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Interactive comment on "Forest conversion to poplar plantation in a Lombardy floodplain (Italy): effects on soil organic carbon stock" *by* C. Ferré et al.

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The authors thank Anonymous Referee #1 for careful reading and for providing useful comments which will certainly help to improve the paper.

Author responses and explanations (ACn) are given with referee comments (RCn).

R1C1: How did the authors account for the obviously significant difference in stone content between the soils under the two land uses?

AC1: With an elevation of 65m a.s.l. and a distance to sea of 250km, the PP and NF sites are already in the center of the Po-valley, the nearest steep slopes of prealpine

hills are observed only 70 km upstream at the outlet of the Ticino river from the more than 300m deep Lago Maggiore, where stones are effectively retained. Consequently, the river deposits in the study area are mainly consisted of sand, gravel is observed only locally (presumably linked to historical river beds), and stones bigger than the coring device could not be observed at all. At the NF site we could not find any rock fragments in five soil profiles and in soil corings; rock fragments were absent also in most part of the PP site with the exception of one corner of the survey area represented by the Haplic Arenosol of profile 4 (see Table S1), where stone content ranged between 1 and 7%. Anyhow, the volumetric stone content was considered both for calculating the SOC stock and the bulk density. Correspondingly, the SOC stock of the abovementioned profile 4 decreased from 4.9 to 4.7 kg m-2 (4%) after stone volume correction for the 0-55cm layer. We will introduce this important detail in the method description.

R1C2: Second, it was not, in my opinion, warranted to estimate bulk density of the deepest layer from a regression established on the shallowest layers, also considering the previous point. A similar regression from a limited number of direct measurements, or a mixed one would have been better; direct measurement better still. As a minimum, data concerning the performance of this regression should be presented for readers to see for themselves, and results not be discussed mixed up with real measurements.

AC2: For the upper two layers considered in the land use comparison of the SOC stock we carried out direct measurements of BD; the deepest layer we considered as relevant mainly as an indicator for the soil conditions before the land use change. In this sense we found a clear difference of C-horizon bulk densities both from pedotransfer estimates (NF = 1,51 and PP = 1.33 g cm-3 for the 55-100 cm layer) and from averaging the measurements taken at the NF(5) and PP(4) soil profiles (NF = 1,55 and PP = 1.37 g cm-3 for the C-horizon). The variability of values within a site was high, however the difference between sites appears to be clear, but we consider of minor relevance for the overall finding of the paper: we have good reason to assume that the nega-

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tive correlation between BD and SOC was valid also before land use change, then the PP area would have contained a higher SOC stock compared to NF, resulting in even higher SOC losses. We will compare in the results section the BD-data measured at soil profiles and BD-data estimated from pedotransfer functions, providing also details of the regressions and of the pedofunctions like determination coefficients and statistical significance values. If the editor agrees, we may also provide as supplementary information the correllation matrix included in a previous version of the paper.

R1C3: I think the geostatistical processing was not necessary and possibly misleading. Again, readers must by supplied with data allowing them to judge for themselves the goodness of the processing; description given is not very encouraging. This way of proceeding introduced an unnecessary difference in data treatment between the two theses, and the resulting higher SOC stocks in the PP might well be a complete artifact.

AC3: We do not fully agree, as geostatistics aimed at verifying the spatial and statistical dependence or independence of SOC data between points of each site. For the NF site, the lack of spatial structure at the investigated scale led us to consider the sampling points as independent from each other: thus, their average represents the mean SOC stock of the forest soil. At PP, SOC stock was instead spatially correlated; the spatial relationship between the pseudo-replicates was thus identified and modelled, giving us the possibility to obtain a map of the SOC stock and to calculate a more accurate and representative value than the simple average. In order to facilitate the understanding of this point, for each site we might introduce the semivariogram to show the absence of autocorrelation for NF soils (pure nugget effect) and the presence of a trend of SOC stock for the PP site.

R1C4: A point about introduction; given the issue being dealt with, it looks like literature review was somewhat lazy. The quite long reasoning between lines 5 and 15, page 9603, which represents the main rationale of the work, is supported by only two references. I really think a somewhat larger effort could have been done to review the literature and correctly reference researchers having published on these issues.

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AC4: we fully agree and will modify the introduction accordingly

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