Interactive comment on "Estimating spatial variation in Alberta forest biomass from a combination of forest inventory and remote sensing data" by J. Zhang et al.

J. Zhang et al.

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Reply to anonymous Referee #2

RESPONSE: We thank the reviewer for the thoughtful and constructive comments, which certainly helped improve the manuscript. See below for our point-to-point responses (text in blue type).

The manuscript "Estimating spatial variation in Alberta forest biomass from a combination of forest inventory and remote sensing data" is one of several ongoing efforts around the world to improve and map estimates of biomass density. Such studies are important and have the potential to greatly improve scientific understanding of forests and their distribution. The authors have combined two large data sets and the information could potentially be very useful to a broader scientific community if the authors had complied with agreed-upon scientific conventions.

RESPONSE: Thanks for the positive response.

The fundamental problem with this manuscript is that the authors attempt to estimate "forest biomass" but do not follow international definitions of what constitutes forest biomass. Given that the authors in their opening sentence of the abstract make reference to studies of carbon cycling, it can be expected that they follow the conventions used in such studies. The Intergovernmental Panel on Climate Change (IPCC) has for many years developed and published guidelines for the estimation and reporting of carbon stocks and stock changes in terrestrial ecosystems (e.g. IPCC 2003, 2006). These guidelines distinguish 5 carbon pools: aboveground biomass, belowground biomass, litter, dead wood, and soil organic matter (see Table 1.1 in IPCC 2006). By definition, biomass does not include standing dead trees or downed dead organic matter (sometimes also referred to as necromass). Note also that the dead wood pool includes dead coarse roots, which are not included in this manuscript's estimates of "debris". Moreover, the methods used to estimate live root biomass and dead wood are questionable: why use Cairns et al. 1997 when a more comprehensive study for Canadian species is available (Li et al. 2003) and why apply a simple 5% scaling factor for "debris biomass" (a misnomer!) to all plots when it is well understood that the relationship between biomass and dead organic matter pools is highly variable over stand development.

RESPONSE: Following the reviewer's suggestions, we reorganized our manuscript by focusing on living tree biomass (aboveground biomass, AGB). All the figures and tables in the main text are now directly related to AGB. In addition, we included our estimates of total biomass, including AGB, belowground biomass (BGB) and debris biomass, in our supporting document (Appendices C and D) and Table 5. For calculating BGB, we used Li et al. (2003)'s equations to replace the ones we used before.

The authors also do not clearly specify the units in which their results are reported which further reduces the utility of the study: are results reported in units of carbon or biomass? This confusion of definitions and lack of attention to units culminates in the discussion section in which their results are compared to those of other studies (e.g. Table 5). However, results in that table appear to be reported in units of biomass (Mg and Mg ha-1) but the original numerical values transcribed from other studies are in units of either biomass or carbon! Moreover, the studies included in the Table report values for different pools which complicates the comparison (See further details below on Section 4.1 errors in Table 5). Fortunately, these two serious problems can be addressed by recompiling the data and reporting revised results.

RESPONSE: Thanks for this suggestion. Now we have kept all the results in units of biomass. In Table 5, as the reviewer mentioned, some values from other studies were in units of carbon. We converted carbon stock into biomass stock by multiplying a conversion factor of 2. The values of carbon stock in these literatures were calculated in the same way.

The authors should therefore: 1. Report only aboveground biomass, excluding standing dead trees, as this is the only estimate directly derived from ground plot measurements. I appreciate that it may be difficult to separate standing dead from live trees, but the permanent sample plots normally make this distinction and the authors know the number of both live and dead trees, suggesting that they do have the required data.

RESPONSE: According to the reviewer's suggestion, we have only reported AGB in our main text, and excluded standing dead tree biomass from AGB. In addition, we reported the estimations of total biomass in the supporting document (Appendices C and D).

2. If there is an interest in total biomass (i.e. aboveground plus belowground biomass in roots) then report this as a second variable (but since this is strongly correlated with and derived from aboveground biomass it may add little additional information).

