

## ***Interactive comment on “Changes in soil carbon sequestration in *Pinus massoniana* forests along an urban-to-rural gradient of southern China” by H. Chen et al.***

**H. Chen et al.**

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Responses to the comments

Anonymous Referee #2 Received and published: 2 August 2013

This is a solid study with potentially important findings regarding the influence of urbanization on soil C in one region of China. The potential significance is enhanced by the fact that this seems to be the first study of its kind in a developing country with a warm, humid environment. This region is likely more representative of areas that will see rapid urbanization in the future than previous studies.

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Answer: Thank you very much for providing the comments. We have found that they are very useful for improving our manuscript. The followings are responses for your comments.

### 1. General Questions

(1) Do they know the age of forests and were they established prior to urbanization or after urbanization? Is that consistent across the study sites? Could that be a factor in the difference in fine roots?

Answer: The age of forests were listed in supplementary material (Table S1). All forests were established prior to urbanization. We have recognized the potential effects caused by the variation of forests age when we designed this experiment. Therefore, for keeping consistent across the study sites we chose the forests with the similar age (40–60 years). In addition, a correlation and regression analysis between forest ages and fine root biomass was also conducted, the result showed that there was no significant correlation between these two variables ( $R^2 = 0.221$ ,  $P = 0.0895$ ), indicating that the variation of forests age is not the main factor affecting the difference in fine roots.

(2) Several times the authors refer to “urbanization induced environmental changes” but this is not defined. Some examples of specific changes they consider relevant to this ecosystem should be given.

Answer: We agree with the comments above.

For “Several times the authors refer to “urbanization induced environmental changes” but this is not defined”, we have inserted and rewritten the following sentences in the first paragraph of the introduction: “Rapid urban development has the potential to alter regional C budgets through urbanization-induced environmental changes (Trusilova and Churkina, 2008; Pouyat et al., 2002). Urbanization-induced environmental changes includes a variety of environmental changing factors caused by accelerating urbanization, such as increases in air temperature, precipitation, atmospheric

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CO<sub>2</sub>, and nitrogen (N) deposition (Shen et al., 2008) .” Please see Lines 33-38 in the revision(attached in supplement).

For “Some examples of specific changes they consider relevant to this ecosystem should be given.” These examples have been given in the first paragraph of “Study region”(also see in Lines 86-92). The air temperature, CO<sub>2</sub> emissions, N deposition and precipitation are all higher in urban area than those in rural areas in our study region.

Reference: Shen, W. J., Wu, J. G., Grimm, N. B., and Hope, D.: Effects of urbanization-induced environmental changes on ecosystem functioning in the phoenix metropolitan region, USA, *Ecosystems*, 11, 138-155, 2008.

## 2. Specific Comments

(1) P11321, line 3, rather than referring to “belief”, phrase these as what the current scientific evidence is supporting

Answer: We agree with your suggestion. We have replaced the sentence “It is generally believed that urbanization-induced environmental changes should increase soil C sequestration of urban forests.” with “The current scientific evidence supports that urbanization-induced environmental changes should increase soil C sequestration of urban forests.” (also see Lines 46-47 in the revision)

(2) Section 3.2 In the discussion of soil C at different depths the results are described as significant for the 0-10cm and for the 0-40 cm, but not the second and third layers. This makes it appear as though all the change is in the 0-10 cm, but Figure 2 shows differences in all the layers. So, is there greater significance in the trend when the whole column is considered than just the top 10 cm? Or is the majority of change driven by the top layer?

Answer: Thank you for your comments. We want to show that the majority of change driven by the top layer. In your comment, you pointed out “Figure 2 shows differences

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in all the layers”. However, we think you may misunderstand our meaning. Actually, Figure 2B have clearly shown that there were only differences in the top layer but not the second and third layers. You can see that the letters in the top layer are different, but other layers with the same “a” (also see Figure 2B in revision). We admit the letters in the top area are easy to be misunderstood for that they are for all the layers, but we do not think it is a mistake.

(3) P11328 line 15 The discussion refers to the elevated soil temperatures associated with urban sites, yet soil temperature is not mentioned in the methods. Was this measured, and was higher soil T associated with urbanization in this study? Or is this statement relying on other sources of information?

Answer: Thanks for this excellent comment. You are right. We did not measure the soil temperature in this study. What we measured were air temperatures and we actually mean “elevated air temperature associated with urban sites” in this sentence. Comparing to air temperature, soil temperature obviously is a more direct index related to SOM decomposition. However, higher air temperature in urban area was also widely used as an important explanation for the higher SOM decomposition rate in urban areas. For instance, the example we given in the manuscript: “Pouyat et al. (2002) suggested that the elevated temperature in urban areas increased litter decay rate, and that the magnitude even can offset increased litter input to the soil”. In their study, they also did not measured soil temperatures. Similar citation can be see in the study of Koerner et al. (2010).

According to your comment, we have rewritten this sentence as follow: “Meanwhile, the elevated air temperatures associated with urban sites would also increase SOM decomposition.”(also see in Lines 242-243 in revision), and we clearly realize that it is deserved to measure the soil temperatures across this gradient in future works.

References:

Pouyat, R., Groffman, P. , Yesilonis, I., and Hernandez, L.: Soil carbon pools and fluxes

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in urban ecosystems, Environmental Pollution, 116, S107-S118, 2002.

Koerner, B. A. and Klopatek, J. M.: Carbon fluxes and nitrogen availability along an urban-rural gradient in a desert landscape - Arizona State University, Urban Ecosystems, 13, 1-21, 2010.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/10/C4861/2013/bgd-10-C4861-2013-supplement.pdf>

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