Response to Reviewer 1 Comments: Reviewer comments are in **bold**, our response in normal text.

Tile:
I think you may replace carbon by Carbon dioxide, because you do not include Methane

Agreed. Revised title now reads: North America's net terrestrial CO_2 exchange with the atmosphere.

Abstract:
Line 9: North America: it would be nice to give the geographic boundaries, also in the text. Later you refer to TranCom3, but I bet, most potential readers not even know what TransCom is. I guess, Mexico includes the tropical southern part up to the boarder of Guatemala. Canada includes the arctic up to the Arctic Ocean? Is Greenland part of North America?

Revised line 6-7 now reads "….for North America (Canada, United States and Mexico) over the period 1990–2009."

Also, the abstract should include a quantification of the total area, and its components of countries and land-use. Maybe you need a separate table in the methods.

We believe the details on area etc. would be too much for the Abstract, but is clarified in the text with the addition to p. 11030, line 29:
The North American land area (21.748 106 km2; Canada = 9.985 106 km2, US (including Alaska, excluding Hawaii) = 9.798 106 km2; Mexico = 1.964 106 km2) is approximately 16% of the global land area (excluding Greenland and Antarctica). North America's net land-atmosphere exchange is thus a potentially important fraction of the global land sink for atmospheric CO_2.

Line 19: Presenting a ratio for source/sink is unfair. I think, at the least, you must present the numbers on which these ratios are based. I would request that instead of writing 4:1 you write 2000:500 or something like this.

We, respectfully, have a difference of opinion on this point, but recognizing the difference of opinion, the revised last line of the abstract now reads:

With North America's mean annual fossil fuel CO_2 emissions for the period 1990-2009 equal to 1720 Tg C yr^{-1} and assuming the estimate of −472 Tg C yr^{-1} as an approximation of the true terrestrial CO_2 sink, the continent's source:sink ratio for this time period was 1720:472 or nearly 4:1.

The abstract should also make clear, that this is a CO2 balance and not a carbon balance, because it does not include methane. Also, N2O was not considered, and we know from Europe, that the GHG-balance changed the CO2-sink into a GHG source.

Line 7 of the abstract is revised to read:

"…2009. Only CO_2 is considered, not methane or other greenhouse gases. This synthesis is based on results from three different methods: atmospheric…"

The abstract should mention also, that trade, fire, and arid regions are included as variable fraction of each model (as far as I understand).

A sentence is added to the abstract after line 12 reading:

This relatively large range is due in part to differences in how the approaches represent trade, fire and other disturbances and which ecosystems they include.

Introduction
I am missing some aspects in the introduction:

- it must be mentioned that CH4 and N2O (and other GHGs such as NO and CO) are not included. This balance refers to CO2 only (including oxidation of methane in the atmospheric approach, which is important in view of fracking)

Line 27, p. 11030 has been revised to read: "…net land–atmosphere CO2 exchange…"

And the following clarification has been added to the end of the Introduction:

This study focuses on estimates of land-atmosphere CO_2 exchange over Canada, the United States and Mexico. Although the inventory approaches included in this study are based on total carbon changes, we do not report flux estimates of other carbon gases such as methane and carbon monoxide or N_2O and other greenhouse gases. This study is a synthesis of the net contribution of the North American land surface to atmospheric CO_2 concentrations and is neither a carbon nor greenhouse gas budget for the region.

- The introduction should refer to the Global Carbon Project map (2013) which depicts North America and China as the main emitter world wide.

The reviewer's intent is not entirely clear. We have inserted the following sentences referring to the GCP map at line 29, p. 11030:

In 2013, fossil-fuel and cement CO_2 emissions from North America (Canada, United States and Mexico combined) were second only to those from China amongst other countries and regions of the world (Global Carbon Atlas, 2013; Le Quere et al., 2014). Quantifying North America's net land-atmosphere CO_2 exchange, potentially offsetting at least a portion of North America's CO_2 emissions, is an important element of understanding and quantifying North America's contribution to the accelerating increase in atmospheric CO_2 concentrations (LeQuere et al. 2014).

