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

Anonymous Referee #2

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This manuscript is a review on the mechanisms of carbon storage in soils. The aim of the review was to provide a comprehensive, in-depth overview on the scientific knowledge on processes involved in soil C dynamics, how they develop over time and how they are controlled by different biotic and abiotic factors. The authors successfully present the state of the art in these fields. The main part of both the manuscript text and the Conclusions section centers around SOM transformation processes, less emphasis is given on the (long-term) dynamics and on potential implications for soil management.

On the level of headers, the manuscript is generally well organized and addresses all fields related to the stated aims. However, my impression is that in the sections, the flow is sometimes lost and a lot of details are discussed without properly putting them into the large context of the review. This is even true for the Conclusions section. As a result of that, the novelty and the synthesis character of the manuscript are not really obvious. As I think this manuscript can be revised to provide a timely, necessary and relevant review, I would like to ask the authors to highlight the novel insights with respect to the aims mentioned in the introduction more in their revision of the manuscript.

I also noted that the authors give relatively statements and quite detailed information without references at a number of places in the text. The authors thus should thoroughly check if additional referencing is needed.

We thank referee#2 for all these comments. We have endeavored to take them into account, adding references, removing unimportant details, highlighting the novel insights as much as possible (by adding *"It is important to highlight the recent insight... " "This leads to the following very important result..."),* as well as appropriate references. The structure has been slightly changed according to the suggestions of referee #1. The conclusion was completely rephrased.

Here are some more detailed comments:

Abstract: The abstracts needs an introductory sentence that should go beyond the statement that recent research improved our knowledge on SOM characteristics and turnover, and a concluding sentence which refers to the aims mentioned. Similar to the whole text, the novelty and synthesis character need to be stressed more. A more formal issue is the list of bullet points, which I suggest to replace by running text, maybe with (1), (2) etc. to indicate the individual items.

According to the above-mentioned suggestions, the abstract has been modified as follows:

Soil organic matter (OM) represents a key C pool for climate regulation but also an essential component for soil functions and services. Scientific research in the 21st century has considerably improved our knowledge of soil organic matter and its dynamics, particularly under the pressure of the global disruption of the carbon cycle. This paper reviews the processes that control C dynamics in soil, the representation of these processes over time, and their dependence on variations in major biotic and abiotic factors. The most recent advanced knowledge gained on soil organic matter includes: (1) Most organic matter is composed of small molecules, derived from living organisms, without transformation via additional abiotic organic polymerization; (2) Microbial compounds are predominant in the long term; (3) Primary belowground production contributes more to organic matter than aboveground inputs; (4) The contribution of less biodegradable compounds to soil organic matter is low in the long term; (5) Two major factors determine the soil organic carbon production 'yield' from the initial substrates: the yield of carbon used by microorganisms and the association with minerals, particularly poorly crystalline minerals, which stabilize microbial compounds; (6) Interactions between plants and microorganisms also regulate carbon residence times, and therefore carbon stocks; (7) Among abiotic and biotic factors that regulate the carbon residence time, only a few are considered in current modelling approaches (i.e. temperature, soil water content, pH, grain size, sometimes C/N interactions); (8) Although most models of soil C dynamics assume that the processes involved are linear, there are now many indications of non-linear soil C dynamics processes linked to soil OM dynamics (e.g. priming). Farming practices therefore affect soil C stocks not only through carbon inputs but also via their effect on microbial and organomineral interactions, yet it has still not been possible to properly identify the main mechanisms involved in C loss (or gain). Greater insight into the mechanisms, their interdependencies, hierarchy and sensitivity to agricultural practices could provide future action levers for C sequestration in soil.

Introduction: I liked the introduction very much and have no issues with it.

We thank referee 2 for this comment. The Introduction has nevertheless been changed slightly according to some suggestions of referee#1, but the main structure is unchanged.

Main processes controlling the C stock and its dynamics in soils: This header needs rewording to better represent the content of the subsections which include information on fluxes and litter composition rather than on processes involved.

The header has been changed to:

Main processes controlling the C nature, stock and its dynamics in soils

- line 54: The TOC concentrations given here do not match the ones in Figure 1, to which this sentence refers. This is because the examples given in the text include organic layers on forest soils whereas the figure uses a grassland site as an example. I also suggest to use the same units in the text and the figures (either mg/g or g/kg). Although the numbers correctly represent the range of TOC concentrations expected in soil, the authors should include reference(s) for this information.

