(Senesi and Plaza, 2007)Interactive comment on "Reviews and syntheses: The mechanisms underlying carbon storage in soil" *by* Isabelle Basile-Doelsch et al.

Anonymous Referee #1

Authors present an interesting review of mechanisms underlying carbon storage. I think it is a potentially interesing paper that however still requires considerable changes before I could recommend it for publication in Biogeosciences.

The main shortcomings that I currently see is that I miss appropriate referencing (detailed below)

We have provided additional references where needed (see below)

and I find the text somewhat inconsistent, e.g. in that some conclusions are not supported by sufficient parts of main text (more below)

We agree with this comment. Therefore, we have rephrased the conclusion

Some parts of text seem to apply only to cropland but from title/abstract/introduction it is not clear whether authors aim at all soils or only cropland or agricultural soil including grasslands.

Indeed, many examples refer to cropland soils because they are more documented in the literature. Moreover, most of the potential for a C stock increase is in arable soils because their initial soil C stocks are low. However, the mechanisms that control OM dynamics are largely the same for all soils, whatever the land-use. We believe that this review devoted to generic mechanisms applies to all soils. We thus feel that a title including "agricultural soils" would not be appropriate.

In general, manuscript could benefit from proofreading by a native speaker – I tried to suggest improvements myself but I am not a native speaker either.

We truly thank the refere#1 and took all the suggestions into account and had a native English-speaking scientific translator proofread it as well (added in the ackowledgments).

The overall structure is sometimes confusing, linking/aligning different parts of the text needs to be improved. I'd like to suggest that the structure of manuscript would maybe benefit from a section or a group of sections on "outputs" (following the logic of figure 2). this could include erosion as well as mineralization (where related terminology could be discussed maybe more briefly than now), followed by (de)stabilization mechanisms (which influence mineralization rate),

We changed the structure and created an "ouputs" section as proposed (including erosion, DOC outputs and mineralization). However, to be consistent with fig. 2, stabilization mechanisms were not included at the end of the section as proposed by the referee.

this could again be referred to in the section about non-linear processes (Table 1) and factors (Table 2) influencing mineralization/mean residence time.

See below

In general, I like most Figures and Tables proposed by authors.

Thanks!

I struggle a bit with Figure 5 and 6. In Figure 6, I think the text nor the caption does not explain well enough how were these data obtained.

The data come from the database available via the Balesdent et al. 2018 Nature paper : Jérôme Balesdent, Isabelle Basile-Doelsch, Joël Chadoeuf, Sophie Cornu, Delphine Derrien, Zuzana Fekiacova, Christine Hatté, Carbon transfer from atmosphere to deep soil layers over the last 50 years, Nature, 559 599-602, 2018. The paper is cited and all the data are available online.

The caption of figure 6 was rephrased to give more details. We added in the legend of the figure *"(Based on data from a meta-analyse of d13C studies Balesdent et al. (2018))."*

Also the figure is a bit overwhelming because it shows differences between grassland, forest and cropland but this aspect is not really leveraged in the main text so I suggest that authors think about what is the main message of this figure and either keep just one land use or discuss the differences more.

We agree that this may be confusing. In fact the main objective of this section is to consider soil C dynamics in deep soil layers. For clarification, we have changed the title of the sub-section, which is now:

Renewal rates at the soil profile scale: deep C dynamics

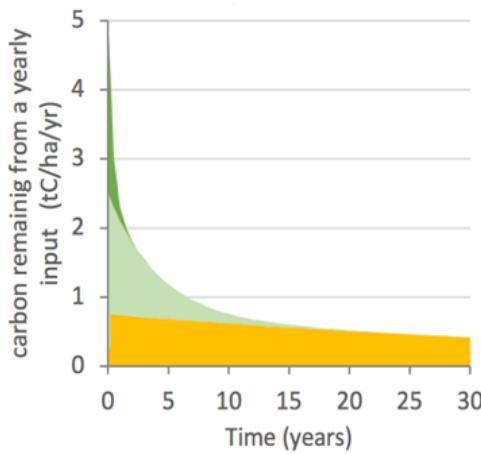
Authors offer some interesting conclusions but some of them are introduced for the first time in the Conclusions section and they are not well supported by the main text. E.g. in second sentence of Conclusions, authors mention that carbon inputs in croplands are often higher than in grasslands and forests, but main text (2.1.1) contains only discussion of a hypothetical example of C input calculation for a crop of certain yield. Discussion of the range of yields (and inputs) observed in croplands as well as a comparison with C inputs estimated for grasslands and forests is missing. Therefore I would recommend authors to carefully review the whole conclusions section and see if the main text is supporting/explaining well all they are referring to.

We agree with this comment. The conclusion was completely rephrased.

I struggle with Figure 5, especially the table part, was the last column calculated as "pool size after 30 years of input fluxes from first column with MRT in second column? Does this representation assume any fluxes between the pools? In the figure why does the slow pool first seem to be lower than 0.75 tC/ha/yr

Details have been added in the caption and the error on the graphic changed to 0.75

Figure 5: Left side: Simplified kinetic representation of the fate of plant organic inputs in soil (single input event, here 5 tC/ha/year). The kinetics is divided into three mineralization phases: fast, intermediate and slow. Organic matter can be divided into three corresponding pools, the size of which is represented in the figure by the colored areas. The sizes of the pools inherited from the inputs at year 0 decreases progressively with their own kinetics over 30 years. Right

side: Pool sizes were calculated based on 30 years of yearly inputs (with fluxes between pools). The numerical values are typical of the 0-30 cm layer of temperate crops. Old carbon represents organic material inherited from a distant past.