- The introduction should mention that the present analysis focuses on forests and croplands only, neglecting grazing lands, arid land and tundra for the land-based approaches. This is important in view of the recent publication by Poulter (Nature 2014), which shows that the arid lands (of the southern Hemisphere) cause the variability over time of the global terrestrial sink. Neglecting the arid lands in the land-based approaches, but including them in the atmosphere based approaches may lead to an important bias, because the effects of ElNino are clearly visible in North America.

We do not disagree with the reviewer's characterization of the potential importance of arid lands. However, the first statement is true only of the inventory-based approach, and even there we do account for tundra. The TBM models do represent arid lands, they may not represent them correctly, and particularly the influence of interannual variations in moisture as highlighted by Vargas et al. (2013)

and Poulter et al. (2014), but they are included in those models. Because of those "complications" we think it best not to treat those differences in the introduction but in the methods, results and discussion (see below).

Methods

I thank the authors that they try to clearly indicate the inclusions and exclusions of fluxes to the different methodologies, and maybe one could think of a cartoon to make this even clearer. Basically, not the length of the observation period (see discussion), but the differences in including or excluding different component fluxes, cause the numbers to be different.

We did not explicitly state that differences across approaches were due to the length of the observation period (we did refer to the length of the observation period with respect to discerning within approach trends over time), nor did we intend to imply such. We have hopefully clarified that point in responding to the reviewer's corresponding comment on the Discussion section. We agree that the differences across approaches are in large part due to differences in the inclusion/representation of component fluxes, a point we make in the discussion section. We have also added a schematic as the new Figure 1 to clarify what is included and excluded in the different approaches. The following text introducing that Figure is added at end of paragraph line 16, page 11031:

More generally, the different approaches include and exclude different contributions to the net land-atmosphere exchange (Figure 1). Those differences are likewise important in interpreting and comparing results and are described in the respective sections.

Page 11031

The references to line 11 of page 11031 here are not clear; the comments do not appear to correspond to the page and line. We reply to the comments here as presented with revisions to text where we believe they are most appropriate.

Line 11 fossil fuel emission: I think you should include a paragraph on fossil fuel emissions, including methane burning (does methane include bio-generated methane, and diesel, and gasoline, which is 10% (?) of the energy burning?). CO2 does include oxidation of Methane, even if it came from fossil sources (fracking), and this contribution has increased in the last decade.

In response to the reviewer's questions:

Bio-generated methane is not fossil fuel methane and is therefore not included in the fossil fuel accounting. Carbon from gasoline and diesel is fossil-fuel accounted.

Methane generated from fracking is included in the fossil fuel accounts as per official government statistics. However, the last year of this two-decade study is 2009, prior to most of the current fracking boom and when CH_4 emissions through this process have likely increased.

You may have to discuss whether an annual budget (Table 3) or a more refined fossil fuel emission model is needed (Ciais, Global Change Biology 2010). Since the atmospheric model runs (I assume) on

a 30-minute time scale, a bias may occur with respect subtracting fossil fuel emissions on an annual scale. I think the same bias is true for the TBMs.

For our purposes here, the annual budget of fossil fuel emissions is appropriate for comparison with annual aggregation of the original finer temporal scale results of the AIM and TBM, especially with the decadal averaging. Just how the AIM and TBM originally interfaced with fossil fuel emissions is beyond the scope of the current synthesis and not, in our opinion, critical to our results/discussion.

I also think a consideration must be made to point sources of power pants as well as of large cities. There were several very important US-based publications since 1990 on this topic.

The effect of point sources and urban areas depends on exactly how the AIMS and TBMs were run. If a subtraction occurred than it depends on which fossil fuel CO2 distribution was used. If the models were run with a fully coupled atmosphere, again, it depends on which distribution was used. Ours is a synthesis of a number of atmospheric inverse flux estimates. Methods for each are published. There are many issues with these estimates. Their treatment of point sources is one of them. The community doesn't have an atmospheric network that can solve for fossil fuel emissions independently. Assuming that they are essentially "known" as compared to biogenic fluxes is probably one of the safer assumptions in the inverse flux estimates. Again for our purposes here of synthesizing a number of inverse flux estimates along with estimates from different approaches, explicating those issues is beyond the scope of our objectives.