We specified "forest soil", used g/kg in the text and referred to Oades (1988) for TOC concentrations

- line 58: In addition to losses by mineralization to CO2, C might also be exported from soils by leaching as DOM or erosion of POM.

This sentence was changed as follows (also considering a suggestion from referee 1):

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.

- lines 64-66: The return of the harvested C in form of manure or sewage sludge is an interesting point. Is there any information or estimate available how much of the total input or of the total harvest this is?

Unfortunately we do not have this information

- lines 74-90: This section should be revised for a better structure with respect to total partial fluxes. Maybe simple separation into multiple paragraphs would already solve this problem. I also suggest to resolve some seeming contradictions, such as a higher contribution via roots (line 74) while shoot biomass dominates root biomass (line 77).

We separated the section into multiple paragraphs, and changed the first two sentences. The section now reads:

Above- and belowground input fluxes

Estimators of soil restitution fluxes are often based on plant carbon allocation equations (allometric relationships) combined with carbon models. The harvest index (HI) is the harvested proportion of net primary production from shoots, while the rest is returned to the soil. Note, for example, that HI values of 45–55% are commonly obtained for highly productive cereals (Fan et al., 2017). Genetic and agronomic optimization of yields generally increases the HI. It can therefore be said that, for a given crop production, carbon returns to soils increase with primary production, but the restitution/yield ratio decreases as the yield increases.

The belowground to aboveground biomass (root/shoot) proportion is an indicator that is closely dependent on environmental conditions while also being highly variable (0.1-0.3) (Bolinder et al., 1997; Poeplau and Katterer, 2017). However, a very important novel finding on SOM mechanisms is that belowground input flux (much less documented than inputs from leaves and stems), are considered to contribute more to soil organic matter through dead roots and rhizodeposition (Clemmensen et al., 2013; Rasse et al., 2005; Katterer et al., 2011). Rhizodeposition represents the contribution of carbon to the soil by living plants via roots. This may include root renewal, release of cells or tissues (epidermis, root hairs, cap cells), macromolecules such as mucilage and extracellular enzymes, or small molecules, i.e. exudates (Nguyen, 2003). Rhizodeposition is a series of processes generally related to the acquisition of water and nutrients by plants (including P, N, K, Fe and Mg). Carbon transfer from plants to symbiotic fungi is one of these processes. It is estimated that the rhizodeposition flux represents 20 to 50% of the net root production (Nguyen, 2003; Jones et al., 2009; Balesdent et al., 2011). Belowground inputs are still largely unknown, highly variable, and constitute a definite but still relatively unexplored lever driving carbon storage in soil. The belowground proportion of primary production is generally greater when the soil conditions are limiting (water, nitrogen, phosphorus, iron).

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. The annual input into the soil is about 5.1 t C/ha/year in this example. In grassland or fodder systems, a greater proportion of the aboveground parts are exported or grazed, and belowground inputs account for the majority of the soil inputs.

- line 88: reword to read "The annual input to the soil adds up to 5.1 t C/ha/year in this example." I think it is important to keep in mind that these numbers do not apply to all soils, but that this is an example.

OK, "in this example" was added

- line 102: Another important component of microbial input is chitin from fungal cell walls.

OK added

- line 103: replace "amino-saccharides" by "amino sugars"

OK changed

- lines 105-106: I do not understand why there should be a contrast between "non- industrial organic waste products" and "compost and sewage slugde" which are certainly non-industrial organic waste products. I guess the term "non-industrial organic waste products" needs some explanation or definition.

We added examples and references; the new sentence is:

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).

- lines 109-111: This part has to be revised to remove a number of inconsistencies. For example, the list in the brackets "terra preta, biochar, coal mines and charcoal production sites" includes a soil type, organic material and sites. Out of these, only biochar would be a "biomass pyrolysis product". In addition, plastics do not really fit into a list about incomplete combustion products.

- line 110: replace "local organic carbon from rocks" by "organic carbon inherited from rocks"

We agree with this comment. Also in light of the comments of referee#1, the whole paragraph has been changed as follows:

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

- line 113: What exactly should be "physical and chemical biotransformation protagonists"? Both fauna and microflora are biological protagonists.

