

## ***Interactive comment on “Black (pyrogenic) carbon in boreal forests: a synthesis of current knowledge and uncertainties” by C. M. Preston and M. W. I. Schmidt***

**Anonymous Referee #3**

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Review of manuscript bgd-2005-0100 “Black (pyrogenic) carbon in boreal forests: a synthesis of current knowledge and uncertainties” by Preston and Schmidt

This review aims to review the role of pyrogenic carbon (PyC) in the C cycle of boreal regions as the C cycle in these regions is thought to be strongly influenced by fire. The authors point out that large gaps exist in basic information that would aid in defining the role of PyC in the C cycle of boreal regions such as the choice (and limitations) of analytical techniques, production rates and stocks of PyC in boreal forests, lack of data on aerosol production from boreal forest wildfires and data on the loss, transport and turnover time of PyC.

However, the authors fail to live up to the expectations that are conveyed by the ti-

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tle and abstract. This review, while providing a good literature review on the latest charcoal studies, current analytical techniques, stocks, fluxes, and technical definitions commonly used in the literature, it is a) too long, b) organized in a rather cumbersome way such that several sections are repetitive and stand-alone paragraphs that seem to be pasted in the text without much context and c) not focusing primarily on boreal regions as foreshadowed in the title and abstract. Instead, it just highlights that gaps seem to “specifically exist in the boreal regions”; however, most studies quoted in this paper are from temperate and even tropical regions. Thus, throughout the manuscript, the emphasis on boreal regions is rather forced (by highlighting the gaps of knowledge in boreal regions at the end of many sections) and not a central point of data presentation in the manuscript. Thus, my suggestion would be to open up the manuscript to other areas of the world and have a more general review, which shouldn't be too difficult to achieve as most of the manuscript is already quoting studies from all over the world and only the title and abstract are somewhat misleading in the focal point of the manuscript.

Furthermore, the discussion of analytical techniques becomes confusing for the reader who is not familiar with the pros and cons of the individual methods, particularly when the authors sometimes recite results from studies that were obtained by different methods but compare them as they were achieved by one and the same method. While they make a strong point that the choice of method can highly influence what kind of information is obtained in terms of quantity of PyC, they do not apply this criticism when comparing data from studies, obtained by very different methods. They also fail to go a step beyond reciting what is already been published in the literature about the different techniques. For example, they give several example for the PAH method that appear to show that this method seems to extract particularly industrially-derived PyC sources. Nonetheless, the authors appear to favour this technique for the quantification of PyC per se. A more critical evaluation would be useful.

Finally, there are a few careless mistakes such as misquoting references and lack of a

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broad spectrum of references and instead relying on one study, which are referred to below.

My recommendation would be that this manuscript deserves publication after major reviews, mainly broadening its emphasis from the boreal to the worldwide region that are influenced by fire and trying to have a more objective view when it comes to evaluating techniques and quoting studies.

Specific comments: Line 26: “large gaps in the basic information” are not only limited to the boreal regions but are an issue for all areas in the world that are highly influenced by fire. Line 29: techniques need to include also physical separation (as mentioned by the authors on p. 7). Line 30: what do the authors mean with “PyC continuum”? Line 31: here, boreal peatlands are emphasized but line 34 and 35 appear to stress boreal forests - thus, it is not even clear which boreal ecosystem the authors would focus their review on. Line 41: the idea of consumption of pyC by subsequent fires is highlighted here but is sadly not really expanded in much detail in the text in terms of what temperature and time would be required to achieve this consumption. Line 53: the Hicks et al. reference does not say that fire is a “driver of ecosystem processes and the C cycle” but that fire affects the C cycle. This is a big difference! Line 76: explain the whole “continuum” aspect better: does that imply that there are no distinct groups, yet still a considerable effort is made to distinguish one from the other by different terminologies? ? Line 78: why “graphite”? graphite is highly metamorphosed organic C and to my knowledge, there is no firm evidence that indeed it had originally formed by pyrogenic processes. Line 86-89: this paragraph is highly misleading as it implies that this review will be focusing on formation, characteristics, stocks and losses of PyC in boreal regions only. Line 131-133: to my knowledge, most analytical techniques that aim to analyse for PyC do not aim to differentiate inorganic C - usually, acid treatment prior to analyses is used to remove this fraction but it is usually not specifically quantified or characterized. Line 158-163: I find this section highly confusing (particularly when being referred to before as PyC as a “continuum”) Line 185: authors stress “boreal

