Response to reviewer 1:

First we would like to thank the reviewer for carefully reading the manuscript and the helpful comments.

*I would recommend the title to better reflect the fact that the paper is based on the review /collection of existing datasets ("We attempt to summarize the carbon budget of South America ...") and that the most detailed and scientifically interesting part of the paper is about carbon fluxes from vegetation.*

We will change the title to

'The Carbon balance of South America: A review of the status, decadal trends and main determinants'

*The carbon emissions from deforestation reported by the study for two last decades (0.5 PgCyr-1) should be compared (with related discussion) to previous estimates, in particular from Achard et al (GBC, 2004) at 0.54 PgCyr-1 for 1990s and from Pan et al (Science, 2011) at 1.51 and 1.37 PgCyr-1 for 1990s and 2000s (fig1 in Pan et al). Note that although Pan et al estimate seems very high and Pan et al paper contains little details on the method, this paper can not be omitted as published in Science.*

This is a good point. We will include Achard et al. 2004. It is our hunch that the numbers of Pan et al. for South America are based on the black line in our figure 7 (but we will contact to ask Yude Pan directly to be sure). If our hunch is correct we are inclined to trust the other lines in that figure instead, for the reasons discussed in our manuscript. It implies that there remains an imbalance in the global carbon budget, i.e. an unaccounted land (?) sink (of approximately1 PgC $yr^{-1}$).

*In pages 642-643 of section 3.2 (Deforestation): "Main uncertainties of the approach arise because of uncertainties in forest biomass density (i.e. forest tree biomass per area (t ha−1)). Our estimates indicate a flux to the atmosphere on the order of 0.5 PgCyr-1 due to deforestation and land use change in South America over the last two decades or so (Figs. 7 and 8)." The beginning of section 3.2 paper describes the datasets on deforestation which have been collected and used, but it is not clear to me from the main text to which dataset on biomass they are combined and through which method (spatially explicit model, bookkeeping model by country, sub-administrative unit, other method?). It is also not explained in appendix.*

Apologies for the lack of transparency. We used one fixed above-ground biomass estimate based on RAINFOR data but mentioned this only in the Appendix. We will make this clearer in a revised version (see also answers to reviewer 2).

*The study reports carbon exports related to "Agricultural production and exports" (sec- tion 3.6) but the conclusions refer only to carbon fluxes to the atmosphere ("we find that South America had been a net source to the atmosphere during the 1980s (0.3– 0.4 PgCyr−1) and close to neutral in the 1990s with carbon uptake in old-growth forests nearly compensating carbon losses due to fossil fuel burning and deforestation." I have difficulties to understand how carbon export related to fluxes to the atmosphere. Indeed these results does not seem to be considered n final estimates of C fluxes.*

Agricultural products are either consumed / respired within a continent or, if exported, outside the continent. If respired inside the continent then agriculture, in a simplified view, is carbon neutral, if outside, agriculture is a sink.

We demonstrate that such fluxes are negligibly small compared to the other 'players'.

*Technical corrections*

*Consider Baccini et al, Nature CC, 2012 in complement to Saatchi et al, 2011 when referring to biomass maps.*

Yes, we will.

*Consider Gibbs et al, PNAS, 2011, when referring to agriculture as driver of deforesta- tion.*

Yes, we will.

*In section 2.2. p 635, the spatial resolution of Landsat data is c. 30m x 30m instead of 100m x 100m*

Thanks – we will correct.

*When referring to the simple version of the book-keeping model and to the issue of lagged fluxes it might be useful to consider also Ramankutty et al, GCB, 2007.*

Yes we will.

*In section 3.3. page 644 "(i.e. approx 0.5% forest area lost per year, estimated from INPE deforestation numbers based on PRODES)". This 0.5% rate is applied from year 1970 when INPE data starts from year 1988. At least a short explanation / discussion about validity of this rate for 1970s should be given.*

Yes, we will. It is the mean over annual growth rates. The data for the pre-PRODES period are from Fearnside 2005 (see also appendix).

*Data in Appendix are difficult or impossible to read (e.g. in section A.4). They should be reformatted as Tables with same units.*

Yes, we will do.

*Problem of figures numbering: In the figures are quoted in following order: figure 8, figure 11, figure 9, . . .*

Yes – thanks.

