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Biogeosciences Discussions
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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 #1

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Review for the paper submitted by Flechard et al. MS No.: bg-2019-333

General Comment This is a very well written paper in the suite of other papers on N deposition at the European scale lead by the principal author. In fact, this paper acts as a "prequel" of the companion paper, the Part II dedicated to untangling climatic, edaphic, management and N deposition effects on C sequestration. A previous paper to Part II is important since uncertainties and gaps of knowledge associated to the different com-

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ponents of the N and C cycles in terrestrial ecosystems need to be examined previously to the attempt of disentangling. Therefore, in this paper, to evaluate the uncertainties and gaps in the estimates in N and C budgets, the authors have made a remarkable effort of gathering N and C data from 31 forests and 9 seminatural ecosystems extended over Europe and covering a wide span of climates, from Mediterranean to boreal. To constrain the N budget they have taken advantage of local measurements of dry and wet deposition at specific sites from the NEU (NitroEurope) database, complemented with the use of deposition models (EMEP model) in some cases. Loss of N by nitrate leaching and by gaseous emissions have been estimated by measurements in some sites and modelling when no measurements were available. For the C budget, data were mostly obtained from eddy covariance sites within the CarboEurope Integrated Project (CEIP) combined with laboratory bioassays and literature mining. The results of this big effort of compilation constitute an important contribution to the evaluation of N deposition on C sequestration at a European scale, by critically evaluating the uncertainties in the quantification of some of the drivers. Also, it calls for attention to neglected fluxes that might have a considerable role in the budgets, e.g. N2 emissions by denitrification. The paper is well written, well documented, scientifically sound, and it fits the scope of Biogeosciences Discussions, so I recommend it for publication with only very minor changes.

Specific Comments Abstract The abstract summarizes the main findings, so it is very important to give accurate figures. In this sense, I suggest to review the sentence in line 110, since from Fig. 3F one can see a different range of values of % N losses to total N dep than those reported in the text (10-35% at Ndep below 1 gN m-1 yr-1 and 35-80% at Ndep above 3 gN m-1 yr-1 in the Figure). This sentence is followed with a consideration that 1/3 of the sites might be in a state of early – advanced N saturation. But, from Fig 3F, I deduce that one third of the sites result from considering a threshold value of 2 gN m-1 yr-1. I suggest rewriting this paragraph, also including a suggested of Ndep that might indicate early N saturation. Introduction I like it very much. In line 175, wetlands might be also included for DOC leaching. Methods For

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dry deposition, the inferential method is based in the same 4 models as in Flechard et al (2011). However, it is not clear to me whether the retained value is the averaged estimate from the 4 models, can you clarify? Later on (line 533) it is mentioned that DD is calculated as ensemble average of 4 inferential models, but this should be stated and explained in Methods. For wet deposition, an estimated NO3 and NH4 deposition was attributed to every one of 40 sites through kriging interpolation of EMEP and ICP-Forest data. Furthermore, 13 sites were provided with BD samplers for 3 years so that BD Ndep was actually measured in these sites, and six more sites already were equipped with BD or WD collectors. Can you comment here on how well did compare the kriging estimates to the actual measures? When calculating losses by leaching, it is mentioned that lysimeters were used to obtain soil N (or DOC) concentrations that were combined with a hydrological drainage model to derive the export fluxes. Can you explain better this hydrological model? Was it possible, in any one of the sites of the survey, compare results from N and C exports calculated using the hydrological drainage model and from actual water runoff at gauged sites? I understand that this is not the main focus of the paper; however, just to know how the two approaches estimate losses can be of interest. Results In line 548, when considering organic N deposition, it is seen that WSON is a small fraction of Inorganic and organic N deposition. But, can you comment on the possible role of dry WSON (e.g. urea is important in some cases) deposition? Should this also need to be considered in the N budgets? And then, if the dry organic N flux is considered to be relevant, should it be included in Figure 1? When commenting on N losses, in line 556 reference is made to Fig 3D to indicate greater losses with Ndep above 2 gN m-1 yr-1. It should be specified that this statement is based on measured leaching and Dise's leaching model, but not on BASFOR estimates. Discussion The N balance is presented in Fig 3D (N losses compared to N inputs) and it is shown that a non-linear fit best describes this relationship. Then the authors argue that above a Ndep of 4 gN m-2 yr-1, N losses might "even exceed " the estimated N deposition, but this should occur when extrapolating the line into a region devoided of data. On the other hand, sites of lower N dep (e.g. EN9) have a leaching loss as close to the N

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dep value than other sites with higher deposition. In my opinion, the pattern towards N saturation is best shown in Fig. 3F, when plotting the % of N losses to Ndep. In this plot, to me it is clear that, at Ndep above 2 gN m-2 yr-1, all site mean leaching values are above 35%. The classification in three ranges of depositions (low, intermediate and high) is OK, but as commented in the Abstract, the % ranges of N losses to Ndep should be revised (e.g. for Ndep above 3 gN m-2 yr-1, mean loss% ranges from 35 to 80%.

Minor corrections Line 524: large Line 569: include here Fig. 3A after Ndep 2 g m-2 yr-1 Line 577: Fig 3A Line 586: why include here the units in kg ha-1 yr-1, besides g m-2 y-1? Line 631: This inter-annual peak in LAI, is it the average of various years? What does it mean “peak”? Line 574: better than? Maybe use: provide a good estimate. . . Fig 6: for CSOM, r2 = 0.00, but this seems too low given the distribution of points. . .can you revise it? Fig. 9: include regression, r2 and p in plot A and B. Figures: Identification of sites are generally difficult to read, especially in Figs. 3 and 6

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