In Table 2, I miss effects of aggregation and erosion

In Table 2, we added:

↗	Water content (except pO₂)	↗ Microbial growth and mobility ↘ Sorption ↗ Diffusion and transfer ↗↘ Aggregation ↘↗ Erosion	↘ ↗↘ ↗↘	Yes, all mechanisms combined

↗	Mineralogy : - Short range order phases - Al, Fe, Ca complexes - 2:1 phyllosilicates	↗ Coprecipitation ↗ Complexation ↗ Sorption ↘ Transfer ↗ Aggregation	↗↗ ↗ ↗ ↗ ↗	No No No No

In main text, I am missing also a part discussing the effect of nutrient availability/stoichiometry on SOM dynamics, maybe this could be discussed in section about chemical nature of OM

We agree with referee#1. However, instead of adding this information in the section devoted to the nature of OM, we added it in section 4 (Control of C turnover times in soil: biotic and abiotic factors):

The availability and abundance of N, P and S *elements has various and complex effects. Organisms require specific ratios of C, N, S and P (i.e. stoichiometry) to survive and function optimally. For example, degradation of ligno-cellulosic debris (low nitrogen) is temporarily delayed in the absence of mineral nitrogen. Many other C, N and P stoichiometry effects are involved in priming, biotic interactions or carbon use efficiency (CUE) (Monzoni et al. 2012). C:N:P ratios ranging from 72:6:1 (topsoils) to 32:5:1 (pasture) have been reported. Soil vertical stratification C:N:P may, however, be important and impacts on the soil stoichiometry have also been reported following land-use shifts. Regarding the additional*

amount of nutrients required to store the quantity of carbon targeted by the 4/1000 initiative, it has been suggested N and P can be provided under current fertilization rates by reducing nutrient losses via improved management practices that include cover crops, fertilizer incorporation, etc (Bertrand et al., 2019).

In the introduction, I think authors should try to report on the on-going discussion of soil C sequestration potential in a more balanced way (since authors are not submitting an opinion paper but a review paper), see my suggestions below.

We agree with this suggestion. Some parts of the introduction have been rephrased (see below)

INTRODUCTION

Reviewer 2 has provided a very positive comment on the introduction ("*I liked the introduction very much and have no issues with it*"). However, reviewer 1 has provided valuable suggestions to improve it. Therefore, the introduction was changed as follows:

Page 1 L32 Add also references to replies from camp number 1: (Minasny et al., 2018; Loisel et al., 2019)

Done. We have also added new references (Rumpel et al. 2019; Baveye and White, 2020)

L33 please explain what you mean by "most of the criticism is focused on the political opportunity of the initiative" and at the minimum provide references to support this statement so that the author(s) of such criticism are clear

One key criticism is that the 4 per 1000 initiative may be misleading for policy makers if it encourages them to delay unpopular decisions needed to transition to renewable forms of energy. We have completed the sentence and added references to make it clearer:

Part of the criticism is focused on the political opportunity of the initiative as it may delay decision making on unpopular initiatives needed for the transition to renewable forms of energy (Baveye et al., 2018; Baveye and White, 2020), as well as on the calculation itself, which is based on several assumptions, some of which are actually highly debatable (e.g. the soil depth to be considered).

L38-39 this sentence is rather vague and "recent findings are not always considered" and "misuse of the concepts" are strong statements, please support them by reference to paper(s) that expand further how Minasny et al. is guilty of either of those. Also please provide more details or reference to your example "e.g. confusion between soil C equilibrium and soil C saturation" this way it may not be clear what you mean.

We agree with this comment. As the second part of the sentence is not really addressed in the paper, we have deleted it. This section was rephrased as follows:

Other comments or questions have been more related to the processes underlying soil C storage: is there an upper limit to C storage in soils? What is the expected residence time of this carbon after its incorporation in soils? Is it possible to store more carbon without additional N and P inputs? Although substantial scientific knowledge exists on soil carbon dynamics, some uncertainty remains on these questions.

L40-L41 "Moreover, current soil C stocks are spatially highly variable, and factors that could explain this variability are not fully understood, although it would be essential to clarify this aspect prior to any attempt to increase soil C stocks." This is a very strong opinion and it should be made clear that this is a point of view of authors or only part of the "critical" part of scientific community. Possibly authors should also voice the opposing "camp" who think that waiting until "we understand it fully" is definitely not required, because they think that the gathered evidence is significant enough to take action. I understand authors wanted to explain why their contribution is needed but the danger of writing introductions in reviews like this in the "we still know too little to take any action" man- ner is that the people outside science read this and interpret this as a consensus of a scientic community

We agree that this sentence may be misinterpreted. We have rephrased this section as follows:

Moreover, current soil C stocks are spatially highly variable, and factors that could explain this variability are not fully understood, although more knowledge in this area would be helpful to design soil C storage strategies more efficiently.

L42-43 "Apart from practices based on additional C inputs..." – are all these references related to reduced tillage? If yes, provide a comparable number of references to first part of sentence about additional C inputs and also consider to start the sentence in a more balanced way, e.g. "Although consensus exists on practices based on additional C inputs, such as ... (refs), the outcome of other strategies, such as...., is more unclear. What other strategies other than reduced tillage do you think there is no consensus on, please be specific. What is described in 2.i (btw. Numbering of paragraphs should be double-checked) is true for carbon fluxes in agricultural soils, this should be either emphasized in the beginning of the paragraph or even the title of the paper or the content should be generalized to apply also to other ecosystems...

