Final author's response: We thank the editor and referee for their comments. Those of the editor require no response. Our responses to Referee 3 follow.

Comment: *This paper explores the relationships between soil properties and aboveground forest biomass, biomass production rate, and individual tree turnover times across the Amazon Basin. It uses the large data set for soil physical and chemical properties that are reported in a related paper (the first one submitted in this set of three) and looks for correlations to assess relationship between various forest attributes and derived indices that summarize soil physical characteristics together (e.g. soil depth, slope,).Overall the work demonstrates the importance of physical soil characteristics in defining vegetation structure and dynamics. A second major point of the paper is the need for correction of correlation statistics if there is autocorrelation with spatial distribution Overall, the soils are a valuable data set as are the spatial patterns of above ground properties and the derived relationships between vegetation and soils across the Amazon region. However, there are some major points that need to be resolved to make the paper complete and publishable. I suggest specifically a reorganization of the Methods and Results sections that include how the major independent variables discussed here are derived (e.g. from primary data collected) see specifics for what this means in the comments below.*

Answer: We thank the referee or his/her comments. As detailed below, they have served to greatly improve the paper.

Comment: *First, and most important, there is no description in the methods section of how the independent variables in this study were derived (these are turnover, AGPand AGP rate). Earlier papers are referenced (as for the soil methods), and contain the necessarily details, but nonetheless the reader here deserves some description as the methods may influence the results. For example, was a single allometric equation used to derive biomass across the entire basin, or were site- or region-specific allometries used? Was the information on density variation part of the calculation of AGB? If so, how important was its variation compared to other components (e.g. mean stem diameter, mean tree height?). For AGB production rate was this based on recruitment as well as mortality or is the stand assumed to be at steady state (recruitment = mortality). Did the treatment of standing dead versus fallen dead trees differ (since the authors make this distinction later in the paper)*

Answer: This is a legitimate criticism. The methodological Sect 2.4 has been expanded and improve, also bringing to the reader's attention a new Supplementary Information providing additional details of the measurements, including the calculations and a study of the relationships between stem density, turnover, basal area, basal increment and with the latter two stand properties expressed on both an area and individual tree basis.

Comment: *Second, variables like turnover time (but also AGB) are derived from other, directly measured variables and distributions may be complicated by interactions among them. For example, turnover (I am assuming this is stem turnover and not biomass turnover, though the authors don't say) is presumably calculated from the number of individuals per hectare and their mortality rate (individuals/ha/year). One can imagine a basin-wide gradient in turnover arising from (in one extreme) constant mortality rates*
and variable stand densities or (in the other extreme) constant stand densities and variable mortality rates. The authors have written most of the discussion as if mortality rates are responsible for all of the variation in turnover across the landscape, but they do not give any evidence that stand density variations are not also varying across the basin. It would be nice to know. Similarly, AGB and AGB production are derived from measures of stem diameter and diameter increment, and perhaps height (these could be given as means or medians or both). Density is already treated separately, though we are not informed as to whether it is used to calculate AGB. Would it not be better to give the relationships to the primary (measured) variables first? This gives more meaning to the relationships to the derived variables.

Answer: Again this is a valid point and we have spent considerable effort in doing what the referee requests and this has, indeed helped in our interpretations (for example as discussed in Sect 3.3 and 4.2. But given the already Wagnerian length of the paper, we have put this information in the supplementary information – the relevant tables being referred to in the main text as appropriate. See Sect. 3.2 in particular.

More detailed comments.

Comment: *Abstract. Considerable importance is given in the text to the need for correcting regressions for spatial autocorrelation. This is not reflected at all in the abstract; especially the fact that the importance of different factors change with the analysis. Perhaps it is worth including a sentence on this.*

Answer: This is now mentioned: (Start of third paragraph of abstract)

Comment: *Line 17. A new hypothesis (line 17). I think the idea of self maintaining forest feedback mechanisms initiated by edaphic conditions is quite old - what is new here is the evidence that supports this hypothesis.*

Answer: As far as we know, no one has specifically suggested this, especially in relation to soil physical conditions. We have, however, rephrased the last sentences of the Abstract to accommodate the referee's concerns.

Comment: *Introduction. Line 23. Perhaps a brief indication of what the authors mean when they say soil fertility would be useful (sum of base cations?). This term is used a lot in the paper and can mean different things to different communities.*

Answer: This is now made clear at the start. More often than not, the paper also now uses 'soil nutrient status' rather than 'soil fertility' as the latter is obviously a subjective and imprecise concept.

Comment: *Methods. Lines 3-5. Again, no methods are reported for how data for turnover, AGB and AGB production were obtained, even though data are apparently updated from previous papers. In this case, there is no mention of what kind of error is associated with the turnover rates given (to two significant figures!); and whether these are multiyear averages that contain information on interannual variation (which at one site is probably at least as big as a factor of 2, given recent Phillips et al Science paper on the effect of the 2005 drought?).*

Answer: We believe much improved Sect 2.4 (with newly created SI) now overcomes this deficiency. Dr Quesada's mentors continue (in vain) to try help him overcome his significant digits problem (now fixed in the text)

Comment*: Line 12. Tree mortality and turnover rates. The authors seem to use these terms interchangeably, implying that all of the variation in turnover rates is due to mortality but the reader is given no evidence that this is true (see comments above).*

Answer: Throughout the paper we are now much more careful; generally avoiding the term mortality rate.