RESPONSE: We have added the results of total biomass in the supporting document (Appendices C and D).

3. Do not report the pool you refer to as "debris" given that in your case this was merely estimated as 5% of the biomass.

RESPONSE: As the reviewer mentioned, the debris biomass is one of the five contributors on carbon pools (IPCC 2006). Therefore, we think it is necessary to include it when reporting total biomass and/or carbon stocks, although we only used the average percentage (5%) of the AGB. We agree that there is a huge variance in debris distribution among different forest types and stand ages, but it is very hard to deal with this issue based on the current available data in Alberta.

4. Decide on the units to use for the reporting (your section 2.3.4 provides methods for the conversion of biomass to carbon) but your figures and tables use unit descriptors that do not include the C, suggesting that these are units of biomass. Please clarify this confusion, and use the correct units consistently.

RESPONSE: Thanks for this suggestion. Now we only used the units of biomass (Mg and Mg ha^{-1}) in our manuscript.

A second issue is the use of LiDAR data at the scale of one data point per 1 km2 (or 100 ha) when all permanent sample plots are at least two orders of magnitude smaller than the 100 ha grid cells. Depending on the region of AB the landscapes are more or less heterogeneous and can contain many stands within any 100 ha grid cell. One can therefore expect a wide range of canopy heights in each cell. However, the authors demonstrate in Figure 2c a reasonable agreement (though see my comments below on Figure 2c). It would be good if in the discussion of the paper they could add the point that further improvements in estimates could be achieved with either fine resolution estimates of canopy height, or other remote sensing data at resolutions higher than 1 km2.

RESPONSE: According to the reviewer's suggestion, we have added one paragraph to discuss this issue in the Discussion section (see *4.3 Canopy height as an important determinant of biomass distribution*), and cited one recent study on assessing the agreement between spaceborne LiDAR canopy height data and airborne LiDAR derived canopy height data in Canada's boreal forests (Bolton et al., 2013).

Reference:

Bolton, D. K., Coops, N. C., and Wulder, M. A. 2013. Investigating the agreement between global canopy height maps and airborne Lidar derived height estimates over Canada. Canadian Journal of Remote Sensing 39, S139-S151.

The authors' estimation of accuracy is also somewhat problematic because it appears that the same data set (1968 plots) is used to derive the model parameters and is then used again in its entirety in the accuracy calculations. This should be clarified and alternative approaches for accuracy assessment considered.

RESPONSE: We didn't clearly explain about model accuracy assessment in the previous version. We now have rewritten this part. You could find the details in the Methods section (see *2.3.5 Model accuracy assessment*). Actually, we did randomly divide the ground inventory plot data into training data (60%) and test data (40%). This analysis was repeated 100 times, and the average values of model accuracy indicators were reported in our manuscript.

I recommend that this paper be rejected and re-considered for publication after major revisions. The subject matter and the potential results are important and therefore warrant publication once the problems are addressed.

RESPONSE: Thanks. We have redone the analyses and revised our manuscript based on your comments and suggestions. We believe that the current version has been greatly improved.

Specific additional comments are provided below.

While preparing the revisions to the manuscript, the authors may want to also consider the results of the new study by Beaudoin et al. 2014 which can be found in CJFR at: (http://www.nrcresearchpress.com/doi/abs/10.1139/cjfr-2013-0401#.Uv_EE0YWLmQ)

RESPONSE: Thanks. We have added this study in Table 5.

P19006: Abstract – clearly identify which pools you report and in which units.

RESPONSE: We have revised the abstract to report aboveground biomass only.

P19012, L8: One of several examples that require editing: “Totally, 490 sampling plots were included for current work, including 36 059 living trees and 7046 snags.” Could be revised to “In total, 490 sample plots with measurements for 36,059 live trees and 7,046 snags were used in this study.”

RESPONSE: We have revised this sentence. Thanks.