*Mayaux et al, 2005 appears in the text but not in reference list. The list of references should be checked (any other missing or not referred in the text?).*

Yes – thanks.

*In table 1, the estimate of flux for "Old-growth forest" refers to year 2005 only but is displayed in column "2005-2009" which is quite confusing. Also the related jump from year 2004 to year 2005 in Figure 11 is also misleading because there are no available estimates for further years (it looks at a future trend and not as a specific drop during a single year)..*

Good point – we will change this.

*Use consistent wording through the text for "old-growth forest" (e.g. suppress "intact forests").*

Yes.

*Why displaying MODIS land cover map in Figure 1 when text is using estimates from GLC-2000 map (Eva et al, 2004)?*

We will use Eva’s map instead.

*Figure 7 should be enlarged as period 1970-2010 is difficult to read in present version.*

Fair point – we will see how we can change this.

Response to reviewer 2:

First we would like to thank the reviewer for his helpful comments. Before addressing specific points we would like to clarify two general issues. Firstly it seems we and/or the editor of the special volume, for which this manuscript is intended, have not succeeded to communicate the purpose of the paper / special volume. The paper is part of a special volume lead by the RECCAP effort with each chapter covering the NET CARBON BALANCE of a large region of the globe including continents and ocean regions. There will be probably on the order of 15 such chapters. RECCAP has provided the authors of all chapters with a fixed protocol which explicitly included the use of dedicated DGVM results as well as atmospheric transport inversion results.

To be more specific, differently from what the reviewer seems to have thought the purpose of the manuscript is NOT primarily ‘to review current scientific knowledge in natural and socio-economic processes which have an influence on the carbon cycle’ (using the reviewer’s words), but rather to review / reanalyze THE NET CARBON BALANCE OF THE CONTINENT AS A WHOLE. It was thus never intended to be a comprehensive compendium of all processes which contribute to carbon fluxes on a scale of, let’s say, a forest stand (a few hectares) nor of all loops and feedbacks on that scale. Although we do provide in addition socio-economic information on controls this is to put into context the main drivers of contemporary trends in the continental scale net carbon balance and likely future developments

Looking at it another way, the primary goal of the manuscript is the best possible estimate of the carbon fluxes into and out of an imaginary box bounded by vertical walls wrapped all around South America’s border. Thus, any carbon flux loops closed within this box cancel and are thus inconsequential for this study (although obviously still of scientific interest in other contexts). As an example, for riverine carbon cycling, the only components relevant in this context are those which lead to a net carbon export from the South American box. This is the carbon export to the oceans either in form of DOC, DIC or POC. The loop where carbon enters the rivers (e.g. soil respired carbon dissolved in soil water and then streams) followed by outgassing back into the atmosphere followed by re-uptake by the land vegetation is a closed loop within the box and thus does not need accounting (although we are well aware of the main relevant papers, those by Ritchey, Victoria, Krusche, Johnson).

A second closely related point is the handling of fire and the request to include studies which have estimated fluxes related to fire 'directly', e.g. by remote sensing, like e.g.Van der Werf et al.'s studies. Whilst these are very nice studies and of great interest in many regards, for net carbon balances of a large region (a continent) associated with fire we have chosen to take another approach, *viz.* to estimate the associated fluxes via differencing of stocks of land vegetation carbon. In our view this is a "cleaner approach" because it circumvents elegantly the issues of accounting for closed loops of fire and subsequent carbon uptake (please see below). The approach is also substantially more accurate. It is this approach that our bookkeeping model follows (please see also below where we comment on this some more). Thus again while we agree that fire emissions are important, we feel a discussion of direct emission estimates is beyond the scope of the manuscript, also serving not to change it's primary conclusions.

In a revised manuscript we will, however, attempt to make much more clear what the purpose of the paper is and what the approach is that we chose and for what reason, as this was clearly a deficiency in the submitted version. We will do so by adding a section along the lines written above. We will also explain the bookkeeping approach in more detail in the main text (rather than in the appendix) and where needed will be more explicit about the 'model' still, possibly by adding an explanatory table.