We agree that the list of references put too much focus on reduced tillage. We have rephrased this section according to the reviewer's suggestion:

Although there is consensus on practices that ensure additional C inputs in agricultural soils, such as exogenous organic matter input, moderate intensification of extensive grasslands, limiting residue exports, growing cover crops, promoting grass cover in vineyards, adopting agroforestry and growing hedges, etc., the outcomes of other strategies such as reduced tillage and liming are more unclear (Dignac et al., 2017).

The Numbering of paragraphs has been checked. The comment about the range of soil land use being considered was answered above.

PART "MAIN PROCESSES ..."

58 Lower plants (algae, mosses, lichens), microbial and faunal necromass also contribute to C input

The sentence was changed as follows:

"Organic matter entering the soil system is mainly synthesized by higher plants"

59 please define here well what you mean by "restitution", e.g. "surface retention or incorporation through tillage" because the term is later used a lot and is not a common one in biogeosciences

Restitution is used for "surface retention" (changed in the text when used). However, in this sentence, this detail is not essential and we simplified the sentence as follows:

It reaches the soil through the roots (dead roots or root exudates) or in the form of shoot litter and via unharvested aboveground plant parts.

60 this sentence is true only for cropland and ecosystems in which you assume constant standing plant biomass

We don't really understand this comment.

60 assuming you mean respiration of herbivores, is it better to say something like "losses by herbivory (production and respiration)" instead of "herbivore production and respiration" because this way it's not that clear which respiration you mean

We agree. We suggest the following sentence, which also takes the comment of reviewer 2 into account:

The soil carbon input flux is the net primary production of the ecosystem minus the exported crop production, losses from herbivory (production and respiration) and dissolved and particulate organic matter outputs.

Page 3 65 explain what do you mean by "estimators of soil restitution fluxes" would "Soil carbon input can be estimated based on" work too?

Yes, that works too and is even better, so we suggest changing the sentence to:

Estimators of soil restitution fluxes are often based on plant carbon allocation equations (allometric relationships) combined with carbon models.

66 the phrasing of the definition of harvest index (HI) is a bit unclear, would this work better? "(HI) is the percentage of aboveground net primary production that is harvested"

Yes that works. We suggest changing the sentence to:

The harvest index (HI) is the harvested proportion of net primary production from shoots, while the rest is returned to the soil.

67 please provide reference for these values

We have added a recent reference focusing on the harvest index (Fan et al., 2017 Harvest index-yield relationship for estimating crop residue in cold continental climates)

73 please provide reference for these values, missing bracket, also consider giving values as true ratios (0.1-0.3), in general please revise and be consistent about terms and corresponding values of ratios, proportion

We have added some references (Bolinder et al., 1997 Estimating shoot to root ratios and annual carbon inputs in soils for cereal crops; Poeplau and Katterer, 2017 Is soil texture a major controlling factor of root:shoot ratio in cereals?). The missing bracket was added, and the ratio was expressed as true ratio.

76 exsudates should be "exudates", please check throughout the manuscript (also figures and tables)

OK corrected

92 what do you mean by OM mobilization, priming?

Yes. The sentence was rephrased to make it clearer

The latter probably have a greater impact on carbon dynamics through their effects on microorganisms and on organic matter (OM) mobilization by their priming effect than a structural OM source has (Keiluweit et al., 2015).

What do 78 please explain the term "net root production" and its relationship with rhizodeposition and belowground net primary production, this formulation implies that rhizodeposition is not part of net root production

The sentence was changed as follows:

It is estimated that the rhizodeposition flux represents 20 to 50% of the net root production

82 better "aboveground restitution" instead of "soil returns"

OK, corrected

83 specify in brackets which assumption you used for calculating the rhizodeposition (to comply with the structure of sentence

OK "(31% of root production)" added:

For example, for a cereal producing 8.5 t dry matter (DM)/ha of grain, the aboveground restitution (stems, leaves) can be 7.5 t DM/ha (HI 53%) containing 450 mg/g of carbon, or 3.4 t C/ha. In addition, 1.3 t C/ha of roots (18% of aboveground production) and 0.4 t C/ha/year of rhizodeposition (31% of belowground production) are added.

86 maybe better "Chemical nature of soil carbon inputs" or "Chemical nature of soil organic matter inputs"

Yes, very good suggestion; "Chemical nature of soil organic matter inputs" preferred

Page 4 87 better "soil" than "ground"

OK changed

86-04 this whole section contains only one reference and that is after a sentence that needs the least support by reference

We added the following references in the section : (Kogel-Knabner, 2017) (Guiboileau et al., 2010) (Kallenbach et al., 2016) (Senesi and Plaza, 2007; Larney and Angers, 2012) (Lehmann et al., 2011)

93 maybe better "reallocates"

OK changed

95 specify how green manure differs from other plant inputs in terms of chemistry

OK , we propose the sentence:

Green manure derived from fresh cut living tissues differs from other plant inputs in this respect.

95 "same molecules" as what?

OK , we propose the sentence:

Microbial products are generally composed of the same molecules as plants products except for celluloses and lignins.

99 give examples of non-industrial organic waste products

OK, added

(e.g. poultry or cattle manure, pig slurry, etc.)

99-00 please rephrase, both these sentences carry the same information (that waste products are enriched in microbial products compared to plant matter) but they are connected with contrasting conjunction "while"

OK , we propose the sentence:

Non-industrial organic waste products (e.g. poultry or cattle manure, pig slurry, etc.) are composed of mixtures of plant or microbial molecules or their monomers, while compost and sewage sludge are enriched with microbial compounds (Senesi and Plaza, 2007; Larney and Angers, 2012).

