

Interactive comment on “An open source database for the synthesis of soil radiocarbon data: ISRaD version 1.0” by Corey R. Lawrence et al.

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Received and published: 10 July 2019

There is a great deal of enthusiasm in the growing soil radiocarbon community for a tool that makes existing data more accessible. This paper presents the latest and most significant effort to develop such a tool, ISRaD, authored by a group of leaders in the field. Overall, this paper is ideally suited to this journal. Its imperfections describe well the challenges faced in this field of research, and the reasons why the development of this database, observable at major conferences for a few years, has been such a substantial effort.

Below I document a number of areas of improvement for the manuscript and the database package. What I say less about is that the work-to-date and overall quality

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of the manuscript here are very good and will generate great benefits through ongoing focus on the role of soil radiocarbon in managing a key part of the Earth's C cycle. The 8500 measurements included so far, valued at roughly \$4.5 million USD, describe the scale of the problem, and the need for further progress in the development of a structured database to support this field of research. Downloading and browsing the database tables also emphasises this. However, there are significant opportunities to improve. These boil down to two things:

1. Because the ISRaD package was not on the CRAN repository for R packages as suggested in the manuscript during this review/discussion process, and the active Github site didn't seem to me to provide an easy/clear substitute, there is a little less checking and transparency than I would ideally like to see given the substantial effort that has gone into this work. This is most evident in that there is nothing resembling a set of worked examples that demonstrate use cases for the database.
2. The presentation appears to have an overemphasis on soil fractionation data and underemphasis on time series, other constraints enabled by the database, including particularly site level C flows such as NPP and soil respiration. This appears to be related to a view of a paradigm shift in the controls on soil organic matter dynamics that I find biased toward recent synthesis and in surprising ignorance of key original work decades earlier. A consequence of this is that the role of early radiocarbon work, that was often more thoughtful than recent studies (perhaps due to the higher relative cost of analysis) is underemphasised.

Overall the concerns related to (2) are significant, because unintentional biases in how scientists or teams of scientists were thinking when methods or datasets are created, selected or pruned can have long-lasting effects to obscure or wall off promising routes forward. Documentation via the scientific literature represents the last chance to correct or clarify any biases.

I provide additional detail and discussion in relation to particular lines in the manuscript.

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L39-41 The sentence spanning these lines is problematic for several reasons. First, on the face of it, its assertion regarding bulk carbon appears to me to be disproven by Baisden et al (2001; 2013a, 2013b). This may simply be a matter of interpretation however – I also have trouble linking this sentence to what follows it, given what is possible in quantifying the stabilisation and turnover of carbon. Second, the confusion I see in this sentence may perhaps lie in what is meant by the word, “predict.” To play devil’s advocate on this point, I’ll suggest that far more would be known and quantified if, since 2000, the field had followed the simple process of collecting and running time series bulk samples, and using math to separately measure the size and turnover rates of pools. In contrast, it doesn’t seem that ongoing efforts to chemically and/or physically fractionate soil have led to clarity or application.

L42 “mean ages and cycling rates” are duplicative, since rate is the inverse of age given simple pools. Also, why imply “mean” ages? Mean ages, especially when used across distinct pools, or without time series data imply considerable risks of biased results (Baisden et al 2013a) so I would like to see the community to be careful and precise in the use of this type of terminology.

L45 The introduction of transit time as a completely different measure is confusing. A great deal of work has included an understanding of transit, for example by explicitly attempting to model transport processes within soil. It may seem pedantic, but it quite important to understand that “transit” times are useful in systems where transport is important as a process. This distinction should either be left vague, noting that the measures differ somewhat, or be better expanded to recognise work focussed on transport. Useful examples include Elzein and Balesdent 1995, Baisden et al 2002; Baisden and Parfitt 2007, Sanderman et al 2008, and Jenkinson and Coleman 2008.

L48-49 This statement appears incorrect. It certainly has been shown mathematically tractable to separate distinct ‘pools’ without physically or chemically separating soil. For grassland soils, the comparisons in Baisden et al 2002, and further work in Baisden et al 2013 and 2013a make a fairly clear mathematical separation with time series sam-

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ples is more efficient. Undoubtedly options may vary on this topic, but at a minimum the case for mathematical separation based on bulk samples has to be acknowledged as valid strategy. This is particularly true if total throughput of C through the ecosystems can be understood (Gaudinski et al 2000; Sierra et al 2012; Baisden and Keller 2013).

