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Discussions

Interactive comment on "An estimate of the terrestrial carbon budget of Russia using inventory based, eddy covariance and inversion methods" *by* A. J. Dolman et al.

Anonymous Referee #1

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Comments on the manuscript by Dolman et al. "An estimate of the terrestrial carbon budget of Russia using inventory based, eddy covariance and inversion methods" submitted to Biogeosciences Discussions.

Overall Evaluation

This manuscript represents a very useful study of estimating the terrestrial carbon budget of Russia in the late 1990s/early 2000s, and is an important contribution to the special issue for the Regional Carbon Cycle Assessment and Processes (RECCAP) activity. The analysis considers an inventory-based approach, and eddy covariance-based approach, an inversion model-based approach, and an approach based on dynamic

global vegetation models (DGVMs). There are several issues that need to be addressed, hopefully in the Methods section with minimal impact on the Results section. There is some confusion in my mind about whether the estimates of the different approaches purely represent land-atmosphere exchange, or whether in some cases they represent changes in ecosystem carbon storage. Also, I don't believe the uncertainties are accurately portrayed (see my comments below). I do think that the Discussion at the end of the manuscript provides a useful discussion of some of the uncertainties, but that there are some additional issues that warrant discussion. Also, I don't think it was justified very well why the DGVMs estimates were excluded from the "best estimate".

I think it would be helpful to organize the Results section into five rather than eight sections that correspond to the four approaches to making estimates plus the methane section. Perhaps rename the eddy covariance section as the "bottom up estimate" section and then have subsections for the components. It would be good to re-organize the Methods section so that it is consistent with the organization of the Results section. Also note that there are some features of the "bottom up" calculation that I didn't understand and need further description in the Methods. Also, some of the definitions in the Methods section aren't consistent with the RECCAP protocols

The writing is a bit rough in places, and I've tried to identify some of the more problematic areas and have provided suggestions for improvement. However, I do think that a revised version of the manuscript will require a careful reading by a good editor prior to submission to improve the written English.

Specific Comments

(1) Abstract. The abstract needs to mention the time period of the analysis. I also think that the abstract would be more effective if it were rewritten and reorganized to more clearly report the central estimates and the uncertainties (for those available) of the four different techniques for making carbon balance estimates. For example, the "best estimate" is reported in the second sentence, but how the best estimate was arrived at

isn't reported until much later in the abstract. The "bottom up" estimate is introduced without being defined. Note that there are numbers reported as Pg C and Tg C, and this is confusing especially when the same estimate is reported as -0.66 Pg C and -659 Tg C. It is not clear why the estimates for fire are reported, as there are many components that could have been reported – it would be good to identify a context. Similarly, the reporting of methane exchange needs context, and it is also somewhat confusing to report at the end of the abstract because it is not clear if the previous estimates included or did not include methane, since carbon dioxide is not mentioned in the abstract. You should probably report the best estimate of -0.66 Pg C per year as the net exchange of carbon dioxide between the biosphere and the atmosphere to be clear. The bottom line is that the abstract needs to be written so that it can stand alone and make sense to a reader without having to get context, etc. from the main manuscript. Finally, avoid ending sentences with prepositions. For example, on line 12 of page 6581, "accounted for' could be revised to "taken into account".

(2) Page 6582, line 6: Change "This could happen" to "This has happened"?

(3) Page 6582, line 14: Neither McGuire et al. (2009) nor Tarnocai et al. (2009) restricted themselves to 1 meter depth. In general the estimates include soil carbon to 3 meters with deeper depths consider in deltas and in yedoma soils.

(4) Page 6583, line 15: The text "on our current analysis" suggests that you have identified the scope of the analysis in this paper. That doesn't occur until page 6584.

(5) Page 6584, lines 16 and 17. The wording of the sentence ending this paragraph is awkward.

(6) Line 6584, line 20: The text about what "DGVM models ignore" seems a bit too absolute in my opinion. I agree with what has been stated when referring to DGVMs in general, but there are exceptions that do consider demography/forest regrowth, nitrogen deposition, and changes in fire regime. Maybe just change the tone to something like "In general, DGVMs do not consider . . ..".

(7) Page 6584, line 25: Change "to give estimates" to "to make estimates"?