Yes they are both biological protagonists but do not have the same role in OM biotransformation

- lines 131-132: The partial sentence about the abundance of fungi and their hyphae length has to be reworded.

According to referee#1, the sentence was reworded as:

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).

- line 139: The lower microbial biomass in cultivated soils usually goes along with lower TOC concentrations.

OK added in the sentence

- line 150: How would growth (alone) affect substrate/enzyme contact?

For example, if a fungus hyphae is growing, it may reach organic compounds that were located too far away before growth and degrade them (via its enzymes).

- line 153: Please reword the phase about final electron acceptors and C oxidation, it combines reaction partners and processes in a strange way.

According to referee #1, the sentence has been changed to:

Oxidative degradation can occur with other electron acceptors, but with reaction rates that can be 10 times slower (Keiluweit et al., 2017; Klupfel et al., 2014).

- line 158: replace "microorganisms" by "microbial"

OK changed

- line 159: The production and release of extracellular enzymes does not only come with energy cost. The cells have to invest carbon and nitrogen (and sulfur), too. Also, as reactions take place outside the cells, extracellular enzyme activity is restricted to reactions which do not require energy input in form of ATP (or similar).

OK, we added "cells have to invest C, N and S"

- line 163: Uptake of almost all organic molecules into the cells is an active process. Please replace "adsorbed" by "transported"

OK changed

- line 180: This statement needs a reference.

According to referee#1, the whole paragraph on CUE has been moved to a new section devoted to outputs (including transfers (erosion and DOC) and mineralization). References have been added.

- line 195: "Many studies" requires a number of references.

The sentence has been simplified to:

Compounds of microbial origin (polysaccharides, proteins, etc.) have a longer lifespan in soil than structural compounds in plants (celluloses, lignins, etc.) (Amelung et al., 2008).

- line 199 and Fig. 2: Most depolymerization processes are hydrolytic processes

We added this information by changing the sentence:

Oxidative depolymerization reactions are mainly hydrolytic processes. They systematically lead to ...

- lines 202-204: SOM carries a number of different functional groups in different chemical environments. Their pKa values cover a much wider range than pH 4-5.

Yes, of course. The COOH was just an example. We do not think that the sentence should be changed.

One example is the carboxyl group, which is mainly in the form of COOH at pH below 4 but of COO- at pH above 5.

- line 209: Figure 1 does not inform about the contributions of the different types of plant material mentioned in line 208.

Yes, the reference to figure 1 was a mistake

- line 216: "elementary molecules": please reword

OK, changed to "smaller"

- line 226: Organo-mineral interactions are determined by both the availability of both the organic matter and the mineral to react with each other.

We agree. The sentence was changed (see section 2.3.1)

- line 226: Delete "Organic matter transfers"

Mistake, deleted

- sections "OM transfers within the soil profile" and "OM transfers on the soil surface: erosion": These sections seem to be a bit misplaced within the section about transformation processes. The whole section also fails to really link well to the processes of C cycling. I also suggest to replace "transfer" by "transport"

This is right. OM transfers within the soil profile section is now the title of section 2.3 (no longer in the biotransformation section). Erosion is now in section 2.5 (Soil carbon outputs) as recommended by referee #1

- line 228: The context suggests "transferred through" or "transferred within"

OK changed

- line 237: "10-30 mg/g/day of dry OM": The unit given here is confusing. First, "dry OM" obviously relates to the "g" and should be placed there. Secondly, is it per g plant organic matter or per g earthworm biomass (which would make more sense?)

Details have been added:

(plant residue or other organisms, equivalent to 10–30 mg of dry OM/g of fresh earthworm biomass/day)

- line 240: "hotspots of OM enriched in organic compounds" should be reworded to sound less repetitive.

OK changed to:

These biostructures represent hotspots of OM enriched in mucus that contribute to the stability of organomineral aggregates (Coq et al., 2007; Shan et al., 2010) and to C stabilization (Martin et al., 1990).

- lines 236-242: How do the different ecological groups of earthworms affect C redistribution?

We added a sentence at the end of the paragraph:

However, earthworms are placed into ecological groups, with each group occupying a specific ecological niche and influencing soil aggregation and C turnover differently (Frazão et al., 2019).