01 consider using "In addition to fresh/recently-derived organic matter described above, byproducts of incomplete combustion may also enter the soil, such as"

OK changed

01-02 double check phrasing and categorization, this way it seems that pyrolysis products belong to incomplete combustion byproducts, also it is not clear what you mean by mentioning coal mines? If you mean coal entering the soil (which is totally valid) that is not a product of pyrolysis .

In geology, OM transformation into coal during diagenesis is controlled by an increase in pressure and temperature. In sedimentary layers, this transformation is carried out in the absence of oxygen. We infer that coal mine wastes can therefore be described as a pyrolysis process. We thus propose the following sentence:

In addition to the fresh/recently-derived organic matter described above, byproducts of incomplete combustion (plant coal from fires, soot from regional or global fallout), biomass pyrolysis products (terra preta, biochar (Lehmann et al., 2011), charcoal production site and coal mine wastes), and plastics may also be present.

03 you may want to mention here the term "geogenic carbon", under this term coal and rock-derived C can be grouped

We agree that coal mine waste and black shale are both geogenic C. However, the impacted soil surfaces differ markedly: very local and circumscribed areas in the case of mining waste versus entire regions in the case of black shale. We feel it is important to illustrate both cases.

We propose to change the sentence to:

Moreover, soils may contain geogenic organic carbon, particularly when the parent rocks are organically rich, such as black shale which blackens the soil color.

08 specify whether you mean soil fauna or consider also microbiota associated with large herbivore

We propose to change the sentence to:

Biochemical reactions that occur during OM decomposition are mainly induced by microorganisms (fungi and bacteria), whether they are soilborne or associated with fauna microbiota (soil fauna and herbivores).

14 whose stability is altered?

We did not change the sentence, as it seems to us that it is clear

Digestion alters the chemical structure of OM: (i) by selective digestion of peptide compounds, which alters their stability

19 better "bacteria-feeding" instead of "bacterial"

OK changed

23 I recommend to use "belonging to" instead of "representing"

OK changed

24 I recommend switching the sentence structure of the part about fungi to match that about bacteria,.i.e. dozens of meters of filaments belonging to 1000 fungal species

OK changed. The new sentence is:

It is estimated that 1 g of soil can support up to 1 billion bacteria belonging to 1 million species (Gans et al., 2005), and dozens of meters of mycelial filaments belonging to 1000 fungal species (Bardgett et al., 2005; Buee et al., 2009).

Page 5 32 better "produced by" instead of "due to the activity of"

OK changed

36 leave out "carried out by", because depolymerization is a type of degradation reaction

OK changed

38 better "represent" instead of "form"

OK changed

45 better "electron acceptors" instead of "oxidation-reduction reactions"

OK changed

46 better "can" instead of "could"

OK changed

48-49 revise the logical structure of sentence, unexpected usage of "while" maybe mention more reasons why are the costs higher (transport of enzymes through membrane, loss/dilution of both enzymes and degradation products outside the cell. . .)

OK changed. The new sentence is:

The extracellular nature of reactions has several consequences. On the one hand biodegradation has a high energy cost for organisms (e.g. transport of enzymes through cell membrane) and cells have to invest C, N, P and S. While on the other hand some compounds escape from the cells and are diluted in the soil solution or adsorb to other organic or mineral compounds.

 51 add "with each other" before "to form"

OK changed.

Page 6 61 maybe better "taken up from" instead from "in"

OK changed.

66 maybe better "rate" than "performance"

OK changed.

60-67 can you provide some reference?

We added : Cotrufo et al. 2013, The Microbial Efficiency-Matrix Stabilization (MEMS) framework integrates plant litter decomposition with soil organic matter stabilization: do labile plant inputs form stable soil organic matter? Global Change Biology (2013) 19, 988–995, doi: 10.1111/gcb.12113

68 better "carbon use efficiency" than "efficiency of C use" to match the abbreviation

OK changed.

68 better just "is" instead of "can serve to estimate"

OK changed.

69 "material" "flux" can be left out, use "to be" instead of "at"

OK changed.

70 leave out "flux"

OK changed.

78-79 repetition of defining supramolecular structures (with line 51, page 5)

OK changed.

81 problematic usage of the word "source" (plants are the original source of the building blocks but these have to undergo microbial transformation), consider using "producers" instead

OK changed.

84 there is no section 1.4, check all similar references throughout the manuscript

OK changed. (appropriate section is 2.3)

85-89 provide reference(s)

The cited reference (Kleber et al. 2015) provides a comprehensive review in the section "1.4 Oxidation Enhances Solubility and Chemical Reactivity of Organic Compounds." We do not think that it is necessary to add more references.

Page 7 91 consider using "which contains" instead of "i.e."

OK changed.

In Figure 4, specify that you start off with 2-mm soil,

OK , <2 mm added in the legend.

also discuss

In the fig 4 caption, we added:

Organic matter makes up less than 10% of soil constituents. The stock of C in a soil is mainly made up of dead organic matter molecules mainly resulting from microorganisms activity.

93 say "fauna" instead of "microfauna" not to exclude mesofauna

OK changed.

94 reference to Figure 1 seems irrelevant

OK removed.

98 consider acknowledging that humification theory also worked with the contribution of decomposition products to OM pool (similar to progressive decomposition concept)

OK changed. The new sentence is:

This mechanism is at odds with the historical "humification" model, proposing the formation of "humic substances" by progressive condensation of plant molecules and their decomposition products into macromolecules.

04 consider leaving out "regarding some organic compounds"

OK removed.

08-09 2x "its" instead of "their" (OM)

OK changed.

09 leftover "Organic matter transfers"

OK removed.

11 did you mean "within" instead of "to".

OK changed.

