**Earth System
Dynamics**
Discussions

Interactive comment on "Explaining the seasonal cycle of the globally averaged CO_2 with a carbon cycle model" *by* G. A. Alexandrov

G.A. Alexandrov

g.alexandrov@ifaran.ru

Preface

After reading the Referee Comments, I realized that I failed to write clearly about the most important things, and supposed that it would be better to start the Author Comments with a response to the conceptual comments. To improve readability of the Author Comments I divided them into the following sections: Response to conceptual comments, Response to the technical comments of Referee #1, Response to the technical comments of Referee #2, The plan for the major revision of the manuscript.

Response to conceptual comments

The virtue of using a globally averaged CO2

Why I am trying to simulate the seasonal changes in globally averaged CO2? The answer is simple: nobody did it before. I did not find any publication where it was done. It seems that this is the first paper where the seasonal cycle of globally averaged CO2 is simulated with a model of terrestrial carbon cycle.

I wonder why nobody did it before me. The globally averaged CO2 is much more important characteristic of the Earth's atmosphere, than any local concentrations, or local concentrations averaged over a latitudinal belt. Are we going to stabilize CO2 concentrations on spatially explicit basis? No, we are not. Do we calculate radiative forcing in climate models based on spatially explicit data on CO2 concentrations? No, we do not. I use globally averaged CO2, because it is an important driver of the Earth system dynamics.

The globally averaged CO2 shows the amount of carbon dioxide stored in the atmosphere. Multiply globally averaged CO2 concentration by 2.13 – the result is the mass of carbon dioxide, in GtC, stored in the atmosphere. The increase in the mass of carbon dioxide stored in the atmosphere is the item of the global carbon budget. It reflects the difference between the mass of carbon dioxide released to the atmosphere from sources and the mass of carbon dioxide removed from the atmosphere by sinks.

Terrestrial part of the biosphere provides a sink for atmospheric carbon dioxide. The magnitude of the sink is changing on seasonal basis. The seasonal changes in globally averaged CO2 concentration allow us to estimate the amplitude of seasonal changes in the terrestrial sink. Thus estimated amplitude should be roughly similar to that simulated by carbon cycle models. However, it does not. Carbon cycle models suggest that terrestrial part of the biosphere cannot provide the summertime sink of such magnitude. Therefore, I think it is reasonable to do some research to realize how we could improve carbon cycle models to reconcile simulated seasonal changes in the terrestrial sink magnitude and the seasonal changes in the mass of carbon dioxide stored in the atmosphere.

Can we prove something by tuning a knob in a model?

There are no serious doubts that the seasonal changes in globally averaged CO2 concentration reflect the seasonal changes in the terrestrial sink. However, if this is true, and the models are valid, then model coefficients can be set at the values that bring simulated seasonal changes in the terrestrial sink magnitude in agreement with the seasonal changes in the mass of carbon dioxide stored in the atmosphere. If we fail to find such values, then at least one of our hypotheses is not valid.

This logic can be formalized as follows. We have four statements:

A. the seasonal changes in globally averaged CO2 concentration reflect the seasonal changes in the terrestrial sink

B.global carbon cycle models are valid

C. the global carbon cycle models can simulate the seasonal changes in globally averaged CO2 under some values of model coefficients

D.If A is true and B is true, then C is true

In terms of mathematical logic, D means that (A and B) implies C, and D is true in the following three cases:

(A and B) is true and C is true

(A and B) is false and C is true

(A and B) is false and C is false

Therefore, if D is true and C is false, then (A and B) is false. And if (A and B) is false, then either A or B is false, or both are false.

We can prove that C is true by tuning a knob in a model. Or we can prove that C is false, if tuning does not make C true. If we prove that C is false, then both A and B cannot be true, at least one of them is false. We cannot prove that (A and B) is true by

proving that C is true, however. This merely allows us to say that there is no reason for doubts that (A and B) is true.

The range of realistic values for model coefficients

The seasonal cycle of the globally averaged CO2 suggests that terrestrial ecosystems remove 9 GtC from the atmosphere in summer (May - August) and release 9 GtC to the atmosphere during other seasons (from September to April). Could ecosystems located to the north from 45N remove 9 GtC from the atmosphere in May-August? Since their annual NPP is estimated at 12 GtC/yr, they probably could do this.

Let us assume that ecosystems located to the north from 45N provide the net sink of such magnitude in May-August. Then the release of carbon associated with their heterotrophic respiration should be less than 3 GtC. In other words, the monthly average Rh for the summer period should be less than $\frac{3}{4}$ GtC/month.

Climatic conditions of the summer period are more favorable for litter decay than climatic conditions of spring, autumn or winter. We may expect that the monthly average Rh for September-April should be smaller than the monthly average Rh for May-August. In contrast to such expectations, the monthly average Rh for the period from September to April is equal to 9/8 GtC/month – that is, it is larger than in May-August.

This can be explained either by substrate limitation or by the low estimate of NPP. The realistic estimates of NPP vary in a wide range. In the case of global terrestrial NPP, they vary from 40 to 80 GtC/yr. Thus annual NPP is one of the "knobs" that can be tuned for reconciling simulated seasonal changes in the terrestrial sink magnitude and the seasonal changes in the mass of carbon dioxide stored in the atmosphere.