1.1 *Unfortunately, the authors ignore a large part of the scientific literature that deals with the influence of fire in various ecosystems of South America, its human modifica- tion, of which the use in the deforestation process is widely discussed and estimates of related carbon emissions are available. The compilation of knowledge on this topic must be added before this manuscript can finally be published.*

While fire flux estimates of the type suggested by the referee are of great interest for various reasons, the purpose of our manuscript is PRIMARILY to address the whole continent carbon balance (please see above) and we have included in our analysis those aspects of fire which affect changes in carbon stocks of the continent on timescales of a few years and longer. To reiterate, conceptually, fluxes can be measured / estimated by at least two approaches: by estimating fluxes directly, e.g. via remote sensing, the type of estimates suggested by the referee, or by estimating differences of stocks over defined time periods. We feel strongly that the second approach is much more adequate/robust for our purpose *(viz.* estimates of longer-term carbon balances) and this is thus the methodology used in our book-keeping approach. We favor this approach because with the first approach (direct flux estimation) it is difficult / impossible to assign which portion of the fires are 'reversible' (e.g. part of a natural cycle like savanna burning) and which 'irreversible' on time-scales of decades. Additionally direct fire emissions estimates are, particularly in the tropics, highly uncertain due to both methodological and observational (cloud coverage) issues (this is discussed in great detail in many studies).

We see as main values of direct fire emissions estimates that they help to analyze and understand inter-annual variability in global atmospheric $CO_2$ (e.g. the Mauna Loa record) as well as mechanistic understanding of fire feedbacks of forests. But neither of these subjects fall under the mandate of the manuscript (although we do mention the possibility of fire feedbacks and its role for the rainforests in the section on controls where we believe it logically belongs, and cite key papers).)

We will try to make these points much clearer in a revised manuscript (as well as much clearer what the purpose of the manuscript is) early on.

*1.2 There is a growing literature on the carbon balance in rivers and VOC, which should be considered and the interactions that could occur need to be formulated better. This is a point where the authors stop compiling scientific evidence and put vague state- ments.*

For our rationale regarding the treatment of rivers please see above. Also we would like to stress that the relevant numbers (the ones on riverine carbon export to the oceans) are not based on vague statements.

Regarding the role of VOCs for the South American net carbon balance we will now add a detailed paragraph.

*1.3 The section on deforestation is insufficient if the authors aim to reflect recent knowl- edge on deforestation areas. More description on deforestation in, e.g., the Chaco region of South America, is needed and must be added.*

We have actually made a great effort to find any publications on deforestation estimates based on traceable (reproducible) methods (remote sensing) outside (and inside) Brazil. They are listed in the appendix (A.4), which possibly was not available to the reviewer. We list below again the publications on which appendix A4 and our bookkeeping model is based. It DOES include estimates of deforestation for the Chaco region (specifically Gasparri et 2008, Huang et al. 2009). It would seem our section on this aspect of the study was not sufficiently clear in terms of detailing the data we compiled and we will change this in a revised manuscript.

Also we have indeed not been able to find proper (non-grey literature) publications for some regions (see below) despite great efforts and would be very grateful if the reviewer would be able to point us to publications, which may fill that gap if he knows of any.

*Humid tropical forests*
*Achard et al. 2002 (Table 1) based on remote sensing*
Forest cover change Latin America Humid Tropical Forest
*Deforestation estimates of humid tropical forests 2000-2005 based on remote sensing*
*(Hansen et al. 2008)*

*Deforestation data for countries other than Brazil*
*Andean Amazon*

*Bolivian Amazon*
*Steininger et al. 2001*
*Killeen et al. 2007*

*Peruvian Amazon*
*Perz et al. 2005*
*Oliveira et al . 2005*

*Colombia – no reliable data found (although see Sierra 2000)*
*Venezuela – no reliable data found*
*Ecuador – no reliable data found*

*Non-Amazon*
*Paraguay*
*Huang et al. 2007*

*Argentina*
*Gasparri et al. 2008*

*1.4 The section on agriculture production and exports is very short and more informa- tion is available through FAO statistics on agricultural production and exports to cover the process of "carbon lost" from the study region.*

This is a fair point and we will expand this section as suggested by the reviewer.