Also consider adding a connecting sentence between sentence 1 and 2 to frame the whole section, saying that transfer occurs mainly via pedoturbation or water transport.

OK added.

13 consider replacing ". Otherwise bioturbation is" by "(bioturbation). Bioturbation occurs" to clarify that bioturbation is a subcategory of pedoturbation

OK changed.

17 the study by Jagercikova et al. 2017 does not support this statement

We made mistakes when citing the Jagercikova et al. papers. Appropriate Jagercikova et al. 2014 and 2015 papers are now cited.

19 better "mineral particles" than "minerals"

OK changed.

19 provide reference to statement "mix several dozen tonnes/ha/year"

We add the following reference: A review of earthworm impact on soil function and ecosystem services February 2013 https://doi-org.insu.bib.cnrs.fr/10.1111/ejss.12025

Page 8 24 "pore" instead of "poral"

OK changed

25 provide reference for the 2 µm cutoff and for this definition of leaching, I thought leaching is the transport of DOM (i.e. lixiviation in your definition which I am not familiar with at all)

These terms are used in different manners from one author to another and not that often used ("DOC/DOM fluxes" are more common!). To avoid any confusion, we changed "leaching" to "lessivage" (with a reference) and remove lixiviation.

26 I don't think OM co-precipitated with oxy-hydroxides counts as DOM (something is either dissolved or precipitated)

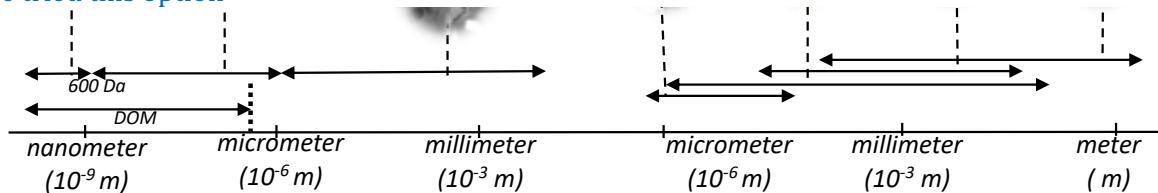
Coprecipitates can be smaller than 450 nm (see Tamrat et al. 2018 and 2019). So the C they contain is considered as dissolved OC when measured in a <0.45 μm fraction. But you are right, this is not true dissolved C! To avoid any misunderstanding we changed the sentence to:

OM of less than 0.45 μm is called "dissolved organic matter" (DOM). This OM is free, but may also include adsorbed on minerals and colloids smaller than 450 nm, co-precipitated with oxy-hydroxides smaller than 450 nm or complexed with metals.

27-28 consider rephrasing this sentence to fit the whole paragraph better, I think translocation is movement of a particle or colloid or DOM within the soil profile, described is eluviation and is only one type of translocation, again citing Jagercikova et al. here seems irrelevant

We removed that sentence which was inherited from a previous version and no longer really useful with regard to DOM

29 in Figure 4, I'd recommend to denote the DOM cutoff

We tried this option

In doing so, one interpretation could be that any molecule <0.45 is dissolved in the soil solution. To avoid any confusion, we decided to not take advantage of this suggestion.

34 consider leaving out "depending on the site" and explain how did you obtain the value 0.7 Gt year-1

OK, changed. The new sentence (in the new section 2.5.2) is:

When integrated on a global scale, C exports via DOC flux have been found to range from 0.002 to 0.05 t of C ha-1year-1 (Doetterl et al., 2016), i.e. 0.7 Gt year-1 (0.05 t of C ha-1year-1 times 15.109 ha).

35 maybe better "OM transfer along the soil surface: erosion:"

OK changed

35 I'd recommend choosing a different intro sentence. Indeed enhanced erosion contributes to soil degradation but this sentence implies that erosion is always a bad thing. This sentence can come later when you discuss that some level of erosion (which is balanced out by

pedogenesis/weathering, actually some level of erosion promotes weathering and is a good thing which enables the mineralogy/nutrient availability of soil to be rejuvenated

We changed the sentence to:

When no longer counterbalanced by pedogenesis, erosion is the major factor in soil degradation at the decade timescale

43-45 this sentence feels clumsy, rephrase

The sentence was rephrased as follows:

Not considering the contribution of erosion to C flux budgets between soils and the atmosphere is a major source of error in the interpretation of soil C dynamics model outputs (Chappell et al., 2016).

47 rephrase, it is not that clear what you mean by "results in C budgets that often generate debate regarding C sinks and sources"

The sentence was rephrased as follows:

C transferred laterally by erosion is lost at the pedon scale, but the integration of processes at the watershed scale (detachment, transport, sedimentation, burial in lowland areas) results in C budgets that often give rise to debate regarding C sinks and sources (Doetterl et al., 2016; Mulder et al., 2015).

51 leftover "."

OK

Page 9 57 better "crystalline" than "crystallized", correct throughout manuscript

OK, changed

62 what do you mean by "organic function"? Do you mean functional group?

Yes, changed

62 replace "on the" by "for a" and "the active" by "an active"

OK, changed

63 not sure what is mean by "localized", would "unevenly" work too?

Yes, changed

63 better "in patches" than "by patches"

OK, changed

65 better "is" than "leads to"

OK, changed

68 it doesn't seem correct to include chelates in "mineral phases"

OK, changed. The new sentences are:

Allophanes, Fe and Al oxyhydroxides are the most commonly described mineral phases. They are often associated with chelates (organic ligands associated with a Fe or Al metal cation) in low pH soils (Rasmussen et al., 2018).

