Title:LTER: Long Term Ecological Research at the Hubbard Brook Experimental Forest
Institution: Institute of Ecosystem Studies
NSF Program:LONG TERM ECOLOGICAL RESEARCH
Principal Investigator: Lovett, Gary M.

Rating: Very Good

Review:

In the context of the five review elements, please
evaluate the strengths and weaknesses of the proposal with respect to intellectual merit.

The overarching theme of this proposal is the impact of disturbance on Northern Forest ecosystems. They focus especially on atmosphere chemistry, climate change and changing biota as agents of disturbance and express a clear interest in assessing how these disturbances may interact to drive changes in ecosystem structure and function. A relatively new focus is on spatial heterogeneity that comprises a geophysical and historical template upon which ecological interactions play out to determine dynamics of food webs and biogeochemical cycles.

This proposal contains a number of very exciting ideas that emerge from long term data and continuation of this work seems very likely to contribute in several significant ways to a deep understanding of ecosystem structure and function. The most exciting specific elements, in my view, are the impacts of the lengthening of the vernal window on biogeochemical cycles and food webs, the idea that mineral soils can act as an 'N bank' and the impacts of that on N losses, and the mysterious impacts of Calcium addition on forest and stream N dynamics.

The main weakness of the proposal is a lack of clear discussion on how research elements are linked, how disturbances may interact, or how processes may be integrated to produce interesting insights into ecosystem dynamics. The structure of the proposal erects barriers between different aspects of the work that, if reflective of how the research will proceed, will limit the impact of this project. Examples of this include a lack of discussion of how 'changing biota' may be responding to the other disturbances and what feedbacks may arise; how N accumulation, Ca deficiency, and gaseous loss of N may be influencing each other; how in-stream processes may be influencing NO3 exports; and how food web-based feedbacks on disturbances may influence biogeochemical cycles.

This proposal has a number of strengths, which are balanced by some less-critical weaknesses.

1) Previous work has made good use of long term data to uncover interesting and unexpected patterns of ecosystem response that beg for explanation. It is in many ways an iconic example of why we need long-term datasets, and shows how interesting and highly relevant puzzles arise that can lead to deeper insights into ecosystem function. The proposed work on drivers of N losses, in my opinion, is the best example of this in the proposal.

Some of the highlighted results, however, are not really major advances in understanding, but

restatements of things that had already received some attention in the scientific literature. In particular, statements about plant and microbe resource allocation, changes in the identity of limiting nutrients during succession and N and P stoichioimetry lack novelty. Some of this is reflected in the proposed research as well.

2) The comprehensive treatment of disturbance and spatial heterogeneity at multiple scales is laudable and if carried out in a truly integrative way, with rigorous attention to interactions between disturbances and explicit acknowledgement of cross-scale (space and time) dynamics, will lead to fundamental advances in our understanding of the response of ecosystem structure and function to environmental change, especially in forest ecosystems.

The proposal, however, does not show convincingly how this comprehensive treatment will be accomplished. The conceptual model is too vague and general and does not show explicit, detailed recognition of interactions between disturbances. The overarching model in fig 1 is not terribly informative. It lacks a clear connection between the conceptual framework and the elements of the proposed research. It may be too much to ask for an integrated quantitative model, but a more extensive attempt to illustrate cross links between scales, disturbances, levels of organization, and spatial heterogeneity would make me more confident that a strong, comprehensive understanding will grow from this work.

In addition, a more minor concern is that the description of 'changing biota' does not clearly distinguish how it is being treated as a disturbance. A number of statements in that section treat it as more of a response to some other disturbance that has important consequences for ecosystem characteristics. This may be somewhat semantic, but, given how fundamental the concept of disturbance is to the proposal, I suggest that this should be considered carefully moving forward.

3) I like the use of well-developed models with strong historical roots in the ecological literature as described in Box 1. This will broaden the appeal of the research and deepen its potential impact.

These models, however, are treated quite separately, applied in a piecemeal fashion and it is not clear how or if they can be integrated. I believe this may reinforce the apparent lack of connection between parts of the proposal. This seems especially relevant to the integration of biogeochemical cycles, climate change and changing biota.

4) The response to the mid-term review is generally well thought out and they appear to have responded to most comments quite appropriately.

Responses to comments about the inclusion of more stream ecology, however, seem to have left out any discussion of stream biogeochemistry, particularly nutrient retention. I'm a little surprised that there is no plan to look at nutrient spiraling, especially given the changes in stream productivity, hydrology and NO3 concentration and export. Nutrient spiraling theory seems like a powerful way to link these parts of the proposal to two of their great puzzles; persistently low stream water NO3 concentrations and the increase NO3 export in the Ca enriched watershed.

In the context of the five review elements, please
evaluate the strengths and weaknesses of the proposal with respect to broader impacts.

Previous work has been linked to policy in a number of significant ways, and these activities will continue to be strong moving forward. Education and outreach programs have been successful, and will also continue to be. Overall, broader impacts are good.

Please evaluate the strengths and
weaknesses of the proposal with respect to any additional solicitation-specific review criteria, if applicable

Program management is tied fairly tightly to the structure of the HBES organization, and seems to be dominated by the PIs. It is unclear exactly how the co-PIs and other personnel will be involved. I don't see any particular problems here, and the tight connection with HBES is important, however, the lack of an executive committee of LTER personnel should be considered.

Summary Statement

This proposal has a number of interesting ideas spread throughout the research elements, and is very likely to continue to produce new insights into forest ecosystem structure and function. The proposal, however, lacks tight integration and a detailed conceptual framework that clearly shows how connections across scales, disturbances, and levels of organization will be accomplished.