Section 3.1: The authors report an average biomass density of 172.33 Mg ha-1 and then list that the biomass densities for species ranged from 23.95 to 50.86 Mg ha-1. How can ALL of the species-specific estimates be so much lower than the provincial average? Is this because the species-level estimates are only part of the biomass in a plot, and multiple species sum to the plot-level biomass? Even then, the sum of the biomass density of the four species listed is less than the provincial average. Please clarify.

RESPONSE: The average AGB density based on ground inventory data is 128.24±76.64 Mg ha^{-1} (Section 3.1). The AGB densities for lodgepole pine, trembling aspen, black spruce and white spruce were 75.79, 73.21, 34.43, and 38.84 Mg ha^{-1}, respectively (Section 3.1, Table 4). The reason why we had low biomass densities for some species is because we included the inventory plots without species occurrence (i.e., AGB of this species equals to zero) before. Clearly, it is not the right way to do this summary. Thanks for pointing it out.

Section 4.1. The authors state that “Compared with other studies, our estimate of mean biomass density was close to several studies at global and regional scales, while it also had a large difference from the estimates of some other studies, such as Dixon et al. (1994), Pan et al. (2011) and Penner et al. (1997) (Table 5). Clearly, there is a huge disagreement among different estimates, but it is hard to compare them because of different data sources, estimation methodologies, and time periods of data collection.” While it is true that these studies compiled in Table 5 include methodological differences, the magnitude of the differences is in part due to the fact that the authors compare estimates in different units (some are biomass others carbon – a two-fold difference) and for different pools (some studies are for aboveground biomass only, others are for total biomass and others include additional pools). In an effort to better understand Table 5 I started to discover several errors and inconsistencies that admittedly added to my frustration with this manuscript and tipped the balance towards rejection in its present form. For example: Penner et al. 1997 report their estimates in units of biomass, while Pan et al. 2011 and Kurz and Apps 1999 report units of carbon. Penner et al. 1997 report only aboveground biomass (see p. 17 in Penner et al. 1997) though it is unclear if this includes dead standing trees. Neither roots nor dead organic matter are included in that study. Kurz and Apps report an aboveground biomass density of 35.9 Mg C ha-1 (their Table 6) and all pools excluding soil are reported as 70.0 Mg C ha-1 – neither of these numbers agrees with the value of 71.8 Mg C ha-1 cited in Table 5 of this manuscript. Pan et al. 2011 (Table S3) reported a C stock for Canada's forests of 30.7 Pg C (not the 38 Pg C cited in Table 5). Using the correct C stock estimate then changes the C density from 165.65 Mg C ha-1 reported in Table 5 to 133.82 Mg C ha-1. In summary, Table 5 is a mess and while such comparisons are useful, they need to be conducted with more diligence. Lastly, the study by Stinson et al. 2011 provides detailed estimates of carbon density by ecozones, including those in Alberta, and is the basis for the estimates cited in Pan et al. 2011.

RESPONSE: Thanks for checking these literature, and correcting our possible errors. Previous summaries in Table 5 were reported in units of biomass (Mg ha^{-1}), not carbon (Mg C ha^{-1}). Although we had the unit in the column names, the units might not easy to find. According to the reviewer's suggestion, we now have used the units of biomass through the whole manuscript. Also, we added three notes, which are related to how we converted different units into the same units, under Table 5 to help readers to better understand this table. Our responses on species comments are as below.

1) *"Penner et al. 1997 report only aboveground biomass (see p. 17 in Penner et al. 1997) though it is unclear if this includes dead standing trees. Neither roots nor dead organic matters are included in that study."*
   RE: Penner et al. (1997) only reported AGB, so we converted it into total biomass by multiplying 1.36 (belowground biomass is assumed to be 0.36 of the AGB, Jarvis et al. 2001), in order to compare with other studies which only reported total biomass or carbon storages. Now we included both AGB (Table 7 in Penner et al. 1997) and total tree biomass in Table 5.