70 maybe better "dissolved" than "destructured"

OK, changed

71 better "can also play" instead of "also plays"

OK, changed

74 again, not clear what you mean by "organic function"

OK, changed

76 "bonds" or "associations" instead of "bounds"

OK, changed

77-81 given its great importance I think authors should expand this part on saturation concept. This comes out now as "saturation concept works only when we consider adsorption but now we know about other mechanisms so maybe saturation is not a real thing" but actually all organo-mineral associations require a mineral counterpart and the sources of mineral phases suitable for those associations is not infinite in soil so therefore saturation still should apply

We agree with the reviewer on the importance of the saturation concept—which is why we quoted it. However, our paper is a review, not an opinion paper. We just want to draw the reader's attention to the fact that this concept is very often used without the mechanisms behind it being clearly explained.

Page 10 3 explain why these models cannot be used at the plot level

The sentence was modified as follows:

They operate in short time steps and have been validated for simplified systems, but they cannot be used at the plot level because they require many parameters not available at this scale

8 Maybe better "Soil carbon pool" than "Soil carbon" to fit better with the following sentence

OK, changed

10 can you provide reference(s) for this equation

OK, Elzein and Balesdent (1995) added

14 here you define "carbon turnover time", then on page 13 section 3.4 you refer to "renewal rates", and on page 13, line 98 you define mean residence time, please unify the terminology/move all terms to when you mention turnover for the first time

We agree with this suggestion. Definitions have been detailed based on those provided by Sierra et al. (2016). The new text is:

The system is at equilibrium (as an annual average) if the input and output fluxes are equal. The system is at steady-state if these fluxes are constant (dC/dt = 0 and I = kC). The term "turnover time" (year) commonly refers to the ratio of total carbon stock to the input or output flux (Eriksson, 1971). In steady-state systems, turnover time, mean age, and mean transit time are synonymous terms. The term "residence time" (year) is used to describe the age of the C in the output flux, the age of the C stock or the turnover time. Hence the term is ambiguous. Sierra (2016) discouraged its use in carbon cycle research.

Renewal rate describes the quantity of new C that has been stored for a period of time. We added this definition in section 3.4.

Page 11 26 what is "hypercomplex functioning"?

We have replaced by "complex functioning" in the new version

27 delete "that"

OK, deleted

26-28 these two sentences seems to be duplicates of each other and

OK, changed:

Finally, dating methods have confirmed that organic materials can be inherited from a distant past of several decades (Mathieu et al., 2015).

33 I found the whole section a bit out of context here and somehow confusing, I'm not sure about its usefulness, I must say I haven't encountered the term mineralization (flux) too much, usually I think respiration is the prevailing term used, I'm also not sure about interpreting biological activity as "efficiency" and also using the term "efficient" decomposing organisms, with possible confusion with CUE. I agree biological activity is a vague term but I view it more as "how much microbes there are and how active (and growing) they are". How this translates into mineralization flux is a matter of CUE.

44 again, here you talk about k, but call it in several ways, but on page 10 you introduced it as mineralization rate, please unify

We agree with this comment and have removed the whole section.

Page 12 55 better just "Non-linear" processes ??

Yes, changed

59 here you repeat Equation 1 from section 3.1, can you somehow refer to it to frame it better

Ok, we referred to Eq 1

60 what do you mean by "change regimes"?

The sentence was changed for clarification

61-72 can you provide more references or is this all based on Vogel et al 2015?

We added: Nunan et al. 2020 The ecology of heterogeneity: soil bacterial communities and C dynamics

Also Table 1 does not contain any references
We added in the text a list of references where non-linear processes have been described (Liyanage et al., 2020; Montagnani et al., 2019; Banegas et al., 2015; McNicol and Silver, 2015; Chen et al., 2013; Wen et al., 2012; Bisigato et al., 2008; Keiluweit et al., 2015)

63 I don't understand how the first implies the second, would "driver" work better instead of "implication"?

OK, changed

68 better "help to gain" than "help gain"

OK, changed

70 what do you mean by "less determined and reversible changes"

This unclear sentence has been removed

73 better just "Priming effect"

OK, changed

75 this is confusing since priming studies typically use glucose as the source of labile C to induce priming effect (e,g, Bastida et al. 2019 Nat. Comm) please correct/explain, please also mention we distinguish positive and negative priming effect

This was a mistake. The text has been corrected.

Page 13 02 refer to the number of section rather than the name

The listed factors are not systematically linked to a section so we cannot refer to the number of section nor remove the name

03 explain how nature of incoming C affects organomineral interactions

We referred to section 2.2.3

05 better use "texture" or "particle size distribution"

OK, changed

8 better "caused by" than "in relation to"

OK, changed

8 better "drying-rewetting cycles"

OK, changed

Page 14 13 really there are no studies showing accelerated respiration after tillage?

We agree that the initial sentence was too affirmative. We moderated the text as follows:

Ploughing is often considered to accelerate biodegradation by aerating the soil, but tillage-induced CO_2 release ends after a few days (Rochette and Angers, 1999)

14-16 please provide references for all sentences

OK added

17 what do you mean by second sentence?

The sentence was changed as follows:

Mineral phases interacting with SOM (particularly poorly crystalline minerals) are not always stable (Basile-Doelsch et al., 2015) and can be modified by land use, plants, pH and amendments (Collignon et al., 2011)

24 refer to the section where you discuss Ca2+ bridges previously

OK done

24-25 provide references on each statement about Al and Na

We have added the following references: (Rasmussen et al., 2018; Heckman et al., 2018). (Qadir and Schubert, 2002).

26 section 2.5.2 doesn't exist

Yes, right. The correction was made.

