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Interactive comment on "Long-term bare fallow soil fractions reveal thermo-chemical properties controlling soil organic carbon dynamics" *by* Mathieu Chassé et al.

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We wish to thank Dr Ding for his positive feedbacks and comments on our preprint. His major concern is about the discussion on the role of physical and physico-chemical protection in soil organic carbon persistence and the role of chemical recalcitrance, considered to be limited to pyrogenic organic carbon. We agree that this point could be improved. In order to emphasize this, we modified the discussion as follows: (i) The distinction between the three mechanisms leading to soil organic carbon persistence is made in the introduction (p.2, ll.7–32). We refer to broad and highly-cited papers on these topics to support our introduction (e.g. Sollins et al. 1996, Balesdent

et al., 2000, von Lützow et al., 2006, Angst et al., 2016). In order to emphasize that pyrogenic organic carbon is a specific form of soil organic carbon for which chemical recalcitrance may be a determinant mechanism explaining its persistence, we added this sentence : "Even if chemical recalcitrance is regarded as a secondary parameter to explain bulk SOC persistence (Amelung et al., 2008), it could be a relevant parameter for a specific form of SOC: pyrogenic organic carbon (PyOC; Schmidt et al., 2011)." (ii) A specific section (4.3.1) of the discussion is dedicated to the discussion on the major role of physical and physico-chemical protection in soil organic carbon persistence in the fine fractions in light of the major works which studied and explained these processes. (iii) We discuss the specificities of pyrogenic organic persistence and the likely role of chemical recalcitrance in a separate section (4.3.2) to distinguish properly this mechanism, specific to pyrogenic organic carbon, from the two other mechanisms, more widely acknowledged. As detailed in this discussion, we must exclude physical and physico-chemical protection from the dominant mechanisms explaining pyrogenic organic carbon persistence considering the size of the fraction. Other minor comments, detailed in the attachment, are addressed as follows: p.8, l.20: The reviewer is right, the increase of the OC content is not so clear at first sight. We will modify the sentence accordingly: "Inputs of OC in the fine clay subfraction is evidenced by the absolute increase of OC content after 10 years of experiment (Fig. 1b). Compared to the decreasing trend, such increase is also observed in the clay fraction and subfractions." p.8, l.25: The table numbering was wrong, Table 2 will be modified to Table 3. p.18, l.17: We acknowledge that the sentence highlighted by the reviewer was not clear and we will modify it as follows: "Breakdown of coarse OM would lead to inputs of new OC with variable chemistry in the finest fractions, particularly in the clay subfractions. The scattering of the HI and OI values in the clay subfractions during the intermediate years of LTBF experiment strengthen this idea (Figs. 3b and 3d)."

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