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Interactive comment on "Multi-factor controls on terrestrial carbon dynamics in urbanised areas" *by* C. Zhang et al.

Anonymous Referee #1

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Using the DLEM model, the authors conducted a simulation analysis addressing the urbanization effects on regional carbon balance in the Southern US (SUS). The strength of the study is the comprehensive consideration of multiple contributing factors such as land use/cover changes, green space managements, and climate and atmospheric chemistry changes resulting from urbanization and global changes. Another merit of the study is the involvement of relatively large spatial (1.2âŢ§105 km2 urban areas in the SUS) and long temporal (1945-2007) scales. The manuscript is written clearly in general but contains some grammatical errors/typos (see minor comments below). Overall, I think the study is timely and the quality of the manuscript is good.

My concerns are mostly on the methodology part. First, in simulating the all-factor combined effects (SUBNZ in Table 1), same factors such as temperature, [CO2], and

N deposition rate are manipulated at the same time for both the global environmental changes (GECs) and urbanization-induced environmental changes (UECs). In parameterizing the model, how were the values of these variables determined separately for GECs and UECs that are happening concurrently in reality? For example, temperature and [CO2] data listed in Table S1 should be derived specifically for the region. Do those input data reflect global change effects or urbanization effects, or both?

Second, the parameter values describing urban managements and urban-induced environmental changes in Table S2 seem too arbitrary to me, although these had been published in Zhang et al. (2012). I strongly suggest that the authors provide some scientific bases for adopting the values of these key input parameters. Otherwise, the study is more of a model parameter sensitivity analysis than an urbanization effect analysis.

Third, modeled carbon balance is essentially a result of the interactions among model assumptions, empirical and mechanistic relationships, and model parameterization. Therefore, discussions on the modeling results, especially those with respect to different vegetation types (forest, grass, shrub), should also be linked to the built-in mechanisms/assumptions of the model, not only to the general conclusions of previous studies as in the current form of the Discussion section.

In short, the authors should provide more specific descriptions on model parameterization and conduct some evaluations on the major model output (i.e. NCE) of interest, although the parameterization part was referred to a previous paper by the authors. I understand that rigorously validating all aspects of the model for various ecosystem types in such a large area is almost impossible and beyond the scope of this study, but even some rough comparisons between the modeled and observed NCEs (e.g. in the Discussion section) are helpful for me/readers to believe that your simulated numbers are at least in the ballpark.

Minor comments: Page 7 line 20, soils are disturbed. Page 7 line 13, UHI, spell out in its first appearance. Page 8 Line 11, takes place Page 12 Line 9, please use the same unit of C storage (Pg or Tg) in the main text and in Figs. Page 17 Line 8, large amount of C loss? Not clear Page 18 Line 26, considerably reduced by—-. Page 33, I don't think Fig. 5 is necessary, since the main ideas have been given in Fig. 1 and those numbers can be summarized in a table or a figure for better comparisons.

Interactive comment on Biogeosciences Discuss., 10, 17597, 2013.