- line 243: "Water that circulates": Usually water movement in soil is vertical, not circular.

Changed to flows

- line 244: I never heard of the differentiation between "leaching" and "lixivation", which I found to simply be the French word for "leaching".

According referee #1, we simplified using only DOM

- line 246: If OM sorbed to minerals is < 0.45 μm to meet the definition of DOM, the minerals must be very small, rather colloids than particles.

Nanoparticles are nano-sized, yet they are still particles!

We added colloids

- lines 249-254: Additional references are required for this section.

We added the Kleber et al. 2015 review in which there is a section devoted to DOM, entitled "Organic Matter in the Aqueous Phase is Small and Mobile"

- line 254: The DOC flux varies quite substantially. Is there more information about controlling factors?

We added the following sentence with a reference:

The DOM content thus depends on site-specific soil, climate, and land management conditions, but there is still a sparsity of experimental data on DOM mechanisms and processes in agricultural soils (Gmach et al., 2020)

- line 258: Erosion is just compensated for by pedogenesis only if the soil is in steady state. If this is not the case, soil is degraded or formed.

According to referee #1, The erosion section is now in the 2.5 Soil C outputs section.

We agree with referee #2: in the text, we say: *"In undisturbed natural systems, material loss through erosion is generally offset by pedogenesis (Doetterl et al., 2016), …"*.

We don't really understand the referee's request here.

- line 263: depleted in what?

In C. Added

- lines 264-265: This sentence needs rewording. Also: The effect of neglecting erosion on the C balance depends on spatial scale and how much of the eroded soil ends up in surface waters.

According to referee #1, the sentence was reworded as:

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).

- section "Stabilization and destabilization of soil organic matter": Although destabilization is an important aspect for SOM turnover, it is not discussed in this section.

This is right; destabilization has been removed from the header

- lines 272-281: The sorption capacity can be simply described by the density of sorption sites and the specific surface area of the minerals.

We do not totally agree: for a single mineral, the different sorption sites may have different adsorption coefficients.

- lines 275 and 278: "clays": The two definitions could also be reflected by using "clay particle size fraction" and "clay minerals"

The use of clay is often confusing because "particle size fraction" and "minerals" are often not specified. This is why we wanted to stress this possible double meaning by using other more rigorous terms which, when used, cannot lead to confusion (<2 μm and phyllosilicates). We added "clay particle size fraction" and "clay minerals":

The smallest minerals, mainly contained in the grain size class of less than 2 μm (clay particle size fraction, classified as "clays" by agronomists), (…)

These include phyllosilicates (clay minerals, classified as "clays" by mineralogists) (…)

- line 285: This statement is only true if sorption and degradation are in direct competition. As soil is a heterogeneous material, there sorption might be dominant in some microhabitats whereas degradation prevails in other places. In addition, spatial separation of enzymes/organisms and substrate may also limit degradation even if the substrate is not sorbed.

We agree with the reviewer. However, the idea was just to explain how adsorption leads to OM stabilization. Questions about colocation enzymes/substrate are discussed elsewhere (2.4.2)

- lines 292-293: As chelates are usually soluble, they cannot be considered a mineral phase.

We agree. The sentence was modified as follows:

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).

- line 300: In acid soils, Fe and Al are effective complexing ions.

Added in the previous sentence

- line 308-309: A simple correlation does not prove causal relationship, and certainly is not appropriate to identify cause and effect.

We apologize but this comment is unclear for us and we are not sure of the changes that are being requested.

- lines 318-320: The term "microhabitats" should be introduced in this section. Soil heterogeneity also controls the accessibility of substrates.

OK, sentences modified to:

The action of decomposers on their organic substrates (see section 2.2.1) takes place in the soil pore network in microhabitats. Since the smallest bacteria are in the micron range, the soil structure and its heterogeneity controls accessibility and biodegradation at the micrometer spatial scale (Juarez et al., 2013)

- line 331: Which information can be used for upscaling from the small scale to the plot scale?

This issue is the focus of ongoing research.

Time dependent processes: dynamic representations:

- line 337-340: In the description of the degradation equation (Equation 1), it should considered that it is not only k which is not fixed, but also inputs are not necessarily constant.

OK added.

