

Interactive comment on “Fire-derived organic carbon turnover in soils on a centennial scale” by N. Singh et al.

Anonymous Referee #1

Received and published: 30 January 2012

The paper calculated turnover rates of fire-derived organic carbon in soil. The topic is interesting and pyrogenic carbon has received wide attention in recent years. This topic is appropriate for the journal *Biogeosciences*. Unfortunately, the manuscript deals with an inappropriate combination of available pyC data from fires and charcoal-making (which should not be compared), uses a meta-analysis that combines these studies when too few studies are available (15 studies that vary too widely), utilizes short-term observations for long-term predictions, and does so with an inappropriate decomposition model. I do not expect that the manuscript can be sufficiently revised to be acceptable for publication, but requires a different calculation approach for which the data are not available (also as per the authors online response). More detailed comments below.

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A meta-analysis seems to be the wrong approach in order to arrive at a meaningful conclusion, since not enough studies over a sufficient period of time are available. If this is possible in the future, it may lead to interpretable data.

Meta-analyses are useful in situations where effects can be compared using a sufficient number of different boundary conditions with sufficient information available. This is clearly not the case in this paper ($n=15$ studies), and can result in misleading conclusions.

The focus of the manuscript appears to be pyrogenic carbon from fires, as per the title and introduction. But many of the papers used in the manuscript are from charcoal that was made in experimental furnaces under very different conditions than open wildfire. The very concept of a black carbon continuum was championed by one of the co-authors, Dr Schmidt. This central premise that was published ten years ago, should be fully incorporated here, and will lead to a very different treatment of the subject. Two conclusions may emerge from this evidence: (1) data from charcoal made under exclusion of air do not necessarily allow insights into stability of pyC from vegetation fires. (2) insight from pyC generated under wildfire conditions may not allow conclusions about black carbon made under exclusion of air (often now termed ‘biochar’), but may be an underestimate of its turnover time.

According to several cited papers, and basic principles known from charcoal as a fuel, pyC may consist not of one homogeneous material, but of aromatic carbons, partially interlinked in clusters of varying sizes (see Knicker, 2007, *Biogeochemistry*), and very labile organic materials partially from uncombusted remnants of carbohydrates as well as from condensates that have been described as explicitly promoting microbial activity and that can be metabolized very easily (Steiner et al., 2009, *Pedobiology*). Due to this evidence, it is not possible to use first-order kinetics for the calculation of turnover times and reach a conservative estimate of short turnover times. This is pointed out well by Jenz Leifeld in his comment to this paper on the *Biogeosciences Discussions* webpage. If one wants to have a conservative lower estimate of turnover times, this

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may be appropriate. But in the case of the reviewed paper, this is not possible. (see additional comments by Leifeld that should be considered) In principle, an extrapolation from incubation studies that were conducted over a period of weeks or months to a calculation of turnover that supposedly is centuries or more, is inherently flawed, irrespective of the peculiar case of PyC chemistry, unless one wants to give a lower boundary of turnover times (as mentioned above).

The authors should also be aware of some interesting work that shows indeed that some portion of pyC may be inorganic C, which would dissolve as carbonates and depending on the pH, lead to evolution of the CO₂aq. Another problem with the treatment of the data is that several cited papers arrive at loss rates of pyC that do not distinguish between leaching, erosion, mineralization and decomposition and subsequent stabilization of decomposition products in soil (only additional input is mentioned in the manuscript as a confounding factor). This applies to virtually all field data that are used in the analysis. For obvious reasons, it is not possible to calculate turnover rates from such studies that do not distinguish between these processes,- looking at the data table, those are studies that have particularly high reported loss rates. If one excludes those data, the already small data set becomes even smaller (and with 15 studies it was too small to begin with). The check mentioned on pages 12183-12184 to compare field and laboratory data is not appropriate due to the low number of studies and the fact that typically all laboratory experiments are also short-term and all field data are also longer-term (comparatively to lab data), which confounds the comparison.

Most of the data on pyC in soils comes from Australia,- curiously, none of them are cited when the authors refer to pyC accumulation in soil, but only refer to analyses of US, European and African soils (Bird et al/Schmidt et al/Skjemstad et al 2002), for which the database is very scant. This makes the reader suspicious that not enough care has been taken to mine the available data for their analysis.

Some more care is required to accurately cite the papers that they do reference: page 12181 for example cites four papers with a range of turnover times, which seem to lie

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at greater turnover than the 100-1000 years that are cited here.

On page 12186, the arguments are not very convincing that mineralization of PyC in sand is slower because there are fewer or less complex communities of microorganisms than in soil (with presumably finer texture). That goes against our understanding of decomposition and soil organic matter dynamics. I assume that the differences in turnover time are an artefact of the small number of observations, where many other factors may have large influence over turnover time (such as temperature of incubation, temperature of burning etc.).

The authors are encouraged to consider the review by Spokas (2010 in Carbon Management), which they may not have been aware of, to compare their data with (to my knowledge the only paper that reviews the mineralization data available in the literature to arrive at conclusions about PyC turnover or half life). Also several other key references on pyC stability have not been included, which is curious as the authors attempted to perform a meta-analysis (e.g., Gavin et al 2003, Ecol; Cheng et al., 2008 JGeophysRes; Bruun et al 2011 Biomass Bioenergy).

As the authors correctly point out on page 12183, even with the incorrectly calculated turnover times "PyC remains more stable than all known plant-derived organic compound classes in soil", which is the correct comparison to make, rather than with total organic matter (as posed in the main conclusion and abstract), as the soil may contain large percentages of black carbon to begin with and may therefore have long turnover times for that reason.

A word of caution when drawing conclusions from the paper by Ohlson et al (as done on page 12187): that paper only assessed macroscopic charcoal that could be manually removed (when we know that PyC is mainly present below the size that can be removed, Skjemstad et al 1996). It is not useful to draw any conclusions about PyC stability from that paper.