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Interactive comment on "Carbon / nitrogen interactions in European forests and semi-natural vegetation. Part II: Untangling climatic, edaphic, management and nitrogen deposition effects on carbon sequestration potentials" *by* Chris R. Flechard et al.

Anonymous Referee #2

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Summary: Flechard et al. use a "meta-modelling" analysis of forest C fluxes and balance to examine the response of these processes to N deposition for ~30 sites (22 forests). They confirm that estimates of C gain from N deposition are smaller if environmental drivers are considered first.

General comments:

Overall, the analysis seems generally reasonable, and broadly supports the past re-

analysis of forest C gain from N deposition by Sutton et al. (2008), which showed a much smaller C gain than imputed from the widely-critiqued Magnani et al. (2007) study. This text seems a bit long for that main take-home, with a data set only a bit larger – though analyzed in greater detail that that earlier dataset. It would be nice to have somewhat more focus in parsing this overall NEP response (i.e., more GPP vs less Reco or Rh?) beyond the surprisingly large reported GPP response.

Extensive discussion space is used on C sequestration efficiency (CSE =NEP/GPP), though it's not apparent quite what this adds over more in-depth examination of the individual C flux responses that go into this ratio. In particular, discussion of mechanistic explanations for the N effects on GPP and Rh (or Rsoil) would seem to be more directly related here – i.e., to explain saturation of the GPP response (discussed reasonably), or suppression of decomposition as a substantial portion of the overall dC/dN response.

The direct effects of N on decomposition process appears largely restricted to the last page of the Discussion, and they merit much greater attention earlier and throughout the manuscript.

The reported response of a saturation of the growth response to N dep, coinciding with an increase in N losses, fits exactly within expectations of N saturation theory (e.g., Aber et al. 1989, 1998, BioScience), which deserves more explicit recognition and discussion.

A less central suggestion: The authors state the importance of detailed site-level N deposition values over estimated modeled ones. While believable, this point would be supported more substantively by showing it directly, e.g., by comparing estimated v measured N deposition values, and quantitatively comparing dC/dN results for these two types of N deposition estimates.

Overall, the manuscript might be revised to reduce sometimes redundant-seeming extensive discussion of CSE, and provide greater and more direct emphasis on its novel insights (beyond Sutton et al. 2008, or classic N saturation theory).

Detailed comments:

Abstract

Line 65 – somewhere in the abstract, specify the number of sites included in this analysis

Line 67-71 – The reduction of dC/dN from considering factors other than N deposition was for GPP, not NEP, right? that should be clear in the abstract, which describes this response in terms of C sequestration, which generally aligns more closely with NEP. Similarly, be clear about which C cycle term yielded the 40-50 gC/gN response.

Line 74 – text indicates that the dC/dN response saturates above 2.5-3.0 gN/m2/yr "due to" leaching and other losses.. but the latter don't appear to be measured here? If this attribution is from the model analyses, do indicate that as a modeled result.

Main text:

Line 81-93. The cited references provide examples of experimental studies that indicate little or no increase in C sequestration from N addition, and might be presented as some of the conflicting evidence for a universal N-deposition induced C sink, rather than challenging the entire notion of this phenomenon in its entirety

Line 92. Is the Dezi et al. 2010 reference a model-based analysis of dC/dN? If so, clarify that it's different from the empirical approaches of the other studies

Line 93-97. In this review of dC/dN values – and throughout the manuscript – be clear as to which values pertain to which C pool (i.e., tree, soil, or whole ecosystem) or which specific C cycle processes.

Line 107 – 120. It's not wholly apparent why this review of basic N balance processes is needed? That is, nearly every forest N budget shows that new N deposition supplies only a small fraction of plant annual N demand compared to internal N recycling; the

key (missing?) point is that the value of N deposition is that it may be acquired directly or at little energetic cost to plants, and can accumulate over time.

Line 121. Add specific citations to this sentence critiquing "some previous estimates" for failing to account for factors other than N deposition.

Line 122-130. This section seems somewhat oversimplified in pitching its novelty: N deposition can, but does not necessarily covary with gradients of other environmental variables; this covariation often depends on the geographic region selected. The Magnani et al. (2007) simple regression analysis indeed failed to consider this covariation, but its problems seemed very effectively addressed by the Sutton et al. (2008) reanalysis, in demonstrating the need to consider variation in factors besides N deposition. Is the goal in this manuscript to do a similar analysis of tower-based C balance measurements in greater depth than that one? Other gradient analyses have also considered N deposition along with other environmental drivers, sometimes also considering nonlinear responses (e.g., Solberg et al. 2009, Thomas et al. 2010).

Line 175-176. Elaborate on what exactly is meant by "soil C pools rely on various assumptions or empirical models for their estimation." Assumed and modeled soil C can often vary markedly from measured values; how well do these assumptions work?