L51-52 The references given for the shifting view of controls on soil organic matter dynamics give an unfair impression of recent progress, using papers that do present useful recent synthesis. It seems remiss not to include earlier references, or at least Oades 1989. It would be preferable to include Golchin et al 1996 as well.

L57-59 It might be more accurate to say there are either one or three things here, but not two? If there is a ‘fast’ pool, and a slow ‘pool’ then different processes govern the turnover of each, so the two processes each need parameters. But it is equally important that the process of partitioning carbon flows into soil between the two pools be understood. Yet, I could also see another point of view, that there are typically more than two pools recognised in soil, so perhaps an understanding of partitioning only is intended here? Please clarify.

L 68 It seems slightly odd not to have pioneering or earlier exemplars of density fraction in this list. Various students of Oades, and particular series of papers published in 1995-7 by Golchin. Keep in mind that many of these methods were not developed specifically for radiocarbon.

L84 It is interesting here to see version 1.0 (Sierra et al 2012) rather than version 1.1 (Sierra et al 2014) of SoilR referenced. Please see the note below regarding L102 about an interface to soilR. If the goal of the database is to allow improved testing of hypotheses representing understanding of soil carbon dynamics, it seems SoilR should provide an ideal mechanism for implementation. It would be good to see more clarity of thought on achieving this, including a reference to the later version of SoilR.

L88-89 It may be worthwhile considering earlier references to DOM such as Sanderman et al 2008. I say this in part, because what is said in this paper may guide the

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use of the database, and it would be worrisome to neglect early studies containing compelling radiocarbon results.

L102 Here again, I'd propose there is a collective forgetfulness of what was well established in the literature by the 1990s in terms of paradigms of soil organic matter dynamics. These have been reinforced by review and synthesis in recent decades, but this is not a reason to neglect early radiocarbon work that had already largely incorporated the paradigms promoted in this introduction. Therefore, it is odd to see early work that established overall constraint of carbon dynamics in well-studied systems neglected here. Such work can provide useful examples of how to construct strategies for constraining carbon dynamics. The obvious examples driven entirely by radiocarbon are Gaudinski et al 2000 (in relation to followup by Sierra et al 2012) and the set of work in Baisden et al 2002, 2002a, 2003. A second issue the lack of reference to or inclusion of literature using tracer carbon, or natural abundance stable C isotope ratios. Finally, a weakness in papers on recent paradigms is the importance of closing the partitioning and turnover of soil carbon by constraining the overall flow of C through the system via NPP or respiration. This is a strength of SoilR (Sierra et al 2014) so, as noted above, I would like to encourage the authors to consider what link might be made between these two R packages. This is covered to some degree in L225-231 but not with explanation of the value of or rationale for such constraints.

L131 It would be good to clarify here that the site accessible via the soilradiocarbon.org address does not appear to have an R-shiny interface or some other "web interface" to the data running. Either the words "web interface" should be changed to "web site", or an exact address to a "web interface" should be provided.

L270-279 It is good to see these items related to density fractionation included specifically. However, does it make sense to include/explain these stored values but not include the degree to which the sonication method has enabled isolation of occluded vs free light fraction, again originally detailed by the Golchin work to adapt density fractionation to the paradigms the authors promoted at the beginning of this manuscript's

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introduction?

L320 The web interface again appears to be a regular website rather than a web database interface?

L322 This web address only goes through with http:// and not with https://

L332 The ISRaD package was not available at cran.org as implied in this text. The Github version indicates changes. Although these changes are probably minor I was disappointed to find that there was not a version tagged to support this review process.

L501 Here again the "web interface" is mentioned. It seems worth noting here that this link appears to lead to a fairly standard website with a static file download for a database table, rather than an interface to the database.

What's missing? There seems to be neither an accounting of the spread of categories or types the data already in the database represent, or what weaknesses (gaps) can be described. Similarly, there is a rather technical description of data entry, but not a description of how substantial historic datasets might be brought into the database. An additional but admittedly problematic question is whether the extent of available published data not in the database can be better quantified and described. I encourage some discussion of these opportunities for improvement.

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