2) *"Kurz and Apps report an aboveground biomass density of 35.9 Mg C ha-1 (their Table 6) and all pools excluding soil are reported as 70.0 Mg C ha-1 – neither of these numbers agrees with the value of 71.8 Mg C ha-1 cited in Table 5 of this manuscript."*
   RE: The unit of the value "71.8", which we reported in previous version of Table 5, is Mg ha^{-1}, not Mg C ha^{-1}. The value "35.9 Mg C ha^{-1}", which reported in Kurz and Apps (1999) in Table 6, was total biomass carbon (the sum of AGB and belowground biomass), not the AGB only. We converted this value into biomass unit (Mg ha^{-1}) by doubling it (35.9 * 2 = 71.8 Mg ha^{-1}) (Schlesinger 1997, Table 5 in our manuscript).

3) *"Pan et al. 2011 (Table S3) reported a C stock for Canada's forests of 30.7 Pg C (not the 38 Pg C cited in Table 5). Using the correct C stock estimate then changes the C density from 165.65 Mg C ha-1 reported in Table 5 to 133.82 Mg C ha-1."*

   RE: The units of the value "38" is Pg, not Pg C. We calculated it using the sum of total living biomass and dead wood (38 = (14+5)*2) in Table S3 in Pan et al. (2011). We didn't include the litter stock, according to the definitions of biomass related terms in Pan et al.'s paper (Pages 2-3 in the Supporting online material). According to the definition of litter, litter stock includes the litter, fumic, and humic layers. We didn't include this part for our current study.

4) *"Lastly, the study by Stinson et al. 2011 provides detailed estimates of carbon density by ecozones, including those in Alberta, and is the basis for the estimates cited in Pan et al. 2011."*

   RE: We have added this reference in Table 5. "Pan et al. (2011)" has been changed to "Pan et al. (2011) & Stinson et al. (2011)"

Reference:

Jarvis, P. G., Saugier, B., and Schulze, E. D.: Productivity of boreal forests, in: Terrestrial Global Productivity, edited by: Roy, J., Saugier, B. and Mooney, H. A., Academic Press, San Diego, 211–244, 2001.

Kurz, W. A. and Apps, M. J.: A 70-year retrospective analysis of carbon fluxes in the Canadian forest sector, Ecol. Appl. 9, 526–547, 1999.

Pan, Y., Birdsey, R. A., Fang, J., Houghton, R., Kauppi, P. E., Kurz, W. A., Phillips, O. L., Shvidenko, A., Lewis, S. L., Canadell, J. G., Ciais, P., Jackson, R. B., Pacala, S. W., McGuire, A. D., Piao, S., Rautiainen, A., Sitch, S., and Hayes, D.: A large and persistent carbon sink in the world's forests, Science, 333, 988–993, 2011.

Schlesinger, W. H.: Biogeochemistry: an Analysis of Global Change, Academic Press, San Diego, 1997.

Stinson, G., Kurz, W. A., Smyth, C. E., Neilson, E. T., Dymond, C. C., Metsaranta, J. M., Boisvenue, C., Rampley, G. J., Li, Q., White, T. M., and Blain, D.: An inventory-based analysis of Canada's managed forest carbon dynamics, 1990 to 2008, Global Change Biology 17, 2227-2244, 2011.

Figure 1: The caption refers to 1968 sample plots – but unless each dot includes multiple plots, the figure shows far fewer plots. Please clarify.

RESPONSE: The reviewer is right. There are a lot of overlaps on site locations, because a lot of plots are very close with some others, especially in Foothills and Rocky Mountains regions. This is pointed out in the figure caption.

Figure 2c: The use of a logarithmic scale on the Y-axis is misleading – please revise and replace with regular Y-axis scale.

RESPONSE: We have revised the logarithmic scale of AGB into regular scale (Figure 2).

Figure 3: The text reference to Figure 3 refers to results from the four methods but only 3 maps are shown?

RESPONSE: We didn't include the biomass estimation based on non-spatial regression in Figure 3, because the estimation based non-spatial regression is almost identical to the one based on spatial regression modelling (Figure 3b).