- line 345: Isotope tracer approaches only rarely can address millennial scale processes.

This is true, but it does in some studies (Balesdent et al. 2018; Matthieu et al. 2015). We added these references at the end of the sentence.

- Figure 5 is somehow misleading in this context as it only shows the fate of a single input event. It ignores repeated or continuous input and thus carbon accumulation over the years. It also remains unclear if fluxes between pools were considered, and how much of each carbon fraction is transferred between the pools.

We agree with the referee that the figure is a simplified representation. We changed the legend to:

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.

- line 354-356: rephrase to read "Finally, dating methods have shown that, in addition to OM several decades old, organic materials are partly inherited from a distant past (Mathieu et al., 2015)."

OK, the modified sentence is:

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

- line 366: I do not think that "mineralization" always implies "mineralization rate". It might also be used to describe cumulative mineralization over a given time or, in a slightly different field, the process of organic matter oxidation.

- line 368: replace "reserves" by "stock". One obvious reason for decoupling of miner- alization rate from C stock is the availability of OM for microorganisms as a substrate.

- line 379: Just slowing down plant debris biotransformation would result in accumula- tion of plant residues (e.g. in peat bogs) and slow down microbial C cycling and thus accumulation of microbial necromass. It would thus greatly affect the quality of SOM. The aim must be to optimize the delicate equilibrium between using SOM as a C and nutrient source and protecting SOM in order to remove CO2 from the atmosphere and to improve soil properties.

- line 380: "equivalent" to what?

The four above comments refer to the previous section 3.2 (Ambiguous mineralization and biological activity paradoxes) which has been removed as suggested by referee #1.

- section "linear processes, non-linear processes": I had problems following this classification, in particular because first-order decay is classified as a linear process, while in fact it follows exponential decay kinetics. In addition, most soil C models assume a number of pools with different decay rate constants, and introduce "non-linearity" this way.

We do not fully agree with this comment. The definition of linear systems may be found, for example, at: https://en.wikipedia.org/wiki/Linear_system.

- line 393-394: "inputs, microorganisms and OM are not co-located": This statement needs to be rephrased as both inputs and microorganisms are part of OM. The message should be that the microorganisms might not be able to access a potential substrate because of spatial separation.

OK; the modified sentences are:

A major driver of non-linear behaviors is the fact that OM is not homogeneously distributed, and that plant inputs, microorganisms and dead OM are not co-located. Consequently, microorganisms might not be able to access a potential substrate because of spatial separation (Vogel et al., 2015) (Nunan et al, 2020).

- lines 396-400: This section remains rather general and would profit from some more information on potential mechanisms, feedbacks etc. Some additional references might also help.

- lines 409/410 and 412: There seems to be a contradiction between these two sentences: in lines 409/410, the authors state that the lower level of priming results in longer MRT in subsoil than in topsoil, whereas in line 412, they stress that priming is particularly important in deep soil horizons. This needs to be resolved. This also applies to Table 2, where an increase in decomposable carbon input flux is assigned a negative effect on C storage.

- line 412: I think that the "risk of destabilization of pre-existing OM by carbon inputs" in deep soils is overrated by the authors. Even if it is true that C input to these horizons might result in enhanced degradation of old SOM, the net effect would always be an increase in OM because the priming effect does not completely outweigh the C input.

We agree with the above three remarks and propose to modify and expand the section on the priming effect as follows:

A major non-linear effect is the so-called "priming effect" (Fontaine et al., 2007; Kuzyakov et al., 2000; Sallih and Bottner, 1988). In short, the supply of complex decomposable substrates provides competent microorganisms with the energy resources required to biodegrade stabilized OM. Typically, the soil OM residence time is thus shorter in soil zones that receive substantial inputs, such as on the surface, relative to deep horizons. Except in cases of the burial of exogenous OM (which is considered to represent a risk of destabilization of pre-existing OM especially in deep horizons), the rhizosphere is the main soil compartment concerned by priming. The rhizosphere priming effect (RPE) is defined as the stimulation (or suppression) of OM decomposition by live roots and associated rhizosphere organisms as compared to SOM decomposition from rootless soils under the same environmental conditions. Findings of studies conducted in plant growth chambers and glasshouses indicate that the magnitude of the RPE varies widely, ranging from 380% enhancement (positive RPE) to 50% reduction (negative RPE) as compared to basal respiration from root-free soils (Cheng et al. 2013). These RPE levels demonstrate that rhizosphere processes are major drivers of SOM mineralization. Yet the RPE does not necessarily result in an ultimate decline of total OM because the overall input of organic materials from the rhizosphere may compensate for the enhanced mineralization of accessible OM (Cheng et al. 2013). Priming mechanisms may also be linked to complex combined biotic–abiotic mechanisms whereby root exudates promote carbon loss by releasing organic compounds from protective associations with minerals (Keiluweit et al., 2015). Priming effect mechanisms are thus crucial in the dynamics of C, N and P elements and they highlight that carbon storage is not proportional to the inputs.