Line 11: Trade of grain, wood and fiber emerges in each of the models

All approaches are affected by trade of biomass (grain, fiber, wood), and it would be nice to know, which components are included, and the magnitude of these fluxes. I may refer to Ciais et al (2008) Biogeosciences 5, 1259ff. I mention this, because there are differences between countries. As far as I know, most state forests are not managed in the US, but the US imports most of its construction wood (as saw wood), Canada exports round wood to China.

The following sentences have been added to the end of the paragraph at line 16, p. 11031:

Lateral flows of carbon as they ultimately influence vertical exchange with the atmosphere, including the trade of grain, wood and fiber, are an important consideration in interpreting and comparing each of the approaches. The respective treatments of lateral fluxes in each of the approaches are discussed in the corresponding sections below.

Line 11: Sorry for another methods section: Fire and diseases

You basically exclude fire except for AIMs, but fire and insects changed Canadian forest from a sink into a source (Kurz et al). I think, you need to state the total area burnt in the TransCOM region. Also, it would be nice to know, how much area was affected by forest diseases in this period (Mountain Pine beetle, Gypsy Moth and others). These areas are now re-growths, and contribute to the land-based sink more than if these outbreaks would not happened?

Treatment of fire and disease (disturbance more generally) is an important component in understanding differences within and among approaches. Fire is included in some of the TBMs and at least implicitly in the inventories as it manifests itself in altered carbon stocks and even explicitly in the case of the Canadian forest inventory accounting. We believe discussion of the contribution of disturbance and differential treatment is most appropriate to, and is included in, the respective methods subsections and the discussion.

Page 11032,

line 1: I think it would be fair to refer to NatureGeoscience Vol 2, 842ff (2009), where the top-down and bottom-up approach was used for the first time, and where the definition of fluxes were clearly depicted. Maybe, extending this flux scheme would help to clarify the differences in the approaches, which are discussed in the following.

Added citation of Schulze, E. D., Luyssaert, S., Ciais, P., Freibauer, A., Janssens, I. A., and et al.: Importance of methane and nitrous oxide for Europe's terrestrial greenhouse-gas balance, Nature Geosci, 2, 842-850, 2009.

Line 1: I think, you need to clearly say, that all numbers, also the atmospheric numbers, do NOT include fossil fuel emissions

Inserted the following sentence in line 1, page 11032: In estimating net land-atmosphere exchange, the influence of fossil-fuel emissions are assumed to be well-known and their influence is removed from the problem prior to solving for non-fossil fluxes (Peylin et al., 2013; Schulze et al., 2010).

Line 4: I bet, most readers do not even know of TransCom. I think you must give the boarders of the region, and the areas and the land-use

We have added a new Figure 2 of the TransCom regions taken from http://transcom.project.asu.edu/transcom03_protocol_basisMap.php

Surely, I do not want to be prescriptive, but for clarity in the RECCAP environment, it could help, to include a cartoon, such as the flux bar of Figure 2 of the NatureGeoscience (2009) publication, to show, which fluxes are covered by AIMs (and by the other approaches).

We have added a schematic as Figure 1 to clarify what is included and excluded in the AIM and other approaches.

Page 11033

Line 7: It is not only fire but also Insect outbreaks

End of sentence on line 8 is amended to read "…CO2, or the influence of insect outbreaks.**"**

Line 8:Trade needs to be mentioned.

Trade is included in the lateral transport of that sentence. Text has been amended (line 7) to make that clear: "...include the trade and lateral transport…"

I also think, that it must be stated, that the TBMs ignore CH4 from Range lands (Cattle) and N2O from agriculture and Soy bean plantations.

Following sentences added at end of line 8: These models, as a class, also generally ignore CH_4 emissions from livestock and N_20 emissions from agricultural. But these absences do not impact our estimate of net land-atmosphere CO_2 from these models.

Line 17: It would help the reader, if a cartoon would illustrate the fluxes which are included and which are neglected in the TBMs

We have added a schematic figure to clarify what is included and excluded in the TBM and other approaches.