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wildfires” but then quote studies from slash-and-burn in Virginia, burning in Amazonia, burning in a temperate deciduous forest, and from crownfires in Yellowstone National Park! Line 214: not clear what are high and low values - needs explanation. Line 224: so what are the consequences from this statement???

Line 225-227: discuss conversion rates of biomass obtained by resistance to thermal oxidation but before authors say that methods may vary by a factor of two or more, so how can these data reliably used???

Line 252: “less detailed”? still 4 pages! Line 287: discussion of BC emission in Asia. Line 305: what are the “total C trends”? line 368: this range (63-321 ug kg<sup>-1</sup>) could mean anything and it is not giving any useful information that Wilke and Amelung found similar values. Line 381: seems to imply that PAH is particularly good for industrially-derived combustion products Line 391-392: this is an important point (formation of melanoidins) and should be discussed as a potential source of error in determining PyC. Line 426: this value (126ppm) does not seem to be consistent throughout the literature and other studies quote 131 ppm - authors should aim to be inclusive. Line 426-429: however, authors should also note that lots of problems exist with 15N-NMR, particularly with respect to observability and therefore the results need to be discussed critically! Line 443: this is clearly not boreal! Line 488: just mentioning studies in boreal regions is not sufficient to make this review central to boreal studies line 493: repetitive - as been discussed before in text. Line 497-499: but only in productive ecosystems! Line 521: less sensitive Bloch decay? I don't understand, before it was argued that Bloch decay needs to be employed to correct for the underestimation of aryl C, thus appears to be more sensitive as it “sees” the actual C and not the H associated with the C (as in CP). Line 528-529: repetitive Line 546: two studies only? This is hard to believe and I'm certainly aware of more than just two studies on that subject! Line 572: why do authors say that charcoal proportion would have been higher if small particles if smaller particles were isolated, but the study by Skjemstad et al. quoted there, did exactly that! Line 617: needs reference to back up “presumed high production of BC”. Line 620-626: wouldn't it be useful to find more studies that use 14C dating here? Line 631-63: this process/hypothesis needs to be explained more

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to convince (at what temperature, over what time span, are there experimental data?) Line 642: study from 1966 on incubation of artificial graphite? If certainly biological oxidation of charcoal is occurring than wouldn't there be lots more recent studies? Line 661: what is "hardly any"? Line 691: Chcarcoal production occurs within organic horizons? How so? Line 708: wouldn't that migration rate heavily influenced by the soil type (clay content, water holding capacity), vegetation and climate? Line 727: I don't see the necessity for this section. Line 742: why black shale??? Black shale is not a material that is derived by pyrogenic processes! Looks like authors are comparing apples with oranges here. Line 746: if lifetime of char is 8000 to 80000 years, then why does it occur in Devonian sediments now? Line 778: exactly! Please use this in your critical evaluation of the studies that you discuss. Line 782-785: repetitive Line 821: repetitive Line 827: the study by Bird et al is about the only study I know that quotes such short MRTs for BC in tropical savannas - authors need to be more inclusive and should also refer to other studies that report much older MRTs; otherwise, it appears like charcoal turns over rapidly in tropical savanna but as a more than 10fold greater residence time in other areas of the world (refere also lines 838). Line 847: explain what you mean by "loss" here? Line 849: wouldn't the influence of charcoal on soil function be important to know for all regions and not just boreal forest soils? Implies that the effect of carcoal on soil function is well understood for other areas. Line 861-865: needs references!!! Line 933: what do you mean by "char height"? Line 893: can you really compare "activated char" with naturally-produced char? Line 904: shouldn't you also quote the work by Lehmann et al. here?

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