliminary" mean in this context? How is "biologically mean-

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ingful" defined in this respect? In 10: Statement without prove up to here. What is the "activation energy of the fraction"? Not defined so far ... In 35: py-GC/MS data only can show shifts in the composition of volatilized and ionized fraction - not in the bulk chemistry. Be more precise in descriptions In 16: "microbial derived" is not a chemical compound class (e.g., polysaccharides and lipids as well can be derived from microbes, e.g. the latter from microbial cell walls). Ins 17-20: This appears speculative to me (at least not logically following the previous statements). Ins 23-24: What is "multifaceted organic chemistries"? In my view there is only the (one) organic chemistry as a scientific discipline. Page:4 In 110: How was the temperature set up? How was the transfer from Py-oven to GC capillary? Important: Has cooling & condensation been avoided? Describe ionization mode (at least briefly). How did you distinguish between protein-derived and (other) N-bearing compounds? Page:6 In 165: I think this is incorrect; compounds cannot dominate a pyrolysis temperature. You mean the compounds released at a certain temperature. Page:7 Ins 185-190: This in fact is not new: analytical pyrolysis (i.e. the combination of in-source pyrolysis with field ionization mass spectrometry) has shown this many years ago and for numerous samples (published in many articles and reviews). Ins 194-195: This is unproven statement in my view. Which new insight in detail are you referring to? Ins 196-200: All this (esp. thermal lability and possible origin of carbohydrates) in fact is not new but has been published many times years ago. Ins: 205 ff. From these analyses no indication of any association of compounds with minerals can be made. In contrast to author's discussion higher temperatures for thermal release may origin from binding to minerals. But whether this or chemical complexity of rather intact lignin is the reason for the higher activation energy cannot be judged from the present results. Thus, this discussion should be tuned down or better arguments for the one or the other point of view should be provided. In 209: This has been reviewed already in 2000 (Biol Fertil Soils, 30:399–432). Page:8 This discussion is misleading by restricting to humic substances fractions and the py-GC/MS technique which is not the very best for detecting native N-heterocycles. For discussing this issue first carefully study "Advances in understanding organic nitrogen

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chemistry in soils using state-of-the-art analytical techniques". *Advances in Agronomy* 119 (2013) 83-151 Overall this whole sections is not convincing (only 1 reference for "now considered"). In 235: "aromatic" is not an origin but a chemical structure property. Page:9 Ins 245-254: None of this is a conclusion. It is all summary. Either write true conclusions or omit this whole section.

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