*1.5 To cover the influence on the carbon balance and changes in source-sink distributions the authors need to add a section on timber extraction as this is an important cause for deforestation and is also a carbon extraction from the system. There is information available on quantities and project interactions that the authors need to include in their review.*

We are not sure what the reviewer means by 'project interactions' and thus we cannot respond to this point. With regards to the other comments: for the bookkeeping approach the quantity which matters is wood and wood products export. From FAO statistics we have compiled the table below:

Export of wood and wood products in units of ($10^3$ $m^3$)

| | 2001 | 2002 | 2003 | 2004 | 2005 | 2006 | 2007 | 2008 | 2009 | 2010 |
|---|---|---|---|---|---|---|---|---|---|---|
| Argentina | 829 | 1096 | 1496 | 1506 | 1632 | 1649 | 1328 | 1360 | 1278 | 1335 |
| Brasil | 10452 | 11979 | 14084 | 15732 | 22109 | 21133 | 20719 | 19489 | 17828 | 18639 |
| Bolivia | 46 | 46 | 60 | 75 | 88 | 142 | 194 | 109 | 78 | 138 |
| Chile | 8295 | 7086 | 8552 | 9180 | 10504 | 9789 | 11474 | 12231 | 10668 | 10692 |
| Colombia | 172 | 227 | 302 | 316 | 261 | 228 | 280 | 262 | 274 | 313 |
| Ecuador | 300 | 255 | 387 | 395 | 329 | 352 | 433 | 428 | 435 | 416 |
| French Guiana | 6 | 6 | 6 | 6 | 10 | 9 | 9 | 8 | 8 | 8 |
| Guyana | 135 | 128 | 141 | 174 | 192 | 275 | 246 | 174 | 127 | 168 |
| Paraguay | 266 | 316 | 311 | 198 | 250 | 305 | 338 | 370 | 335 | 344 |
| Peru | 137 | 159 | 154 | 194 | 235 | 223 | 230 | 269 | 193 | 207 |
| Suriname | 17 | 35 | 13 | 13 | 15 | 21 | 15 | 38 | 36 | 57 |
| Uruguay | 1030 | 1263 | 1763 | 2635 | 2989 | 3611 | 4136 | 7013 | 6441 | 9335 |
| Venezuela | 95 | 158 | 231 | 264 | 234 | 126 | 154 | 99 | 76 | 91 |
| Total | 21780 | 22754 | 27500 | 30688 | 38848 | 37863 | 39556 | 41850 | 37777 | 41743 |

Assuming a wood density of 0.65 t $m^{-3}$ and a carbon/wood ratio of 0.5 we obtain total annual carbon exports due to wood and wood products exports in units of (PgC $yr^{-1}$) of

0.007 0.007 0.09 0.010 0.013 0.012 0.013 0.014 0.012 0.014

respectively. Although these numbers are disturbing *per se,* in terms of carbon they are very small numbers compared to other contributions to the carbon balance of South America (see e.g. figure 6). Nonetheless we will report these numbers in a revised version of our manuscript and integrate them adequately in the book-keeping calculation.

*Additionally, this needs to be taken into account in the carbon balance estimation using the book-keeping approach.*

Please see above.

*2. In the presentation of the carbon balance for the study region, be it either tropical South America or the entire South-American continent, the authors must state which processes are included in the book-keeping approach and discuss the missing processes as well as uncertainties associated to the single processes as well as the entire outcome of the carbon balance.*

As already stated we will describe the model in more detail in the main text including a discussion of included and not included processes. Currently the model is described in full in the appendix but we will repeat some of the material in the main text. Also we agree with the referee that an uncertainty analysis needs to be provided and we will undertake the relevant calculations.

*3. It is not well formulated in the manuscript, why future projections from dynamic global vegetation models where first attempted to get included in the manuscript, where this was not really an objective of the manuscript.*

Please see comments at the beginning of this response to the referee's comments.

*Why did the authors expect a homogenous response of the vegetation models*

In our view if the models were realistic they should agree at least in the sign of the cumulated fluxes and they do not even do that but please see also below.

*and why do they think this is a pre-condition to regard the results from the intercomparison to be regarded as publishable results?*

We do not make a judgement whether these results are publishable or not – we just conclude that at this stage such estimates are not helpful for the purpose of a continental scale carbon balance which is the primary purpose of this manuscript. In addition: please see below.