(8) Page 6585, equation 1a and 1 b: I don't think these equations are consistent with the RECCAP definitions for land papers: "NEP = net ecosystem production, GPP – ecosystem respiration (or NPP – heterotrophic respiration); NBP = net biome production, NEP +/- all fluxes in and out of an ecosystem or biome, vertical and horizontal." See the RECCAP consistency document. Actually, I don't see why these definitions are needed, as it is the land atmosphere exchange that is important to be reported from LEA. Both the NEP and NBP estimates as defined include export fluxes, which are fine for changes in carbon storage, but are confuse the issue of land-atmosphere exchange. I'm fine with reporting both land atmosphere exchange and change in ecosystem carbon storage, but it seems to me that these haven't been well segregated in the paper.

(9) Page 6587, line 6: "accounting all" should be "accounting for all".

(10) Page 6587, line 7 and 8: The reference to fertilizers and liming didn't make sense to me. Do you mean "production, including the effects of the application of fertilizers and liming"?

(11) Page 6587, line 13: Change "fertilizing" to "fertilization"?

(12) Page 6587, line 19: On my first read through the methods section, I didn't understand how this section fit into the four methods of estimating land-atmosphere exchange. My suggestion is to have this as a paragraph or a subsection of one of the four sections on making estimates of land-atmosphere exchange.

(13) Page 6587, line 24: Change "through" to "to"?

(14) Page 6587, line 27: Change "encouraging of" to " encouragement of"?

(15) Page 6588, line 3: Instead of section 2.3, I suggest that you relabel this section for the bottom-up estimate, and include subsections for the eddy covariance methods and for other components of the bottom up estimate.

(16) Page 6588, line 5: I'm confused by the reference to "Annex I". I didn't see any supplement for download.

(17) Page 6589, line 4: Note that this NEP estimate is not consistent with the NEP as defined by equation 1a on page 6585.

(18) Page 6589, line 6: "data was" should be "data were".

(19) Page 6589, line 8: I believe that the occurrence of "RECCAP" on this line is the first time the acronym has been used in the manuscript, and it has yet to be defined (in fact it is never defined in the manuscript). There should be a reference to Canadell et al. (2011, Eos) the first time it is mentioned.

(20) Page 6589, line 11: "this data" should be "these data".

(21) Page 6589, line 20: "data is" should be "data are".

(22) Page 6589, line 20: The citation of Table 2 doesn't make sense here. Did you mean to cite Table 4? If so, note that the citation is out of order, as Tables 1-3 haven't been cited yet.

(23) Page 6589, line 24: I found the end of this sentence starting with "the sum of all-ecosystem NBP" to be very confusing. Is this the NBP of equation 1b on page 6585? If so, it already includes CO2 emission from fires. If so, then it also includes fluxes of C to the lithosphere and through hydrologic discharge from the region, which aren't directly relevant to land-atmosphere exchange within the region.

(24) Page 6590, line 3: Change "area" to "are a".

(25) Page 6590, line 5. In first reading through this section, I didn't understand how it was directly relevant to estimating land-atmosphere exchange. If it is here for just calculating the change in carbon storage, then please have that be a topic sentence. I would also prefer that it appear under one of the approach sections of the methods so that I can understand in which approach it was used.

(25) Page 6590, line 10: delete "where".

(26) Page 6590, sentence spanning lines 10-12: Note that this method of multiplying discharge by mean concentration assumes that there is a linear relationship between discharge and concentration. Since that is likely not the case, it is likely that there is bias in the estimate produced.

(27) Page 6590, line 19: "(Soja et al., 2004)" should be "Soja et al. (2004)".

(28) Page 6590, line 26: "period on" should be "period at"?

(29) Table 1: So, how does the balance column in Table 1 relate to equations 1a and 1b? Also, there is no indication in Table 1 of the time period of the analysis. Is this for 2009?

(30) Page 6591, line 19: "taken" should be "taking".

(31) Page 6591, line 21: Note that NPP is reported as positive and Hsr is reported as negative. NPP is reported as negative throughout most of the rest of paragraph except for lines 25 and on the remainder of the paragraph on page 6592. I don't have a strong opinion on the sign used in this paragraph, just that it be consistent.

(32) Page 6591, lines 1 and 2: It seems that this sentence about the DGVM NPP results is out of place here and should wait until the DGVM section. Also note that the reference to Table 5 is out of order.