Line 18: Inventory based approach: In Table 4, you are dealing with n=4 taking the two inventory-based approaches as independent estimates. I suggest that you separate these two approaches already in the methods. Right now, you have 13 lines to present AIMs, but you take 4 full pages to discuss the inventory based approach. To my knowledge, this is the first time, that these two approaches are discussed to this detail, and I think, it would make the paper even stronger if you would capture this in different titles.

Substantial portions of the inventory approaches have been presented in Hayes et al. (2012) and the Appendix: Supplemental Information of King et al. (2012). However, the two variations on the inventory-based approach yield noticeably different results, thus the substantially longer Methods section. We considered carefully the reviewer's suggestions for separating the different variations into two approaches but concluded that the comparison between the two approaches worked better and avoided repetition with them interwoven. However, we believe that revisions in response to the reviewers specific comments on the text below along with a few additional revisions have helped with the readability of that section.

Page 11034
Line 1 and 22: The inventory approach excludes trade. You need to say this.

In the Production Approach trade is included – in that the emissions from HWP produced from wood harvested in the reporting country are reported regardless of where they occur. In the Atmospheric Flow approach, all HWP emissions that occur in the reporting country are reported by that country, regardless of the origin of the harvested wood, including that from trade. For this study, the application of the Atmospheric Flow approach in Canada does not include the emissions from HWP imported to Canada but relative to exports, imports are small. See response to following comment.

To clarify, line 2 has been amended to read "…changes in product pools, including those resulting from trade, are considered…"

Page 11036:

Line 6: I think you have to repeat in the brackets that exports are small for Canada only. I am not even sure about this. Canada is a big exporter of grain to the whole world, but also of round wood to China, and of wood pellets to Europe. (See IPPC special report on renewable energy).

The indicated text states that Canadian *imports* are small relative to exports, not the reverse as suggested by the review comment. For clarity and in response to the reviewer comment which follows, a paragraph break has been inserted at line 4 and the rest of that paragraph replaced with:

Both the production and atmospheric flow approaches were used to estimate contributions of HWP to Canadian and U.S. carbon fluxes. In the atmospheric flow estimate for the U.S., the HWP stock change calculations from the production approach (Skog, 2008) were adjusted for both imports and exports from international trade (USEPA, 2012). For Canada, however, the atmospheric flow estimate includes

only exports; HWP imports to Canada are known to be very small relative to exports and are not tracked. As noted above, data on changes in HWP are not available for Mexico, and therefore the contribution of HWP is not part of the estimate of carbon fluxes for Mexico.

Line 6: You may have to separate Canada from the US by a paragraph. The situation is different from Canada in the US. To my knowledge, you export grain and methanol and bio-diesel, and you import saw wood, because you do not manage your state forests. This is a big bias in the source-sink discussion. Your forests are sinks because of no harvest. I am not sure, if my generalization (based on IPCC) is true, but it needs clarification. E.g. how much of the US forest area is under forest management harvesting wood?

See response to above comment. The statements that "you [the US] do not manage your state forests" and "Your forests are sinks because of no harvest" are unclear, but on the surface appear to be incorrect.

Page 11037

Line 6 and 16: I think you need to discuss the role of arid lands (and range land). How much area are they (including Mexico). These lands respond heavily to rain, which would be seen by AIMs but not by your inventory approach. Again a flux cartoon would help to make clear, what the limitations are.

The list of examples of excluded ecosystem types has been amended to include arid lands (distinction between grasslands already mentioned and range land is unclear). The following sentences have been added to end of paragraph line 16:

Arid lands generally have low carbon stocks, but in wet years or decades could be an additional sink (Poulter et al., 2014) or source (Thomey et al., 2011) missed by the general exclusion of these lands from inventories. Similarly, a potential contribution to the North American sink is missed by the absence from the national inventories of woody encroachment into previously non-wooded lands (Hayes et al., 2012; King et al., 2012).

We have added a schematic Figure 1 to clarify what is included and excluded in the inventory-based and other approaches.

Line 27: I thought that the Monte Carlo permutations were the state of the art for estimating the confidence limits (e.g. Global Change Biology 16, 1462, 2010).