*Minor issues: 1. Section 2.3, last paragraph: It is not clear to me how an* *upward trend in water vapour outflow published in 1996 can support the hypothesis of changes in the water balance published in 2005. Please provide more evidence or explain in more detail how the findings of the mentioned studies complement.*

We cite the 1996 paper by Rao et al. because it states clearly (and provides evidence for) what the mechanism for the upward trend in precipitation in the South of Brazil and the river Plata catchment area is (an upward trend in water vapor export from the Amazon basin).

Rao et al. write on p. 26459

'The most relevant feature of Figure 9 for the present study is the southeastward transport of water vapor into central South America. This transport is more dominant in January than in July, and it starts becoming important in October. These features can also be inferred from the earlier studies of Marques et al. [1979a, b, 1980a, b] and James and Anderson [1984]. This corroborates the suggestion made earlier in discussing Figure 2 **that the increase of moist conditions in October over central Brasil represented by Brasilia is due to the transport of water vapor from the Amazon region.'**

*2. Section 2.4: Rammig et al 2010 NewPhyt and Jupp et al. 2010 NewPhyt. investigated the uncertainty of climate projections and the role of the CO2 fertilization effect under future climate conditions. Conditions of shifts in between savannah and tropical forest where investigated by Hirota et al. This literature is missing in this section.*

There is a long list of climate model studies on Amazon dieback probably starting in 1999 and similarly on the topic of $CO_2$ fertilization (for which much older studies exist). We cite the studies, which predicted a dieback for the first time and believe that this is a fair choice. We will have a look at the Hirota et al. paper and will compare with the on-ground observations we cite. Depending on how well they agree we will discuss and cite the paper.

*4. Section 3.4 Please describe how measurements from the ATTO tower can contribute to improve the situation on monitoring CO2 fluxes over the Amazon.*

In our view the best strategy to improve the situation is to use aircraft vertical profiles (biweekly, $CO_2$, CO, $CH_4$, ..). Such a program is currently ongoing (stations Santarem, Rio Branco, Tabatinga, Alta Floresta, biweekly profiles since staring by the end of 2009, NERC AMAZONICA and FAPESP project) led by several of the authors of this manuscript. Manuscripts calculating integral $CO_2$ and $CH_4$ balances for the Amazon basin are in preparation. I (MG) do not know that much about the ATTO tower and thus would prefer not to comment on it (although I would be happy to discuss the matter offline; my email address is eugloor@gmail.com).

*5. Section 3.5: Please consider in the explanation of the model results, which carbon- relevant processes are captured by the models to explain the difference in the simu- lated responses.*

We will add a table listing the key processes represented by each of the models.

*It is required to explain the quality measures that are needed from the DGVMs in order to get included in the carbon balance analysis! Are these uncertainties really larger than the uncertainties from fossil fuel emissions? This must be explained and quantified.*

Firstly, as already mentioned, the models do not even agree on the sign of carbon gains / losses of the land vegetation in clear contrast to fossil fuel emissions (please see also below). Worse still trends disagree substantially as well, both in sign and magnitude. This is shown clearly in the figures presented. Given what we present in this paper we can point to other deficiencies though which include for example the poor agreement with the results from forest inventory data (RAINFOR).

Yet further measures (a bit out of scope of this study though) would for example include agreement with large-scale features of e.g. forest turnover rates, which are known robustly to be substantially faster in the Western Amazon than the Eastern Amazon. None of the DGVM models do capture those so far without hard-wiring (prescribing) forest turnover rates themselves. Yet other constraints include proper attribution of inter-annual variation of land vegetation carbon gains and losses to productivity versus mortality (see e.g. Phillips et al. 2009). Again models do by no means generally do this correctly. The list could be made substantially longer. Overall thus given that we have other data which can give us estimates of changes of land vegetation pools and given the disappointing inter-comparison of model predictions, in the end we decided against using DGVM estimates in our final balances. Maybe it is also worth pointing out that we are talking about quite small signals and that the uncertainty induced by model uncertainties as documented in the manuscript are much larger than uncertainties of other components (please see also below).

Finally some crude uncertainty estimate of the DGVM flux estimates, as requested by the reviewer, may be obtained from the spread of the simulations. The standard deviation is on the order of 0.15 PgC $yr^{-1}$ or 5 to 10 times larger than the uncertainty in fossil fuel emissions (see below).