26-31 please provide references

We have added the following references: Monzoni et al. (2012), Barrios et al. (2007), Daam et al. (2011)

32 mycorrhiza should be mention in this section (actually sentence in line 33 seems to be about mycorrhiza(?), but should also mention bacteria (who also participate in priming)

The sentence was changed as follows:

Plants release exudates and feed fungi through symbiotic associations (mycorrhiza) and bacteria from the rhizosphere that biodegrade OM (Fontaine et al., 2011) or even destabilize organomineral associations (Keiluweit et al., 2015), while releasing nitrogen compounds or phosphorus.

35 give examples of such interactions

The sentence was changed as follows:

Many other plant-microorganism interaction mechanisms (e.g. mycorrhizal type or polyphenol concentration) can also exert control over soil C through N competition (Northup et al., 1995; Averill et al., 2014), which allows the ecosystem to maintain a substantial reserve of elements and therefore a high degree of resilience.

41 maybe "limited" is better than "very confusing"

Sorry, we think that confusing is more appropriate. We changed "very" to "rather"

Page 15 60-61 explain how? This should be expanded on in the main text

This has been removed from the conclusion

64 would this work better "predation and competition between fungi and bacteria"?

OK changed

70 delete "of"

OK deleted

74 maybe better "understood" instead of "explained"

OK changed

Page 30 65 "dashed" instead of "dotted"

OK changed

Averill, C., Turner, B. L., and Finzi, A. C.: Mycorrhiza-mediated competition between plants and decomposers drives soil carbon storage, Nature, 505, 543-+, 10.1038/nature12901, 2014.

Balesdent, J., Basile-Doelsch, I., Chadoeuf, J., Cornu, S., Derrien, D., Fekiacova, Z., and Hatte, C.: Atmosphere-soil carbon transfer as a function of soil depth, Nature, 10.1038/s41586-018-0328-3, 2018.

Banegas, N., Albanesi, A. S., Pedraza, R. O., and Dos Santos, D. A.: Non-linear dynamics of litter decomposition under different grazing management regimes, Plant and Soil, 393, 47-56, 10.1007/s11104-015-2472-y, 2015.

Bardgett, R. D., Bowman, W. D., Kaufmann, R., and Schmidt, S. K.: A temporal approach to linking aboveground and belowground ecology, Trends in Ecology & Evolution, 20, 634-641, 10.1016/j.tree.2005.08.005, 2005.

Basile-Doelsch, I., Balesdent, J., and Rose, J.: Are Interactions between Organic Compounds and Nanoscale Weathering Minerals the Key Drivers of Carbon Storage in Soils?, Environ. Sci. Technol., 49, 3997-3998, 2015.

Baveye, P. C., Berthelin, J., Tessier, D., and Lemaire, G.: The "4 per 1000" initiative: A credibility issue for the soil science community?, Geoderma, 309, 118-123, https://doi.org/10.1016/j.geoderma.2017.05.005, 2018.

Baveye, P. C., and White, R. E.: The "4p1000" initiative: A new name should be adopted, Ambio, 49, 361-362, 10.1007/s13280-019-01188-9, 2020.

Bertrand, I., Viaud, V., Daufresne, T., Pellerin, S., and Recous, S.: Stoichiometry constraints challenge the potential of agroecological practices for the soil C storage. A review, Agronomy for Sustainable Development, 39, 10.1007/s13593-019-0599-6, 2019.

Bisigato, A. J., Laphitz, R. M. L., and Carrera, A. L.: Non-linear relationships between grazing pressure and conservation of soil resources in Patagonian Monte shrublands, Journal of Arid Environments, 72, 1464-1475, 10.1016/j.jaridenv.2008.02.016, 2008.

Buee, M., De Boer, W., Martin, F., van Overbeek, L., and Jurkevitch, E.: The rhizosphere zoo: An overview of plant-associated communities of microorganisms, including phages, bacteria, archaea, and fungi, and of some of their structuring factors, Plant and Soil, 321, 189-212, 10.1007/s11104-009-9991-3, 2009.

Chen, F. L., Zheng, H., Zhang, K., Ouyang, Z. Y., Wu, Y. F., Shi, Q., and Li, H. L.: Non-linear impacts of Eucalyptus plantation stand age on soil microbial metabolic diversity, Journal of Soils and Sediments, 13, 887-894, 10.1007/s11368-013-0669-3, 2013.

Collignon, C., Uroz, S., Turpault, M. P., and Frey-Klett, P.: Seasons differently impact the structure of mineral weathering bacterial communities in beech and spruce stands, Soil Biology & Biochemistry, 43, 2012-2022, 2011.

Dignac, M. F., Derrien, D., Barre, P., Barot, S., Cecillon, L., Chenu, C., Chevallier, T., Freschet, G. T., Garnier, P., Guenet, B., Hedde, M., Klumpp, K., Lashermes, G., Maron, P. A., Nunan, N., Roumet, C., and Basile-Doelsch, I.: Increasing soil carbon storage: mechanisms, effects of agricultural practices and proxies. A review, Agronomy for Sustainable Development, 37, 10.1007/s13593-017-0421-2, 2017.

Doetterl, S., Berhe, A. A., Nadeu, E., Wang, Z. G., Sommer, M., and Fiener, P.: Erosion, deposition and soil carbon: A review of process-level controls, experimental tools and models to address C cycling in dynamic landscapes, Earth-Science Reviews, 154, 102-122, 10.1016/j.earscirev.2015.12.005, 2016.

Eriksson, E.: Compartment Models and Reservoir Theory, Annual Review of Ecology and Systematics, 2, 67-84, 10.1146/annurev.es.02.110171.000435, 1971.