- line 423: As the authors want to stress how much of the C in deep soil horizons is young, I would suggest to reword to "more than 15% in permanent grasslands"

Yes. We changed this to "15-20%" to be more rigorous

- lines 424-426: Are the responsed of the subsoil as fast as in topsoil, or does it take some time until the signal arrives at larger depths?

In most cases, the subsoil response is posterior to the topsoil response. This is shown by the meta-analysis which calculated that, on average, "Renewal is 7-10 times slower in the 30-100 cm layer than in the 0-30 cm layer (Mathieu et al., 2015; Balesdent et al., 2017; Balesdent et al., 2018)"

Control of C residence times in soil: biotic and abiotic factors

- lines 438-439: Although temperature clearly increases mineralization, it is not clear yet how this is reflected in SOM concentrations, as net primary production and thus litter input to soil also increases with temperature.

Yes, this is right. However, we are focusing the discussion on factors controlling the turnover time, as stated in the introduction of this section: …"*focusing on carbon turnover time variation factors (but variations in stocks also depend on incoming carbon fluxes that are not considered here*"

- line 441: 20-50% water content is rather moist. Assuming about 50% pore space, a 50% volumetric water content would correspond to saturated soil. At this water content I would already expect decreasing aerobic microbial activity.

These data come from Sierra (2015): See for example fig 8b and 8d.

- line 442: replace "desiccation-humectation" by "drying-rewetting"

OK changed

- line 444: Fermentation is energetically extremely unfavourable for microorganisms. In the absence of oxygen, they therefore tend to use alternative electron acceptors such as nitrate, ferric iron, sulfate rather than relying on fermentation.

OK, the sentence has been changed to:

The partial pressure of oxygen. In saturated environments, if the environment becomes anaerobic, microorganisms tend to use alternative electron acceptors such as nitrate, ferric iron and sulfate. The degradation of ligno-cellulosic debris is slowed or even stopped.

- line 448: Increased aeration is certainly one reason why ploughing accelerates biodegradation. Other factors such as making previously physically protected (en- trapped) substrates available also contribute.

This sentence has been changed to:

Ploughing is often considered to accelerate biodegradation by aerating the soil, but tillage-induced CO2 release ends after a few days (Rochette and Angers, 1999).

- line 449 - 452: This section needs substantial rewording. I also found the link between the very small scale (2 μm particles) and the national scale difficult to follow.

We propose to change this section to:

Grain size is quite closely correlated with carbon stocks. The <2 μm fraction content can lead to a 2-fold carbon stock variation. Fine granulometric fractions are often described as enriched with stable C, with turnover times longer than the average values in soil (Balesdent, 1998). Meta-analyses have shown that the <2 μm fraction contains between 0% and 50% of the C stocks in national soil C inventories (Hassink, 1997) (see section 2.4.1).

- lines 457-463: Microbial activity and community composition is clearly affected by soil pH. This should not be neglected.

We added:

The soil pH also seems to control the phylogenetic diversity of microbes (Shen et al., 2013).

- line 479: Most models use a number of pools with different degradation rate constants to describe C dynamics. This might not consider each and every mechanism, but has been found to sufficiently describe the dynamics over sufficiently long times. These models do not claim to identify factors and mechanisms, but to give quantitative estimates.

We added this last information and changed the sentence to:

In current operational models, only temperature, soil moisture, grain size and substrate C/N are taken into account. They give quantitative estimates but fail to properly model dynamics.