(33) Page 6592, line 22: Note that the change is soil carbon for both Asian Russia and European Russia was equivalent to the change in litter carbon for the 2000-2007 period in Pan et al. (2011), so I'm wondering why soil carbon isn't explicitly mentioned in this sentence.

(34) Page 6594, lines 2, 5, and 13: Change "accounted for" to "taken into account".

(35) Page 6594, line 6: Change "could be" to "should be".

(36) Page 6594, line 22: Change “agents, grass” to “agents, and grass”.

(37) Page 6595, lines 1 and 2: Change “is the estimate of” to “estimates”.

(38) Page 6595, line 14: Change “different” to “variable”.

(39) Page 6596, line 10: Change “neglect of winter fluxes” to “neglected winter fluxes”.

(40) Page 6596, line 14: Change “is somewhat being biased to a CO2” to “is somewhat biased towards a CO2”.

(41) Table 2: Note that the acronym “GLC” isn’t defined in Table 2, and it isn’t defined in the manuscript until page 6597 (line 1) while Table 2 is introduced on page 6596 (line 16). “This large effects” in the legend of Table 2 should be changed to “This largely affects”. Also, I suggest changing “allowing” to “accounting” in the legend of Table 2. Also, what should be the correct sign for NEP? According to the RECCAP consistency document, it should be positive for a sink and negative for a source. It appears here that it is the opposite.

(42) Page 6596, line 17: The “-1.33 Pg C” is not consistent with Table 2, which reports “-1.033 Pg C”. Also, it would be helpful to see a column for the NEP estimates that were on a g C m-2 yr-1 basis so that one could infer the estimate of winter CO2 loss from the atmosphere through comparison to the corrected NEE estimate.

(43) Page 6596, lines 20-22. I’m confused by the comparison of LEA to the eddy covariance estimate. When I subtract the NPP minus Hsr estimates in Table 1, I get -1.30 Pg C yr-1 as the LEA estimate that should be comparable with the -1.03 Pg C yr-1 eddy covariance estimate reported near the bottom of Table 2. It would also help to sum up the forests in Table 2 so the sentence spanning lines 20-22 could be better evaluated through direct comparison of Table 2 to Table 2 (but one would still need to calculate NPP minus Hsr for forests in Table 1.

(44) Page 6597, line1: Change “Gobal” to “Global”.

(45) Page 6597, line 9: The logic of this sentence is lost on me. You know the details of the methodologies used to produce the estimates. You must be referring to something else here.

(46) Page 6597, line 11: Again – I’m not quite sure why River Export is here. As far as I can tell it is not included in the estimates in Table 1 or Table 2. If the point here is to compare the atmospheric exchange with the total change in carbon storage, then I’m fine with that, but that does not seem to occur in this manuscript.

(47) Page 6598, line 8: I’m assuming that land use change was already included in the LEA estimate, or am I mistaken. In any case, the estimates of this component should occur under the approach(es) where it is reported in the manuscript.

(48) Page 6598, line 10: Change “while” to “when”.

(49) Page 6598, line 11: Change “this present a” to “this presents a”.

(50) Page 6598, lines 24 and 25: Change “Central Asian former SU territory” to “Central Asia”.

(51) Page 6599, lines 20 and 21: Change “most realistic area abandoned arable lands” to “best estimate for the area of abandoned arable lands”.

(52) Page 6600, line 1: Again – it is not clear to me why there is a need to calculate the harvest and export-import of wood. I could trying to correct the “NEP” estimate of the eddy covariance for the losses to the atmosphere associated with harvest (I assume that this has already been accounted for in the LEA estimate of net exchange reported in Table 1, but perhaps not). From my perspective, what needs to be accounted for is the loss of carbon to the atmosphere from wood harvest activity and the decomposition from total wood products. The balance between import, export, and harvest influences the size of the wood products pool. I think that the relevance of this section needs to be better explained in the methods section with some equations for how this calculations reported in this section are to be used in either calculations of land-atmosphere

exchange or changes in regional carbon storage.

(53) Table 4. In Table 4 your report the estimates of CARBONTRACKER as starting in 2000. My understanding was that they didn't start until 2001.

(54) Page 6601, lines 3 and 4. I thought that it was agreed at the May 2011 RECCAP meeting that the uncertainty estimates from inversion models and from DGVMs should be the range and not the standard deviation (or standard error). The reason for this, as stated by Martin Heimann at the meeting, was that any of the models could be right and therefore if they are included in the analysis, then the range needs to be used as the estimate of uncertainty. This is what has traditionally been done by the inverse modeling community for among model uncertainties, and I don't think you should use the standard deviation to define the uncertainty. Note also that the "range" is not equivalent to "the standard deviation" as is implied on line 4. The range is the full spread of the model estimates.