Fontaine, S., Henault, C., Aamor, A., Bdioui, N., Bloor, J. M. G., Maire, V., Mary, B., Revaillot, S., and Maron, P. A.: Fungi mediate long term sequestration of carbon and nitrogen in soil through their priming effect, Soil Biology & Biochemistry, 43, 86-96, 10.1016/j.soilbio.2010.09.017, 2011.

Gans, J., Wolinsky, M., and Dunbar, J.: Computational improvements reveal great bacterial diversity and high metal toxicity in soil, Science, 309, 1387-1390, 10.1126/science.1112665, 2005.

Guiboileau, A., Sormani, R., Meyer, C., and Masclaux-Daubresse, C.: Senescence and death of plant organs: Nutrient recycling and developmental regulation, Comptes Rendus Biologies, 333, 382-391, 10.1016/j.crvi.2010.01.016, 2010.

Heckman, K., Lawrence, C. R., and Harden, J. W.: A sequential selective dissolution method to quantify storage and stability of organic carbon associated with Al and Fe hydroxide phases, Geoderma, 312, 24-35, 10.1016/j.geoderma.2017.09.043, 2018.

Kallenbach, C. M., Frey, S. D., and Grandy, A. S.: Direct evidence for microbial-derived soil organic matter formation and its ecophysiological controls, Nature Communications, 7, 10.1038/ncomms13630, 2016.

Keiluweit, M., Bougoure, J. J., Nico, P. S., Pett-Ridge, J., Weber, P. K., and Kleber, M.: Mineral protection of soil carbon counteracted by root exudates, Nature Clim. Change, 5, 588-595, 10.1038/nclimate2580 http://www.nature.com/nclimate/journal/v5/n6/abs/nclimate2580.html#supplementary-information, 2015.

Kogel-Knabner, I.: The macromolecular organic composition of plant and microbial residues as inputs to soil organic matter: Fourteen years on, Soil Biology & Biochemistry, 105, A3-A8, 10.1016/j.soilbio.2016.08.011, 2017.

Larney, F. J., and Angers, D. A.: The role of organic amendments in soil reclamation: A review, Canadian Journal of Soil Science, 92, 19-38, 10.4141/CJSS2010-064, 2012.

Lehmann, J., Rillig, M. C., Thies, J., Masiello, C. A., Hockaday, W. C., and Crowley, D.: Biochar effects on soil biota - A review, Soil Biology & Biochemistry, 43, 1812-1836, 10.1016/j.soilbio.2011.04.022, 2011.

Liyanage, A., Grace, P. R., Scheer, C., de Rosa, D., Ranwala, S., and Rowlings, D. W.: Carbon limits non-linear response of nitrous oxide (N2O) to increasing N inputs in a highly-weathered tropical soil in Sri Lanka, Agriculture Ecosystems & Environment, 292, 10.1016/j.agee.2019.106808, 2020.

Mathieu, J., Hatté, C., Balesdent, J., and Parent, E.: Deep soil carbon dynamics are driven more by soil type than by climate: a worldwide meta-analysis of radiocarbon profiles, Global Change Biology, 21, 2015.

McNicol, G., and Silver, W. L.: Non-linear response of carbon dioxide and methane emissions to oxygen availability in a drained histosol, Biogeochemistry, 123, 299-306, 10.1007/s10533-015-0075-6, 2015.

Montagnani, L., Badraghi, A., Speak, A. F., Wellstein, C., Borruso, L., Zerbe, S., and Zanotelli, D.: Evidence for a non-linear carbon accumulation pattern along an Alpine glacier retreat chronosequence in Northern Italy, Peerj, 7, 10.7717/peerj.7703, 2019.

Mulder, V. L., Lacoste, M., Martin, M. P., Richer-de-Forges, A., and Arrouays, D.: Understanding large-extent controls of soil organic carbon storage in relation to soil depth and soil-landscape systems, Global Biogeochemical Cycles, 29, 1210-1229, 10.1002/2015GB005178, 2015.

Northup, R. R., Yu, Z. S., Dahlgren, R. A., and Vogt, K. A.: Polyphenol Control of Nitrogen Release from Pine Litter, Nature, 377, 227-229, 10.1038/377227a0, 1995.

Qadir, M., and Schubert, S.: Degradation processes and nutrient constraints in sodic soils, Land Degradation & Development, 13, 275-294, 10.1002/ldr.504, 2002.

Rasmussen, C., Heckman, K., Wieder, W. R., Keiluweit, M., Lawrence, C. R., Berhe, A. A., Blankinship, J. C., Crow, S. E., Druhan, J. L., Pries, C. E. H., Marin-Spiotta, E., Plante, A. F., Schadel, C., Schimel, J. P., Sierra, C. A., Thompson, A., and Wagai, R.: Beyond clay: towards an improved set of variables for predicting soil organic matter content, Biogeochemistry, 137, 297-306, 10.1007/s10533-018-0424-3, 2018.

Rochette, P., and Angers, D. A.: Soil surface carbon dioxide fluxes induced by spring, summer, and fall moldboard plowing in a sandy loam, Soil Science Society of America Journal, 63, 621-628, 10.2136/sssaj1999.03615995006300030027x, 1999.

Senesi, N., and Plaza, C.: Role of humification processes in recycling organic wastes of various nature and sources as soil amendments, Clean-Soil Air Water, 35, 26-41, 10.1002/clen.200600018, 2007.

Wen, Y., Su, L. M., Qin, W. C., Fu, L., He, J., and Zhao, Y. H.: Linear and non-linear relationships between soil sorption and hydrophobicity: Model, validation and influencing factors, Chemosphere, 86, 634-640, 10.1016/j.chemosphere.2011.11.001, 2012.