Line 184. Specify the minimum and/or mean number of years of EC data are used to compute the C fluxes of interest here.

Line 202-204. Does this model's soil dynamics allow it to represent the inhibitory effect of N deposition on soil decomposition? If not, this point should receive explicit attention in the Methods and/or Discussion, on how this effect of N was considered in this analysis.

Line 207-208. It's certainly difficult to reliably simulate N loss fluxes to DON and N2, and it's correspondingly understandable that this set of model-based estimates would not include them. However, when measured, these fluxes can dominate ecosystem N loss fluxes and should thus receive more attention as to the uncertainties introduced by their omission in these calculations.

Line 246 / Section 2.2.3 – why focus so much text here and the Discussion on the ratio of C sequestration _efficiency_, CSE, (NEP / GPP and similar), rather than NEP itself and its component parts (i.e., increased GPP? Suppressed Reco or Rh)? The Introduction provides no context or central questions for focusing on questions concerning the CSE ratio, and so this emphasis seems somewhat unexpected and extraneous here, and the lengthy text in the Discussion (∼line 600-700)

Line 265 – late text (line 273) indicate N fixation wasn't considered, so be consistent with that point here

Line 314-316 – identify what is "the broad pattern of GPP vs N dep. in Flechard et al. (2019)."

Line 373-374 – in this N balance (N mineralisation + N dep – N plant uptake – N leach – N emissions), what about accumulation of N in soils or soil organic matter? Often a very large if not the largest sink.

Line 457 – suggest "at the low_er_ N dep sites…" That is, 1.0 g/N/m2 is often considered elevated.

Line 466 – clarify which "this set" is meant – higher or lower N dep group?

Line 468-485. These dC-GPP/dN values are simply _enormous_ and correspondingly difficult to fathom – even when reduced from 425 to 234 gC/gN! How do these compare to empirical NPP values? Presumably a 50% GPP to NPP efficiency would yield something like 212 to 117 gC/gN, still well beyond empirical NPP responses. How / why is it so large compared to eventual dC-NEP/dN response?

Line 495-502. These dC-NEP/dN values (∼40-60 gC/gN) seem more consistent with empirical responses: to what extent are these values due to modeled plant vs soil C sequestration? Is the soil C sink from additional litterfall inputs or from suppressed

decomposition?

Line 535. Yes, the results here seem to confirm that of Sutton et al. (2008). How does this analysis provide additional insights beyond that one?

Line 537. Provide citations for observations of N losses. Thresholds of 0.8 – 1.0 g N/m2/yr for N leaching have been reported commonly (e.g., MacDonald et al. 2003, Global Change Biology, and similar).

Line 540. These responses are exactly as expected – i.e., The saturating response of ecosystem NPP to N deposition, and corresponding increase in N losses, are standard predictions of classic N saturation theory as originally proposed (e.g., Aber et al. 1989 & 1998, BioScience). Discuss how this work provides an advance over that prior set of expectations.

Line 560-570. This paragraph states that the detailed, more-precise N deposition measurements improve calculation of dC/dN responses.. and while plausible, it should first be demonstrated how these estimates compare with the simpler alternative.

Line 580-582. This conclusion on Reco vs N dep does not seem to have been discussed in the Results?

Line 585. Per above, the annual N input is small relative to annual N demand. But its accumulation over time can support a much larger fraction of N demand.

Line ∼590. This would seem one of several places to mention the effect of N on decomposition

Line 598 – 708. It's not apparent why so much emphasis is placed on carbon sequestration efficiency (CSE = NEP / GPP) rather than the component C fluxes (GPP, NPP, NEP, Rh). It seems somewhat redundant with these other responses, and a direct outcome of individual responses. What additional insights does it provide?

Line 649. A large soil C stock doesn't necessarily indicate higher heterotrophic respiration responses – and can result from the opposite situation (i.e., lower Rhet allows more soil C to accumulate).

Line 652. The history of N and S deposition at this site (EN8) indeed might be important. What about considering cumulative N deposition across the range of sites?

Line 709 onward. This seems quite late for a first substantive mention of the direct effects of extra N on decomposition and belowground processes, often shown to be of comparable magnitude as many aboveground responses (e.g., Janssens et al. 2010, Frey et al. 2014). In addition: does the modelling approach consider these processes, or would it miss them?

Line 714. Add citation(s) for this "traditional theory of role of N..."54

Line 737-742. This content seems more appropriate to the Methods, as a general data synthesis activity part of this study. Similarly line 743-763 seem more appropriate to Results.

Line 775. NEP dC/dN of 40-50 is on the lower end of inventory? Many inventory-based assessments seem to show something in this range, with ~20-25 for trees and 20-25 gC/gN for soil.

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