(55) Page 6601, line 12: The sentence that starts with "For the entire Russia 4 different inversions" is very awkwardly written, and needs to be revised.

(56) Page 6601, line 17: "biospher " should be "biosphere "

(57) Page 6601, lines 22 and 23: I have no idea what is meant by "is determined by a steady assumption". Do you mean "steady state"? If so, it still doesn't make sense to me. Also, what is meant by "From other side"? What "other side"? In this paragraph, I don't think it is that useful to point out the shortcomings of DGVMs, as this is a results section. I recommend saving the discussion of these shortcomings for the Discussion section and perhaps using them as a reason for excluding the DGVM-based estimate from the "best estimate".

(58) Page 6602, line 4: Do you mean a "small sink" instead of a "small source". It is not clear from Table 5 because of possible confusion over signs, but Figure 6 suggests it is a sink.

(59) Page 6602, line 9: Why do you conclude that the DGVMs are overestimating Hsr and that LEA is not underestimating Hsr. The simulation of Hsr is a very uncertain aspect of both approaches.

(60) Page 6602, line 14: This bottom up estimate methodology should be described more fully in the Methods section. I don't totally understand how outflow and trade play into this estimate of land-atmosphere exchange.

(61) Page 6602, lines 11 and 12: I don't understand the logic that leads to the conclusion that "NPP is increasingly allocated into more stable pools". If anything, the higher estimates of Hsr by the DGVMs would suggest that NPP is being allocated to labile pools in comparison to the LEA approach.

(62) Page 6602, line 21: In this paragraph I think you are mostly focusing on anthropogenic methane emissions, and so I suggest changing "total emissions" to "total anthropogenic emissions". Also, "EDGAR" is not defined and there is no reference to a paper in which EDGAR is used to produce the reported estimates.

(63) Page 6603, line 6: Change "methane flux are very diverse" to "methane flux from Russia are very diverse"?

(64) Page 6603, line 8: The sentence that starts with "More recent" is a mouthful, and I really don't understand it after reading it several times.

(65) Page 6604, line 1: Change "and indication" to "an indication".

(66) Page 6604, line 4: Note that McGuire et al. (this issue) shows the opposite tendency, i.e., the approaches tend to show an increasing sink in Arctic tundra.

(67) Page 6604, line 6 and 7: I don't think the issue is so much the magnitude in fire emissions, but more how fire emissions have affected the stand age distribution in these studies. That is, it is assumptions about how the fire regime has changed that leads to large effect of fire in Hayes et al. (2011), not the levels of emissions per se (which differ on the order of 70 Tg C yr-1) from the estimates of LEA.

(68) Page 6605, line 19: Change "still may fortuitous" to "may be fortuituous".

(69) Discussion of "best estimate". Note that the best estimate is only presented in the abstract and is not presented in the Results or Discussion sections. There needs to be at least a paragraph in the Results or Discussion devoted to the reporting the best estimate, and the justification of the methodology for coming up with the estimate.

(70) Discussion of uncertainties. I think you need to discuss key uncertainties of each approach used more thoroughly. The key uncertainty of the LEA approach in my opinion is its calculations of decomposition from wood, litter, and carbon in organic and mineral soil horizons. The key uncertainty in the eddy covariance approach is the lack of replication. You've identified a number of uncertainties in the DGVMs. The key uncertainty in the inverse models is that they may not be well constrained over this region. Dargaville et al. (2002; Estimates of large-scale fluxes in high latitudes from terrestrial biosphere models and an inversion of atmospheric CO2 measurements. Climatic Change 55:273-285) showed that at least one inversion approach could not substantially modify the priors through the ingestion of data from the atmospheric monitoring station. I'm suspecting that it is still the case. So – the bottom line is that there is substantial uncertainty in each of the approaches, and I don't think that has been adequately captured in this manuscript. Given this level of uncertainty, I do think that the agreement among the approaches is "fortuitous" , and that the uncertainty is much larger than 100 Tg C yr-1 reported in the last sentence of the manuscript.

Interactive comment on Biogeosciences Discuss., 9, 6579, 2012.