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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.

Anonymous Referee #6

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Studies like this one are important, because they highlight the uncertainties inherent to wide scale estimation of carbon stocks, which is necessary for the implementation of regional and global estimates of global change, models of future climates and even regulatory frameworks of carbon emissions. It would be a mistake to evaluate such an article merely on its merits as a novel scientific study. Favoring the publication of ‘novel’ results only serves to bias the body of literature by either favoring studies that are statistical outliers or pressuring authors to extrapolate beyond the proper realm of inference of a limited number of samples in a given study. What is greatly needed by the community considering the impacts of climate change and land use are robust methodologies for site-level estimates of carbon stocks and their responsible application to a

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great number of sites. Thus, while the study presented here may not be 'novel' in its scientific findings and implications, it could very well make a useful contribution to the discussion of such methodologies and provide a useful data point in larger analyses.

This study highlights the importance of spatial variability in at least two ways. First, the conclusion that this land use conversion has resulted in a order-of-magnitude greater change in SOC at this site (-57%) when compared to the IPCC standard of -8%. This conclusion does not invalidate the IPCC standard by itself, since it is only a single site in a large landscape, but it does highlight the great uncertainty attached to any such standard. Secondly, this study highlights the spatial variability within and between plots in this study area, in that spatial variability in the PP site was structured similarly to that of coarse sand content, but not at the NF site. The implications of this second point need to be discussed more thoroughly and explicitly. On one hand, it suggests a need for inclusion of such spatial statistics in methodologies of SOC sampling, which is a very important contribution to the methodology in this field of study. On the other, it calls into question the very nature of this type of study, which substitutes space for time in addressing land use change. If there is an underlying spatial structure driving SOC variability, can we really be sure that differences between NF and PP can be attributed to land use change? If so, the case needs to be made more clearly in the manuscript. If not, the authors need to make the case for why this sort of study should be pursued rather than more intensive, controlled studies with a greater number of preconversion measurements and/or number of replicates (as opposed to pseudoreplicates as presented here), despite such uncertainties.

There remain a number of technical issues raised by the previous reviewers, such as the presence of large stones and handling of data at depth, that should also be addressed in the final version of this paper. However, I think those have been adequately covered by those reviews, so I will not reiterate them here. The greatest area for improvement of this manuscript is in placing this study in a larger context (as highlighted by other reviewers comments regarding literature citations) and making sure each sec-

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tion contributes to an overarching message of how this study contributes to the target readers' (I presume those interested in the implications of land use change on SOC) understanding of this larger issue. This, in turn, may lead the authors to decisions on reducing or expanding the discussion of different subtopics to produce a more readable paper that will make a greater impact in this research community.

Interactive comment on Biogeosciences Discuss., 11, 9601, 2014.

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