Another important "knob" is Q10 factor of autotrophic respiration. It is normally assumed that Q10=2, but Q10=1.5 is not an unrealistic estimate. Reducing Q10 is another possibility to bring simulations closer to observations.

The other important "knobs" are the coefficients that are used to divide Rh into three

components: 1. Rhq – the component which seasonal changes reflect the seasonal pattern of litter production; 2. Rhs – the component which seasonal changes reflects the seasonal pattern AET (i.e. climatic conditions); 3. Rhi – the component which is relatively constant over the year.

The share of Rhq in the annual Rh depends on the share of "herbaceous" material in the litter production and the share of quickly decaying compounds in the "herbaceous" material. The realistic estimates of Rhq may vary depending on biome (probably from 0.2 in evergreen needleleaf forests to 0.4 in grasslands). They are also sensitive to the estimates of the share of quickly decaying compounds in the "herbaceous" material.

The share of Rhi in the annual Rh depends on the share of belowground coarse wood material in the litter production. In managed forests, it may depend on the ratio of harvested wood to the total wood material in the litter production. Any estimate of the Rhi share is difficult to justify. Therefore, tuning this "knob", one rather proposes an assumption that should be validated in a separate study.

At this point I would like to emphasize the purpose of reported research as it was stated in the manuscript. This purpose is quite modest – to reveal the structural components of the terrestrial carbon cycle models which are essential to understanding where the discrepancy between simulated and observed globally averaged monthly atmospheric concentrations of carbon dioxide comes from.

Point-by-point response to the technical comments of Referee #1

The realistic range of partitioning between fast and slow fractions in litterfall

Esser (1984; 1994) summarized the data on the share of herbaceous fractions (such as leaves and fine roots) in NPP for 17 biomes and found that the ratio of herbaceous NPP (NPPh) to the total NPP (i.e. herbaceous factor of NPP) vary from 0.3 for forests to 0.9 for grasslands.

In a growing forest stand the herbaceous factor of litter production may be larger than

that of NPP, because a large part of NPP is allocated in the annual increment of wood. At the larger scale we should take into account the effect of wood harvesting. About half of NPP is allocated to trunks. Therefore herbaceous factor of litter production in managed forests could be much higher than the herbaceous factor of NPP. Besides, forests do not occupy the whole territory of a forest biome. They cover from 40 to 70 % of area. Hence, the herbaceous factor of litter production at the regional scale probably varies from 45 to 85 %.

Parton et al (1987) divided herbaceous plant material into structural and metabolic compounds. The maximum rate of metabolic compounds decay is 3-4 times higher than that of structural compounds.

The fraction of metabolic compounds in the litter production depends on the lignin to nitrogen ratio (L/N): Fm=0.85-0.018 L/N. The L/N is high in needle leaf litter. For example, Berg (2008) reported that average content of lignin in the samples of fresh Scots pine needle litter was 226.5 mg/g, and the N content was 3.9 mg/g. This suggests L/N=57, that results in negative Fm. Scott and Binkley (1997) provided data on lignin and N content in the foliar litter of several species. As it can be seen from these data, the typical values of L/N in foliar litter of deciduous tree species vary from 15 to 25, and from 5 to 15 in the litter of grass species. Hence, the equation for calculating the fraction of metabolic compounds suggests that it could be 0.55-0.75 in grasslands, 0.40-0.55 in deciduous forests, and negligible in evergreen needleleaf forests.

Parton et al (1987) also assumed that only 55% of carbon are released to the atmosphere in course of fresh litter decomposition, other 45% go to the pools of soil organic matter. Hence, only 55% of metabolic compounds may contribute to Rhq. Hence, the share of quickly decaying fractions in the litter production is below 0.2 in needleleaf forests, 0.2-0.3 in deciduous forests, and 0.3-0.4 in grasslands. Suggesting that at the regional scale there are at least 30% of grasslands in forest biomes, and there are at least 10% of forests in grassland biomes the share of quickly decaying fractions in the litter production could be 0.2-0.25 in the biome of evergreen needleleaf forests,

0.25-0.3 in the biome of deciduous broadleaf forests, and 0.3-0.35 in grassland biome.

Partitioning coefficients between fast and slow fractions in litter pools are not kept constant during the whole year

The constant partitioning coefficients between fast and slow fractions in the litter production do not lead to the constant partitioning coefficients between fast and slow fractions in litter pools. The pool of slow fractions stores roughly constant amount of carbon during the whole year, whereas the pool of fast fractions contains little carbon during the warm period of the year, and is refilled in the autumn. The constant partitioning coefficients between fast and slow fractions in the litter production lead only to corresponding partition of heterotrophic respiration. The notation Rh(m)=0.3Rhs(m)+0.7Rhq(m) means the following: 30% of carbon contained in the annual litter production return to the atmosphere via slow decomposition pathway and 70% via the fast decomposition pathway.

