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Biogeosciences Discussions

Interactive comment on "Soil-atmosphere exchange of nitrous oxide, methane and carbon dioxide in a gradient of elevation in the coastal Brazilian Atlantic forest" *by* E. Sousa Neto et al.

Anonymous Referee #2

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The authors report one year of data on soil N2O, CH4 and CO2 fluxes along an elevation gradient in the coastal Brazilian Atlantic Forest, a scarcely studied region, amended with supporting data on C and N stocks in the soil, fine root biomass, and litter fall and stocks. In principle, due to the scarcity of data from this important range of threatened, but still extensive ecosystems this study merits publication, although the advance in process understanding of formation and consumption of trace gases will be minor. However, before publication, several weaknesses should be addressed and overcome (see comments below).

Specific comments

p. 5229, l. 8-14: Here and throughout the manuscript, you use three different units for the three different gases, i.e. ng N2O-N cm-1 h-1, mg CH4 m-2 d-1, and μmol CO2 m-2 s-1. This does not help to compare the fluxes with each other and with literature. I suggest choosing a common area and time basis (e.g. m-2 h-1) and deciding whether a mass unit (g) or an amount unit (mol) should be used, but please be consistent.

p. 5231, l. 18: The study was conducted in humid subtropical climate, but throughout the paper you dwell extensively on the importance of tropical forests and their soils in the global GHG budget, and the scarcity of data from tropical regions. Strictly speaking, this work does not contribute to reducing the scarcity of knowledge about tropical regions, as it was not conducted in the tropical climate zone.

p. 5231, l. 20: "historical": please give the averaging period here (e.g., 1971-2000)

p. 5232, l. 2: Could you specify "nutrient contents" and "nutritional reserve"?

p. 5235, l.13-14: In Table 2 I find the opposite, i.e. greater fine root biomass in the dry season.

p. 5237, l. 22f and Conclusions: The hole-in-the-pipe conceptual model relates fluxes through the "pipes" of nitrification and denitrification to gaseous losses of NO, N2O and N2. Thus, it requires gross rates of nitrification and denitrification for proper consideration. In this work, only net rates of N-mineralization and nitrification were determined, making the relation of the findings to the hole-in-the-pipe model invalid or at least weak. Greater nitrogen (e.g. nitrate) availability always occurs, when consumption is lower than production, irrespective of the magnitude of the fluxes. If one assumes that N2O losses are proportional to the amount of nitrogen converted, e.g. from ammonium to nitrate or from nitrate to molecular nitrogen, and hence correspond to the gross rates of nitrification or denitrification, the findings of the present work don't necessarily have to be contradictory to the hole-in-the-pipe concept. The same applies to carbon stocks in the soil. Why do we find the largest C stocks especially in colder and drier places (also in this study)? Because the decomposition activity is lower than the production rate,

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despite the lower absolute productivity of the ecosystem. In other words, we have to expect lower soil respiration and N2O emission rates there despite the high soil organic matter content. It all depends on the fluxes, not on the stocks and their availability.

p. 5238, l. 15-20: The reasoning on oxygen availability is indeed obvious, but purely speculative. It would have been nice to have data on O2 concentration profiles. As this is not the case, the authors should use a more cautious wording.

p. 5238, l. 23f: You compare your results from a sub-tropical location with data from tropical forests. Is this justified? If you think so, you should give a reason why.

p. 5239, l. 7f: Again, you bring forward the role of tropical forests as methane sinks, but your experimental sites are located in sub-tropical climate.

p. 5240, l. 11: "within the range of other tropical forests of the world": maybe I am too picky, but again, your study sites are not tropical.

p. 5240, l. 15-18, and last sentence of the Abstract: Your data do not form the basis for this statement, as it is contradictory to your own statement on p. 5231, l. 3-4, that you "expected soil gas emissions to vary with altitude responding to combinations of the factors described above", i.e. differences in climatic conditions, species composition and structure, nutrient supply and soil physical and chemical properties. Therefore, it is not justified to assume that a temperature increase alone will lead to an increase in N2O and CO2 emission and in CH4 uptake rates. The interactions between the factors mentioned above in terrestrial ecosystems are too complex, as that one could assume that, if one is changed, the others will remain constant.

Minor points

p. 5229, l. 5: use lowercase for nitrous oxide and carbon dioxide

p. 5229, l. 19: omit "in" after "increased"

p. 5231, l. 3: omit "that" after "expected"

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p. 5231, l. 5: omit “the” before “global”

p. 5231, l. 6: use singular for “gradient of elevation”

p. 5231, l. 7: period after parentheses

p. 5231, l. 7-9: the last sentence of this paragraph appears like an appendix. I suggest moving it further up to the beginning of the paragraph.

p. 5231, l. 12: change to “on the northern coast”

p. 5235, l. 26: use singular for “litter decay”

p. 5236, l. 7: omit “at” before “than”

p. 5240, l. 17: omit “in” after “increased”

Interactive comment on Biogeosciences Discuss., 7, 5227, 2010.