- lines 491-492: While it is true that crops are bread for maximized plant production, this refers to above ground biomass only, and in particular to grain yield. Belowground biomass might be smaller, but seems to be important as a C input to soil, in particular to subsoils.

The conclusion has been fully revised and this part was removed. New conclusion is:

Soils have a decreasing C concentration gradient from the surface (a few percent by mass) to about 1 m deep (less than 1%). The 0-30 cm horizon has mainly been studied, but it was recently shown that deep horizons (> 30 cm) contribute up to 20% to the sequestration of new C over 20 years. OM observed in a soil at a given date thus results from a complex inheritance history linked to the functioning (and possible use) of a soil over several hundreds (or even thousands) of years.
Soil OM results from the biotransformation of inputs by plants (mainly underground inputs). In soil, decomposer food-webs and microorganisms are essential for the oxidative degradation and mineralization of organic molecules. The compounds that are not mineralized consists of microbe-derived (main contribution) and plant-derived small organic molecules and form the soil OM. In soil, transfer and association processes with minerals

redistribute these organic molecules and/or more or less permanently shield them from the action of microorganisms though aggregation and organo-mineral interaction processes. Long-term stabilization (this formulation is now preferred over the previous term 'humification') is therefore not driven by the same forces as short-term degradation rates. Plant and microbial biodiversity, mineralogy, water content, pO_2, soil solution chemistry and pH, N and P availability, as well as soil temperature, thus control the mineralization-stabilization balance. The interdependencies of these factors, their time dependence (short-versus long-term response), their relative importance with respect to the storage (or loss) of C, are often not explained in detail, thus making it difficult to link C stock changes to specific processes and/or factors.

C stock predictions are conventionally modelled by linear OM decay dynamics approaches, while only considering a few of these factors (temperature, water content, grain size, pH, sometimes C:N interactions). Very few models account for non-linear processes, such as priming, despite the increasing evidence of their key role in C dynamics.

Greater insight into the mechanisms, their interdependencies, hierarchy and sensitivity to agricultural practices could generate future action levers for C sequestration in soil. Due to the time-scale considered, the methods of choice should include retrospective studies or intercomparison of the findings of long-term experiments at specific sites.

- Table 2 needs some revision to fill missing information. In addition, this table is merely quantitative, it does not at all reflect shifts in SOM composition/quality. As peat bogs impressively demonstrate, accumulating large amounts of OM does not necessarily result in highly productive soils.

This table is not quantitative. Missing information has been added.

In our opinion, in terms of factors and mechanisms, this example of peat bogs refers to the line:

↘	O_2 (anaeroby)	↘	Microbial activity	↗	Yes

We have, however, added a few lines according the remarks of referee #1:

↗	Water content (except pO_2)	↗ Microbial growth and mobility ↘ Sorption ↗ Diffusion and transfer ↗↘ Aggregation ↘↗ Erosion	↘ ↗↘ ↗↘	Yes, all mechanisms combined

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

- Fig. 4: I really like this figure as it clearly shows the relative importance of different organic matter fractions and the contribution of different organisms.

Thank you!

- Fig. 5: This figure represents the fate of a single input event, this should be mentioned in the caption. It should also be noted that the numbers given in the table are only valid in case of steady state. It did also not become clear to me if the pools are linked, i.e. if the C lost from the fast pool enters the intermediate and then the slow pool, or if each pool is only degraded. As the old C pool has a turnover time, it also needs some input to maintain steady state (0.002 tC/ha/yr).

OK. We changed the legend to:

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.

- Fig. 6: Please include some information on the experiment presented here. These are data for a particular soil, they cannot be generalized.

These data come from a meta-analysis, and do not pertain to a particular soil. We changed the legend to clearly specify the data source:

Figure 6: Vertical distribution of carbon incorporated by plant matter in the soil in 20 years under average temperate conditions. The data are from the quantification of a recent incorporation of

atmosphere-derived carbon atoms into whole-soil profiles through a meta-analysis of changes in stable carbon isotope signatures at 112 grassland, forest and cropland sites, across different climatic zones, from 1965 to 2015. The quantity (in 10 cm increments) is expressed as a proportion of the total C incorporated in the first meter of soil. The value is a regression mean estimated from climate and land-use conditions. The error bars represent the 95% confidence interval of the estimate. (Based on data from Balesdent et al. (2018)).