We fully agree that Monte Carlo permutations and bootstrapping can be used to estimate the distribution of values (e.g., in model results with parametric uncertainty analysis or bootstrapping sampling statistics) but there is no consensus and is rarely done even as a bootstrapping when characterizing multi-model ensembles. But even then there are still differences of opinion and variety of uses in characterizing uncertainty with various estimates of that variability (for example, with the range, standard deviation, one or two standard error, or confidence limits/intervals.

Page 11038:

Line 10: I think you miss out on the inter-annual variability because you exclude range land and arid lands.

We have amended line 10 to read “The Canadian GHG inventory…” and added the following sentence to line 13: Similarly, the inventories exclusion of arid lands and range lands means that these approaches also miss interannual variation associated with temporal patterns of precipitation in those regions (Poulter et al., 2014).

Page 11039:
Line 6: I guess the AIMS are corrected for fossil fuel?

Yes.

Line 9 to 13: I think this is a bit too much “hand-waving”. First, you need to align the two approaches with respect to trade. AIMs includes trade, TBMs not? Second, The TBMS may be totally driven by arid lands, which you excluded, and by increasing irrigation in croplands (how much did irrigated land increase since 1990?). In fact, I think the area of crop-land increased in the US since 1990? This points at the necessity to quantify the change in land-use since 1990 (crops, rangeland, forest, protection)

The reviewer’s reference to “hand-waving” is unclear. Our comments here refer only to an across-approach synthesis, and our effort to combine these otherwise disparate approach-specific estimates, which we do consider to be individually credible approaches. If a reader is uncomfortable with our “best” estimate, and we believe it is clear we mean that only in the sense of any central tendency across methods, then the reader can examine the results of the individual approaches that are presented.

Our objective is not to reconcile the differences, the reader mentions several plausible reasons among the many possible explanations, but to compare the estimates from the different approaches for North America for the RECCAP periods, and through that comparison point out the need for reconciliation. We very much agree reconciliation is needed, as evidence by the relatively broad range of results, but beyond pointing out in the discussion section where the methods differ in major ways (e.g., in disturbance), differences that might be viewed as hypotheses to pursue in reconciling the methods, that reconciliation is beyond our scope and objectives.

We believe the specifics of the reviewer’s comment more appropriate to the discussion section, and we have revised the discussion section to expand upon the differences in fluxes represented among approaches in response to this and another reviewer’s comments. Specifically, here: 1) True, the AIMs do see the effect of trade while that is not represented in the TBMs as noted in the Methods section, and that potential affect is included in the revised discussion. 2) True, TBM’s may be misrepresenting the carbon of arid lands (although they are included in those models) and they are not included in the inventories as noted in the revised methods and discussion. 3) Changes in cropland area and irrigation that were expressed in changes in carbon stock in croplands would be captured in the cropland inventories, but not by those TBMs that do not consider land-use change and perhaps incorrectly by those that do. Quantifying changes in land use since 1990 is critical in assessing any trend in net land-atmosphere exchange over the period, but we do not attempt to do so here as that is outside the scope of this manuscript.

Line 29: The figure legend depicted inside the box of Fig 2 is not clear. "sum of all countries" means "global emissions". At the first glance I thought that this is the sum of North America. What is the dashed line?

The legend has been simplified to "Global Emissions". The dashed lines represent the decadal mean of emissions, and the Figure caption has been amended to include the description of the dashed lines which appeared in the text, but which we failed to put in the figure caption:

Figure 2. Fossil-fuel-CO_2 emissions for various political units. Solid lines represent annual emissions and dashed lines represent the decadal mean of emissions. The sum of countries is used to represent total global emissions in this plot. This allows comparison of emissions on an equal basis as all emissions are based on apparent consumption data and not production data (see Andres et al. (2012) for a fuller discussion of the differences). The global values used here are less than those in the CDIAC archive (http://cdiac.esd.ornl.gov/trends/emis/tre_glob_2010.html) mainly due to the exclusion of bunker fuels. Data from Boden et al. (2013).