The enhanced seasonality of CO2 exchange in the Northern Hemisphere could be explained either by GPP growth or by increasing the share of fast fractions in the litterfall

The enhanced seasonality of CO2 exchange in the Northern Hemisphere was not a subject of this study. I may only hypothesize that it can be explained not only by GPP growth but also by increasing share of quickly decaying fractions in the litter production. This may happen due to nitrogen deposition, which probably reduce L/N, or due to land cover changes.

Carbon losses from the inert part of SOM may be much less sensitive to seasonal changes in AET

Taking into account that the inert part of SOM may be much less sensitive to seasonal changes in AET, we may include a third type of heterotrophic respiration (Rhi) which is equal to a constant value. How large could be the part of the heterotrophic respiration which is not sensitive to seasonal changes in AET? If we assume that annual

Rh=0.35Rhs+0.65Rhi, then amplitude of seasonal changes in simulated net carbon exchange between the atmosphere and terrestrial part of the biosphere will be close to that derived from CO2 observations (Fig 1).

The pronounced NEP seasonality suggests that something disrupts the monthly balance between the organic matter decay and production

Let us assume that seasonal changes of GPP, Ra and Rh are all proportional to seasonal changes in AET. Then GPP(m)-Ra(m)-Rh(m) will equal to zero. The monthly input and output fluxes will be in balance.

We may disrupt the monthly balance by assuming that inert part of SOM may be much less sensitive to seasonal changes in AET. Another way to disrupt the monthly balance is to assume that the heterotrophic respiration is divided into the flux which is proportional to AET and the flux which is proportional to the seasonal pattern of litter production. And probably the simplest way to disrupt the monthly balance is to assume that Ra depends on air temperature, not on AET.

Point-by-point response to the technical comments of Referee #2 Why the more mechanistic descriptions of litterfall and turnover may fail to reproduce the mean annual cycle in CO2 The model for heterotrophic respiration which is used in this study is not less mechanistic than the model proposed by Parton et al (1987) and other models of this sort. The more simple form results from the main subject of study – steady seasonal cycle. The models of soil organic matter have a different subject. They are developed for simulating the impacts of climate change, land-use, nitrogen deposition and other disturbances on the carbon stocks in the soil. The equations for describing steady state fluxes have more simple form than equations for describing the fluxes in a disturbed system.

Could limiting substrate availability move individual sites in the right direction? This is an obvious omission in this paper. The validity of the approach should be illustrated with a data from at least one of Fluxnet site. The only excuse for this omission is

that the purpose of the paper is to reveal the structural component of the terrestrial carbon cycle models which are essential to understanding where the discrepancy between simulated and observed globally averaged monthly atmospheric concentrations of carbon dioxide comes from. Developing a well-validated model is much more difficult task.

Seasonality in atmospheric CO2 is driven by high northern latitude ecosystems I share the common belief that seasonality in atmospheric CO2 is driven by the ecosystems located to the north from 45N, but suppose that some simulations should be done to prove somehow that these ecosystems can provide a seasonal sink of required magnitude. To provide such sink they should remove from the atmosphere 9 GtC in summer (from May to August) and release to the atmosphere 9 GtC during other seasons (from September to April). The total NPP of these ecosystems is estimated at 12 GtC/year. If we assume that most NPP is formed from May to August, then only 3 GtC could be released to the atmosphere during the summer. In such case the average monthly Rh during May-August should be lower than the average monthly Rh during September-April (3/4 vs 9/8 GtC/m). How can we explain this without limiting substrate availability? There is only one way – to increase the estimate of NPP. For example, if NPP would be as high as 14 GtC, then the average monthly Rh during May-August could be slightly larger than the average monthly Rh during September-April (5/4 vs 9/8 GtC/m). Taking into account that heterotrophic respiration is reduced in winter, one should double the estimate of NPP to make the rate of summertime heterotrophic respiration higher than the rate heterotrophic respiration in autumn and spring (13/4 vs 9/4).

The plan for major revision of the manuscript It seems to me that the manuscript needs a major revision. All Referee Comments are important, and I would like to address them adequately in the final version. The most important changes will include the follows:

1. I will replace the results of simulations presented at Fig 4 with similar results obtained under more realistic assumptions (relatively high but realistic estimate of NPP, relatively

low but realistic estimate of Q10, partitioning coefficients for Rhq, Rhs and Rhi that fall within the ranges proposed by Parton et al (1987)).

2. I will add an Appendix illustrating the validity of the model using a Fluxnet site as a case study.

3. I will add an Appendix explaining simplifications in the mechanistic description of soil carbon cycle which become possible in steady state conditions.
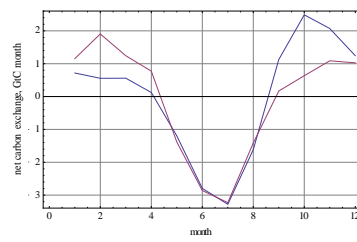
Figure 1. Net carbon exchange between the atmosphere and terrestrial part of the biosphere: blue - derived from globally averaged CO2, red - simulated under assumption that annual Rh=0.35Rhs+0.65Rhi

Fig. 1.

C60