Comment: *Line 25. "physically adverse soil conditions". This is the first time this term is used in this paper, and perhaps it requires some definition (e.g. soils in which physical properties limit root growth, rather than chemical properties that limit access to nutrients?)*

Answer: This is a good suggestion which has been accommodated. See the 5th paragraph of Sect. 2.2

Comment: *Geographical associations and spatial autocorrelation. There authors may wish to cite papers by Holmes et al. (Biogeochemistry (2005) 74: 1738211;203 and GLOBAL BIOGEOCHEMICAL CYCLES, VOL. 20, GB3004, doi:10.1029/2005GB002507, 2006) as examples where recent studies have dealt with spatial distribution of soil properties (e.g. for mapping soil C change with land use at large spatial scales). These papers point out how the magnitude and sign of correlations among soil properties changed with scale, indicating major shifts in distribution and soil dynamics depending on the scale of observation and analysis; (from the abstract of Holmes et al.. 2006).*

Answer: We had a look at this. But – to be quite honest – didn't really see where it would easily fit in.

Comment: P*age 4000, last line Moran's correlograms; Is there a citation for this?*

Answer: We now refer the reader to Legendre and Legendre (1998).

Comment: *Page 4007. It should be explicitly noted somewhere in the paper that AGB gain is not necessarily linearly related to variation in overall net primary productivity. In the central Amazon, leaf litterfall is larger than AGB gain as an overall flux 8211; perhaps the ratio of biomass to leaves:stems:roots varies across the Amazon and that is one reason for the trends in AGB gain (whereas NPP could in theory be constant). The authors do not necessarily say anything wrong, but the reader might make an incorrect assumption if this is not pointed out.*

Answer: This is certainly a valid point and carbon allocation issues are now mentioned at the end of Sect. 4.2

Comment: *Page 4008. AGB divided by AGB-gain is another measure of turnover (biomass turnover). It would be interesting to know how biomass turnover relates to individual stem turnover (which I am assuming is the turnover used in the rest of the paper, given the emphasis on mortality rates?) across the Basin.*

Answer: This is touched on briefly towards the end of the first paragraph of Sect. 4.5. But as mean residence times calculated by the above method are currently the subject of a seperate paper (currently in review) we do not dwell on such an analysis here.

Comment: *Page 4009. Wood density correlations are introduced only in the discussion. We are not told whether or not these data are used in the calculation of AGB, only that it explains variations in AGB. It would be cleaner to list all of the independent variables; 8211; wood density, stem density, mortality rate, mean diameter, height, diameter increment and briefly describe the methods used to measure them, then the derived variables AGB, AGB-increment, stem turnover. That way it is a little more clear what factors underlying the variability in the derived variables comes from. For example, AGB is a function of stem density, diameter, height (maybe not available?), and density. Discussion about what underlying factors cause variations in AGB would follow more logically if the other variables were discussed first.*

Answer: Generally speaking, we have found structuring the Results and Discussion into sub-sections quite a tricky task. Biomass is, however, reserved to its rightful last place in both Sections and with the (now incorporated) stem density, individual tree diameter etc introduced at the start of the Results (Sect. 4.2) and then mentioned in the Discussion as appropriate (for example, basal area increment is considered in conjunction with wood productivity; basal area in conjunction with biomass. On balance, we found this the most logical way to organise things.

Comment: *Page 4011. I am not quite sure what the authors mean when they say (lines 13-15) that "no relationship was found with any edaphic variable when spatial filters were applied";. If (for example) P is correlated with space because of the underlying distribution of bedrock across the basin, does this mean P cannot be said to be controlling vegetation characteristics or does it mean that you cannot say P is controlling because it correlates with space? I guess what is puzzling is the "wrong in principle" statement in line 19. Again, the results of Holmes et al. (see above for citations) may be worth citing here, since I think the authors are finding some of the same things.*

Answer: The referee is correct. The nature of the relationship does not change, only it's likely significance and the 'wrong in principle' was certainly an overstatement. Here is also probably the place to point out that in the course of revising the manuscript, we have re-done the analysis now including three sorts of spatial model. Our totally rewritten Sect. 4.1 (we hope) now reflects a far greater understanding of spatial processes and how one should account for them than was the case for the original version.

Comment: *Page 4012. The discussion of "standing dead" is difficult to understand given that the authors have not said how they derive mortality in the first place (see general comments above). Presumably they counted standing dead in mortality.*

Answer: That standing dead are included in mortality estimates is now explicitly stated in the Methods (SI)

Comment: *Page 4015 "anoxic conditions"; . Is this based on data from 0-30cm soil depth, or the indices that are obviously calculated using the complete soil column information?*

Answer: As outlined in Quesada et al. (2009) the anoxic scores did include depth (lower scores where anoxia was not evident above certain depths). This is now made clear at the appropriate place in the text.

Comment: *Pages 4016-7. Nothing is said here about the possibility of changes in allocation (leaves and roots versus stems) as being a potential reason for variation in AGB/AGB production across the gradient "clearly that is an additional "growth strategy"*

Answer: This is certainly a valid point and carbon allocation issues are now mentioned at the end of Sect. 4.2

Comment: *Page 4019. The missing link in the discussion of kinds of gaps here are data that show the types of gaps are distributed according to soil physical properties. As noted above, these are introduced after the methods/results sections and there is not even a citation*

Answer: That standing dead are included in mortality estimates is now explicitly stated in the Methods (SI). We did not, ourselves, measure "mode of death" or gap size in any of the plots (though this data now exists for many of them and to be the subject of a seperate analysis). That we are talking about forests in general and not our own results *per se* is now made clear in that part of the Discussion.