Page 11040

Line 3 and 10: I think it is unfair to hide behind China. This is a fairly recent event. The US was number 1 in the 1990ies. Maybe you refer to the Global Carbon Project map

The sentence line 6-9 has been replaced with:
In terms of mass emitted globally in calendar year 2010, out of 216 countries, the US is the second largest emitter, Canada is ranked #9, and Mexico is ranked #13. Prior to 2006, US emissions ranked #1; thereafter China has had the largest emissions (Le Quéré et al., 2014). In 2010, North America as a whole is ranked #2 behind China. For the period 1990-2009, uncertainty (in Tg C yr-1) was higher in Mexico (~10% of mean), lower for Canada (~2% of mean) and substantially lower in the US (~0.02% of the mean) (Table 3).

Line 17: I do not agree with the statement that the uncertainties are due to the short record. The reasons, as stated in the methods, are clearly the differences in including some fluxes in AIMS and others in TBMs (trade, fire, insects, Methane conversion, arid regions, and others). I think it would be nice, if you could honestly state, that despite 20 yrs of work, the associated industrial fluxes remain obscure, and that more emphasis should be given to range lands (cattle) and arid lands (not only in North America!!!!).

The text is perhaps not as clear as it should be since we do not say, nor did we intend to imply that the uncertainties in the differences among approaches were due to the short record. The reference here is only to the ability to detect a trend over time in the sink relative to emissions given the short record. The text, line 17, has been amended to make that more clear:
…Table 2, the relatively short record means any apparent change over time in the sink strength relative to fossil fuel emissions …

We agree that the differences in included and excluded fluxes are in large part responsible for differences among methods and while we cannot go as far perhaps as the reviewer wishes, we have added the following sentences to the discussion as a paragraph before the paragraph beginning line 18, page 11044:

Differences in the treatment of trade, fire, insects, land-use change, methane and methane conversions, and arid regions are among the many possible contributions to differences in estimated net land-atmosphere exchange among and within the approaches. Years of research have provided information on these various components, but no single comprehensive, integrated, agreed upon treatment of them in their entirety exists for attribution of the net flux estimated by the AIMs, to guide national carbon inventories, or for implementation in TBMs. Efforts to resolve differences among approaches and specific attribution of the North American sink will likely require a community effort to test specific hypotheses involving, initially at least, one or a very small combination of these components. Recent indications by Poulter et al (2014) of the influence of arid lands under El Nino conditions combined with the uncertain contribution of woody encroachment to the North American land sink (King et al., Hayes et al. 2012) suggest more attention to woody biomass changes in arid and semi-arid environments as a promising area of investigation. This attention might include focus on these lands and dynamics in an inter-model comparison of TBMs or structured synthesis and perhaps additional observations of carbon inventories for these regions.

Page 11041:
Line 4: But Canada changed to be a source due to fires and diseases (Kurz). I cannot believe this sentence.

The reviewers comment relative to the page and line number cited is unclear. We ask for clarification. Perhaps our response to the reviewer's following comment applies here as well.

Page 11042:
Line 21: I think the Canadian situation needs to be discussed. Kurz published an important paper that Canada changed from a sink to a source.

We have added the following paragraph after line 21, p. 11042:
We have made no attempt to resolve temporal trends in the estimates of net land-atmosphere exchange due to the relatively short time frame. However, Kurz et al. (2008) found that Canada's managed forests switched from being a GHG sink to a source in 2002 as a result of large insect outbreaks, and those forests have been a carbon source for all but two (2008-2009) of the subsequent years (through 2012) (Environment Canada, 2014; Stinson et al., 2011). If there had been no changes in either the United States or Mexico over that period, the North American sink might be expected to decline between the decades of 1990-1999 and 2000-2009. There is perhaps some suggestion of a shift in that direction in the AIM estimates and perhaps the TBM estimates (Table 1), but the uncertainties are very large and any conclusion, as noted above, is tentative at best. Moreover, the inventory-based estimates suggest an increase in the sink (Table 1). Increases in natural disturbances (a declining sink) are off-set by simultaneous decreases in harvest rates (an increasing sink) and these two opposing trends in the activity data may make it difficult to identify a clear overall trend in the CO_2 balance using inventory-based methods. Decadal changes in disturbance like those reported by Kasischke et al. (2013) likely influence the North American sink, but a clear definitive signal of that influence in the estimates given their uncertainties is elusive.

