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 #2

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General comments This paper provides estimates of the carbon balance of Russia based on three forms of measurement (inventory [LEA], eddy covariance and atmospheric inversion. Calculations are also presented for 8 DGVMs but these are not used in providing the final overall estimate of the net carbon balance. Each of these has quite different properties and different levels of uncertainty. The inventory approach gathers together a very diverse set of ground-based measurements covering many aspects of vegetation and soil, supported by some remote sensing information. The diversity of these measurements and lack of information in the paper on their individual properties (number, period and location of measurements, selection bias, methodology, known limitations) makes it hard to judge the uncertainty in the individual components. Eddy

covariance measurements are available only at a very limited number of sites but are taken to be representative of behaviour, by biome, across the whole of this vast area, which is a heroic assumption. It is therefore not clear how to assign any realistic uncertainty to the estimate based on this method. Atmospheric inversion (12 schemes) measures only part of the carbon budget and needs to be supplemented by other information to give the full budget. In this case, estimates from individual schemes normally come with some measure of uncertainty but these are not exploited in this paper. The individual DGVMs are controlled by climate and internal parameters and their calculations do not have any attached uncertainty.

The paper concludes that in recent years (the LEA specifically normalises to 2009) the Russian biosphere has been a net sink of 0.66 PgC/yr, primarily in forest ecosystems, based on averaging the measurements from the three approaches. The paper states that these three measures agree well within their error bounds, but it is hard to find in the paper any clear statement of what these error bounds are. Nonetheless the fact that they all give estimates of comparable magnitude lends some confidence that the estimated mean found by averaging the different estimates is credible. Nonetheless, the paper is unable to provide any uncertainty or confidence interval on this mean.

There is a limited amount of discussion on methane emissions, but this does not lead to any firm conclusions on the size and uncertainty of this GHG source.

The paper gathers together probably the most comprehensive set of available information on the carbon balance of Russia to arrive at its conclusions, and cites many relevant sources. It is therefore a valuable contribution to the literature.

Specific comments

p. 6584 talks about the importance of discussing uncertainties in a comprehensive way, but the paper indicates little attempt to develop a comprehensive error analysis even for the LEA, and the presumably available uncertainties in the atmospheric inversion are not used. Admittedly this is a difficult problem that bedevils analyses and comparisons

of this sort, but it would be useful to give a clearer picture of likely errors (biases) and errors bounds in the various terms, and how they combine.

The signs of fluxes are not consistent or are confusing throughout the paper. In most places whether a flow is to or from the atmosphere can be inferred from knowledge or clues in the text, but not everywhere. For example, on p.6591, NPP is given as 4.76, which is normal, but HSR as -3.46 (it would be more normal to have a + sign on HSR, particularly since large parts of the rest of the paper fluxes are take positive to the atmosphere; also see Table 1). Below, NPP is taken as negative. Also p.6596, l.18-20. Also p. 6599 top. Accumulation is positive above, then negative below, but these all seem to refer to sink terms. This whole section is inconsistent in terms of sign. Similarly p. 6600, export – import given as negative so import exceeds export, but not according to the text. Top p. 6601 "atmosphere to land flux" Last line of Section 3.7: do these – signs refer to a source or sink; it's completely unclear?

p.6592, l.27 refers to a DGVM estimate, which presumably is the mean of the DGVM values (not stated), but nothing is said about the variability of the DGVMs about this mean

On p.6586 it notes that estimates of NPP do not include C fertilisation and no deposition. It's not clear how these effects can be removed from inventory-based approaches, which rely on ground-based changes in C stocks or fluxes.

There are several places in the text where there seem to be numerical disagreements or errors. Examples: (a) The abstract and text quote IAV in fire emissions of 0.5 to 3, but this does not fit with the results presented in the text or Fig. 3, unless it refers to GFED. (see p. 6593) (b) P.6581, l. 25: the %s do not sum to 1. (c) p.6595, l.16: these uptake values are not consistent with figure 4. (d) Table 2 contains several things I don't follow. If the corrected NEE is for forest age structure, how does it affect grassland etc? How does one square the value -207 with the bracket below for pine? What to the totals under the first two columns mean – the first is certainly not the sum of the terms above.