Regarding the second question: estimating uncertainties in fossil fuels is unfortunately not entirely straightforward. Nonetheless we borrow from 'the authority' (Gregg Marland):

'Olivier and Peters (2002) estimated that emissions from Organisation for Economic Co-operation and Development (OECD) countries may have—on average—an uncertainty of 5% to 10%, whereas the uncertainty may be 10% to 20% for other countries. The International Energy Agency did not report the uncertainty of its emissions estimates but relied on Intergovernmental Panel on Climate Change (IPCC) methodologies and cited the IPCC estimate that "for countries with good energy collection systems, this [IPCC Tier I method] will result in an uncertainty range of ± 5%. The uncertainty range in countries with 'less well-developed energy data systems' may be on the order of ± 10%."

(G. Marland (2008) Uncertainties in Accounting for $CO_2$ From Fossil Fuels, Journal of Industrial Ecology)

It is these numbers, which we adopt in our manuscript.

*6. Section 3.6 needs fundamental revision as outlined in the section on major issues.*
This is well taken and we will add the requested information in a revised manuscript.

*7. Section 4: Explain why TRMM or NCEP data are not sufficient to measure changes in precipitation pattern, why is the uncertainty lower when considering river discharge.*

TRMM data exist since 1998 – thus the record is too short for trend analysis relevant to the time-scales discussed in the paper. NCEP data exist for a longer period but ultimately any analysis or reanalysis of meteorological data depends on station density. A sense of station density in the Amazon amenable to trend analysis can be gained e.g. from Figure 1 of Haylock et al. 2006. There are nearly no suitable records (in the sense of Haylock et al. 2006) in the Amazon basin, notably in contrast to the remainder of the continent.

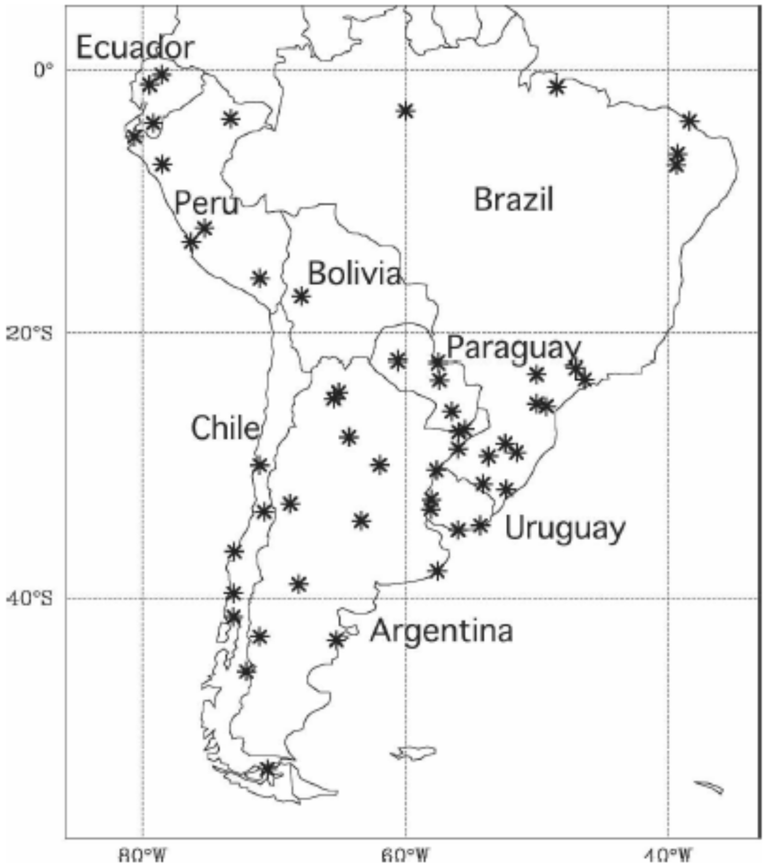

FIG. 1. Location of 54 rainfall stations.

Figure 1 of Haylock et al. 2006 which shows the precipitation records which Haylock et al. 2006 determined according to there quality criterion for being included in a decadal scale trend analysis.