Also, the effects of ElNino on arid lands needs to be discussed in view of the recent Nature publication (Poulter et al).

We have added discussion of Poulter et al. and the arid lands in the methods section (see above) and in the addition to the Discussion in response to earlier comments (see above)

Line 22 to page 11043 line 10: It is extremely reader-unfriendly and un-transparent to present these ratios, but hiding the quantitative numbers. I think, the information is needed, but not as a minimum fraction. You need to present real emission numbers.

As noted in response to the corresponding comment on the Abstract we respectively disagree with the reviewer on the issue of presentation, but recognizing that it is a reasonable difference of opinion, we have revised the indicated text accordingly:
The source : sink ratio for the 1990–1999 decadal average ranges across methods from approximately 1628:83 (nearly 20 : 1, the estimate from inventories using the production approach) to as low as 1628:929 (nearly 2 : 1, the atmospheric inversion estimate). For the 2000–2009 decade that range is from 1812:270 (nearly 7 : 1) to 1812:890 (approximately 2 : 1), with the inventory-based production approach and atmospheric inversion approach again generating that range. For the entire 1990–2009 period that range is from 1720:280 (approximately 6 : 1) to 1720:890 (nearly 2 : 1). Based on "best" estimates of the land sink for that entire period, the ratio is in the range of 1720:360 (nearly 5 : 1) based on the median estimate and 1720:472 (nearly 4 : 1) based on the average estimate.

Page 11043:
Line 16: see above. I think, there was enough time, but the anthropogenic fluxes remain unclear, and the models "see" different components of the anthropogenic part.

As noted above, the text here refers to temporal trends not differences across approaches. We have revised the text lines 15-17 to perhaps make that more clear:
…draw any conclusions about changes in interannual variability from decade to decade for any of the approaches. A time series analysis of variability over a longer time period is likely needed to determine whether the North American land sink has been increasing or decreasing, and any such trend may well vary with approach. We…

We have also added sentences to the end of line 20 to reference the Poulter et al. 2014 findings and work by Raczka et al. (2013):
Findings by Poulter et al. (2014) showing the influence of Southern Hemisphere arid grasslands in wet years on interannual variation in the global carbon sink suggest that it may very well be the former. The work of Raczka et al. (2013) showing that TBMs systematically underestimate NEE relative to North American flux towers also points to the conclusion that AIMs are capturing interannual variability in net-land atmosphere CO2 exchange not well represented by TBMs.

Line 29: Please add arid lands to grasslands, and maybe, the better term would be "rangelands" because an Artemisia steppe is not a grassland.

Arid lands has been added**: "**…but not arid lands, grasslands, …"

We retain the term grassland because "rangelands" at least in the US suggests land-use practice which is not what we believe the reviewer intends, and hopefully our reference to non-forest categories at the

end of the parenthetical will capture Artemisia steppe and the like. There is of course a long unfortunate history of vagueness or ambiguity in reference to grassland/rangeland/pasture…but the same might be said of forest from some perspectives.

Page 11044, line 10: Again, you should mention the gap: Trade

We have revised the sentence beginning line 10:
Atmospheric inversions estimate the total land–atmosphere CO_2 exchange from a given region, including any fluxes associated with carbon traded across the regions boundaries, while inventory-based approaches estimate only those exchanges from ecosystem types represented in the inventories (most commonly forest and cropland), and may or may not represent trade of products from those ecosystem types.

Thanks to the authors for this stimulating paper. It could get a milestone in the discussion on what we are missing, rather on a plea for longer measuring sequences (which are also needed).

And we thank the reviewer for their helpful comments. It's unfortunate that we left the impression with the reviewer that we were pleading for longer sequences to resolve difference among approaches. That was not our intent. That plea only went to the issue of resolving multiyear trends. We agree with the reviewer that the differences among approaches are very much influenced by differences in what is included and excluded. Hopefully our revisions and our extended discussion will make that agreement more clear.