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(e) I can't reconcile the values given in the text for the mean or particularly the SD calculated from table 4. I get a substantially larger SD , even omitting the outliers (GEOSTAT and NMATCH) – see p. 6601; i can see no basis for the quoted SD of 130 Tg/yr. (f) p. 6596, l. 18: should this be 1.033? () 6602, l.10: these ratios are inverted.

p.6592, l.26: which way is this difference , + or -?

p.6593, l.24: it's not clear where the value 25% comes from. If the uncertainty in burnt area is 9%, this implies an uncertain in the "emissions factor" of approx. 16%. How is this arrived at?

What is the reason for the flux due to insects and disease (p. 6594); is this not double-counted in terms of dead wood (DEC in eq. 1)?

p. 6596 makes a hypothesis about the connection between growing season length and variability of NEE, but the data allow this to be tested; why is this not done? Also, I could not see the reason for the statement about zero NEE. The curves mainly have reached an asymptote, so is this a statement that they would be expected to decline if more data later in the year had been included.

The next section discusses then lower value in Table 2; where does the upper value come from?

p. 6601, section 3.7 The DGVMs play little part in this paper. Early on it comments that they are used to provide insight into the mean fluxes and their IAV, but the body of the paper does not show much evidence for this. Indeed, what the paper does indicate is that their estimates of heterotrophic respiration significantly exceed those from ground-based measurements embedded in the LEA. A similar conclusion was drawn in Quegan et al (2011), but in this paper they indicate that the over-estimate is at high latitudes, whereas Quegan et al. found that it mainly occurred at lower latitudes in Siberia (as would be expected, not least because the fluxes are much larger there). The paper says that most DGVMs use potential vegetation, but many of them use

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prescribed vegetation taken from satellite data. Table 5 does not quote any measure of variability within or between the models, and it's unclear what IAV means in the Table; is it some average IAV? Given the discrepancy between data and models regarding HSR, how meaningful is it to conclude that NPP is increasingly allocated to more stable pools? More cogently, do the DGVMs provide any useful addition to this paper, given their known limitations on HSR, representation of fire, land cover, etc? this section also refers to the atmospheric inversion estimates of NPP, but they don't measure this. Why is the last para in this section?

Section 4. This discussion section is not very satisfying and largely consists of not well-developed comparisons with other studies. What is very much lacking is a synthesis of the uncertainties in the results developed in this paper, and how the work of other authors contributes to our knowledge of these uncertainties.

6603, l.20: the remark about not extremely large in absolute terms is meaningless; what is it being compared with?

6604, l. 5: is the ref to McGuire relevant, as most DGVMs do not include permafrost?

The section about length of growing season seems very much out of place in this section, and should probably appear in the earlier discussion of this issue concerned with eddy covariance.

The commentary on the various Russian studies of the C balance is not particularly helpful, largely just noting that they get different results from the LEA method. I've no idea what the words "consistent " means in l. 26 – consistent with what?

6605, l. 23. I really see no basis for the quoted uncertainty of 100 TgC/yr, since we have no idea of the SDs of the individual estimates. It is not valid to use the SD of what appears to be just 3 numbers to represent the uncertainty in the overall estimate.

Technical corrections

p. 6583, l.12: should be atmosphere to land; also p. 6595, l.12 p. 6584, l.14: change

"If" to "To be " I can't find the Annex 1 referred to on p. 6588. p.6588: definition of NEE needed, particularly its sign; in normal usage it would have the opposite sign to NBP In table 3 does "load" mean "flux" p.6589, l.20 Table 2 does not do what it says; nor does table 4 which simply lists the inversion schemes 6590l3, change "Next" to "In addition", change "area" to "are" p.6599, l.1: modify "changes" to "increases" 6603, l.9: Change "If the" to "While"; l.18, change "served" to "provided" and remove "at" in this line; should it be -0.8?

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