Other publications which demonstrate the same point although with slightly different emphasis are Costa et al. 2009, Effects of Climatic Variability and Deforestation on surface water regimes, in Amazonia and Global Change, Geophys. Mon. Ser. 186, AGU, Fig. 2, or also Garreaud et al. 2008, Fig. 1.

A General Circulation Model based reanalysis as an estimator of precipitation relies primarily on the data and also on the quality of the atmospheric circulation model with known deficiencies not the last in the Amazon (representation of Andes and their effect and cloud parameterization). Altogether there are thus good reasons to be skeptical about such reanalyses for the Amazon and to thus look for other, more robust estimators.

In contrast to precipitation measurements, water stage measurements exist at least since the beginning of the 20th century, they are a simple measurement and because of their integrative nature provide robust diagnostics of the hydrological cycle (with the caveats we explain in the manuscript).

Despite these considerations we have done a trend analysis of precipitation data from CRU which we are considering to include in a revised manuscript (see below).

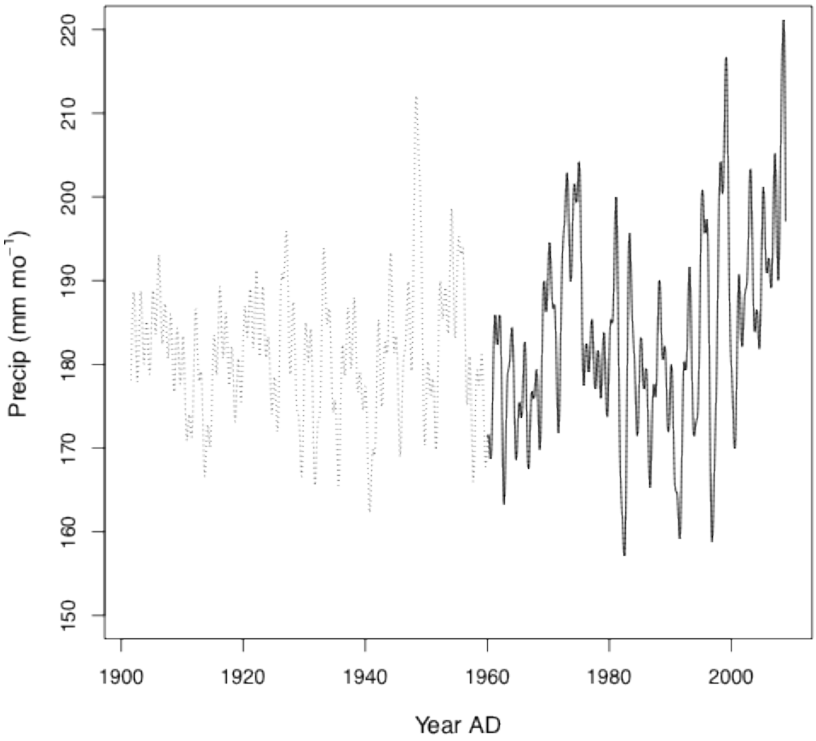


Fig. 1 Low-pass filtered Amazon precipitation record derived from CRU climatology.

*Again, only talking about the Amazon basin in this respect is not enough in a review about processes influencing the carbon balance of a continent.*

We will expand the climate section to include a discussion of climate trends for the whole of South America.

*8. Fig. 4 Explain how the meat export was converted to make it comparable to other carbon-related fluxes. Again statistics from Brazil are not sufficient to give an overview about South America.*

We have not converted meat to carbon (see headers in figure 4). The point of the figure is to show that the numbers (whether converted or not) are so small that they are safely negligible compared to other contributions. To get some idea what the C weight fraction of meat exports may be one may consider the composition of muscle. For human skeletal muscle it is: 79.2% water, 3.14 % nitrogen, 16 % proteins, 0.7 % DNS and small fractions of elements like Na and K (Wissenschaftliche Tabellen Geigy, Teilband Koerperfluessigkeiten, CIBA-GEIGY Limited, Basle, Switzerland, 1977, 8th edition, p.216). Thus the C weight ratio is probably around 10 % (i.e. our numbers would have to be multiplied by a factor ~0.1).

Also, again, we will include statistics for the rest of the continent in a revised manuscript.