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Interactive comment on "Carbon / nitrogen interactions in European forests and semi-natural vegetation. Part I: Fluxes and budgets of carbon, nitrogen and greenhouse gases from ecosystem monitoring and modelling" *by* Chris R. Flechard et al.

Anonymous Referee #2

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General comments: This article presents estimates of nitrogen, carbon and greenhouse fluxes and budgets at 40 European flux towers compiled from different sources (observations, models, literature). Overall, the article contains an integrated approach to estimate the total nitrogen flux at these sites as complete as possible, accounting for different N pathways. It includes a lot of useful measurements, such as local wet and dry Nr deposition from a collocated measurement network, as well as other in-situ observations (NO, N2O, soil samples etc.). These nitrogen flux estimates are comple-

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mented with carbon and greenhouse fluxes, as well as auxiliary information (such as climate variables, forest characteristics, etc.), and as such provide a useful database as a basis for assessment of carbon/nitrogen interactions in European ecosystems, which is also in part included in this article. Even though the content of this article is useful and interesting, it is very lengthy and it can benefit largely from shortening and restructuring. It contains plenty of information which could be transferred as Supplementary information. The materials and method section is for instance very long (almost 8 pages) and puts a lot of emphasis on some of the different measurements and data sources. I would highly recommend adding a schematic overview of the different data sources at the beginning of this section to make clear what is used and from what source. Also, a couple of subsections may be added to the 'nitrogen fluxes' section to subdivide the text into different components. The section about the BASFOR forest ecosystem model is also unnecessarily lengthy, while the output of this model is only a small part of the discussed results. To make the article more focused, I recommend to greatly shorten this section or even better refer to existing publications. The results section seems to focus on two major parts: one being the discussion of the resulting nitrogen, carbon and greenhouse fluxes and budgets themselves (and their inter-comparison and validation) and the other being the link between these N and C fluxes and the interpretation. I would recommend separating these two parts as much as possible. Moreover, the "results" and "discussion" sections seem to be partially intertwined. It would be better to exclusively include discussion points in the latter. Some sections, for instance, the one about "carbon sequestration efficiency", introduce new concepts and results and seem to fit better in the results section (see specific comments below). As for the "discussion" section, it would be more structured if there was a clear separation between the methodological uncertainties used in the estimation of the N, C and greenhouse gas fluxes and the uncertainties related to the interplay of N and C. Some of the results need to be updated, e.g. in section 2.2.1 it is written that EMEP data were downloaded in 2013 and different model versions were used? In a lot of statements there is an unevidenced qualification, such as: 'there did not appear to

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be any systematic overestimation ..', '0,85 is plausible but also much variability ..', We assumed that all sites of the European network followed the same relative time course ..', 'reveals a potential cross correlation ..', 'reasonable overall model performance ..', etc. It would help if such statements are underpinned/quantified. Finally, there seems to be some parts of the paper that are outdated, such as the start of the discussion about the Magnani paper. These can be omitted. It raises the question to me about what the purpose of the paper is and how it relates to the second part. If it is a description of the database, including reference to the data sources, models and including an uncertainty estimate it can be a much shorter well focused paper with detailed information as supplementary information. The second part can then focus on the use and interpretation of the data. It would be my recommendation to split it in such a way. I therefore recommend major revision of the paper. Specific comments: Line 127-130 – The information in the introduction about tropical forest could be shortened, as the paper focusses on temperate/boreal forest. Line 159 – 1 by 1 degree does not correspond to 10 by 10 km, it covers a larger area. Line 195 – How about vegetation changes at all sited, e.g. due to growth and/or composition change? Line 220 - Not all FLUXNET sites seem to be included in this analysis, what are the criteria used to in- or exclude individual sites? Line 262 – In this section it is sometimes unclear to what "models" are referred. Specify what type of models, e.g. dry deposition models. What are the key differences between these deposition models? It would also be good to specify the EMEP model domain and resolution in this section. Line 284 – leaving out the organic N data leads to a systematic bias. Line 290 – There are probably new data available? Line 308 – Could you elaborate on why you decided to scale up the N2O and NO fluxes using linear interpolation? Line 326 – DINTF is not included in the abbreviations table. Line 334 – there is an enormous range, why not used in some way? Line 336 – This section, about the EC processing, is lengthy. Consider shortening it. Line 389 – Missing comma between "some sites" and "such as" Line 427 – Add a reference to the table in the supplement that includes the major publications per site. Line 480 – It would make more sense to move the primary purpose of the BASFOR

model to the beginning of the section. Line 538 – 550 – The discussion on the model uncertainties would fit better in the discussion section. Line 580 – 594 – This section discusses the computation of the denitrification losses and the uncertainty associated with it. Consider moving a part of this section to the methods/discussion. Line 603 – 612 – It seems a bit sudden to address the ultimate objective of the whole project here. It would be better to discuss this earlier on in the paper. Line 640 – You state that there is a "broad negative correlation between MAP and MAT", could you support this with a R2 value? Moreover, the MAP alone does not seem appropriate to address site-specific water-availability, without considering other parameters such as soil water holding capacity etc. Line 647 – Missing "were" Line 659 – 674 – In this section you are discussing results related to Figure 4 again. Consider moving this section up. Line 737 – Add a reference for this number. Line 754 – how can you demonstrate that it 'provides a better estimate' and that it is more realistic than an transport model? Line 804 – It would be better to either add the definitions for N saturation here or refer to this later. Line 930 – Why is the uncertainty in the non-CO2 fluxes possibly >100% larger? Line 972–975 – You state here that your attempts with more advanced forms of regression analysis were not successful. This is a bit on the vague side. You should either elaborate on the attempts that were made and/or for instance add some references, or exclude this statement from this section. Line 1016 – It is unclear why considering the uncertainties would amount to a confident threshold of 2-2.5 g N, rather than just widening the range of the given estimate, could you elaborate on this? Line 1052 – 1055 – This sentence is a bit confusing. It refers to the large uncertainty in dry deposition modelling, but both deposition estimates from CTM and denuders make use of dry deposition models in the end. So I would say that it is not so much the deposition models that improved substantially, but more the estimates of the concentrations of these compounds. Figure 4 – What type of LAI data is used to determine LAImax? Figure 8 – Some of the labels seem to be missing. Figure 9 – "to each tohet" – to each other Consider splitting up / reformulating these sentences for clarity/readability: Line 119-121 – "The global terrestrial . . . Pg (C) yr-1" Line 127-130 – "Tropical forest areas

. . . generally believed." Line 297-299 – "Nitrogen losses to . . . empirical methods." Line 308 -309 – "To address . . . responses of soils." Line 525-528 – "Total Nr deposition . . . Schwede et al., 2018)." Line 548-550 – "Wet deposition . . . most forest sites." Line 754-756 – "Despite these . . . cycling processes." Line 790-793 – "In addition . . . as beech or oak." Line 824-828 – "By analogy . . . observation periods." Line 959-963 – "Through the continent-wide. . . continental scale." Line 972-975 – "Our attempts . . . the dataset." Line 1052-1055 – "Nevertheless, . . . is much larger."

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