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.

I have concluded that the overall proposal score should be Vey Good; while the studies are well formulated and indeed meet very rigorous standards of basic scientific research, there is one topic that I found missing, and that is consideration of the multiple influences of biota on the climate. The proposal does address the converse of that relationship, but seems weak or missing in regards to the effects of biota on climate. Included in the missing category are the collection and analysis of satellite data records that cover HBR and larger spatial scales. Given the field that is represented, and given that it is very common in that field to study the effects of the environment on biota, it is not at all surprising that these researchers have limited their focus.

The proposed research is a logical extension of characterizations, data analysis and modelling at the Hubbard Brook Forest (hereafter, HBR) that have been underway since the 1960's. In fact, the "roots" of this research can be traced back to original studies of acid rain done in the 1960's and 1970's by Prof. Gene Likens. That research began an intensive observational effort in much of N. America, Europe and elsewhere to document and understand the nature of the chemical composition of rainwater and its effects on biota. This effort should be continued, and I would argue that it should be expanded to include the effects of biota on the climate.

Beginning in long-past decades, there has been a struggle of sorts to demonstrate that the study of the impacts of human activities can be a proper focus for basic science research. Indeed, by now that sort of focus has been accepted; however, it seems important to recognize that this research developed through a lengthy process in which its merit seemed to be in almost continuous doubt. As a person who works in related fields (meteorology and air chemistry) I have had first-hand experience in establishing the field as a valid scientific endeavor.
The definition of SCIENCE that I use also should be stated in order to provide a context for my comments.

From Wikipedia:
Science[nb 1] is a systematic enterprise that using mathematics and measurement, creates, builds and organizes knowledge in the form of testable observations, explanations and predictions about the universe.[nb 2][2]:58
That is, "science" consists of two intimately-linked activities.... observation of the real world and reduction of the observations to predictable laws. "Science" is not just observation, nor is it just

development of theory. "Science" is the linkage of those two activities. This proposal thus properly meets the definition of "science" because it proposes a balance of observation and synthesis of the data. My one and only suggestion is that the work could be expanded to include the role of the biosphere on climate; in this case, the very important role of the forest ecosystem on the climate of the region and the globe. What might the temperature data look like if HBR were cut down? What is the importance of evapotranspiration to regional climate? How does that midlatitude deciduous forest fit in the global climate picture?

As to the specific four out of five key principals that characterize LTER:

1. Formulation of a conceptual framework that integrates across ecosystem components.

I judge that the proposal in hand does an admirable job of integrating the traditional components of ecosystem components.

As indicated earlier, I would like to see another component added that considers to effects of the ecosystem on climate (such as evapotranspiration and its effects on the regional cloud field).

2. Use of this framework to develop predictions that link processes and observations across levels of organization, temporal scales or spatial scales.

Again, the general missing area of the effects of biota on the climate come to mind, The spatial scales that may be involved range from very local to global. E.g., the rate of local photosynthesis eventually influences the global level and variability of CO2.

3. Identification of important, general ecological questions that a) derive from key theories, b) are motivated by the analysis of lonmg-term data, and c) require additional long-term data to be answered.

Here, the scientific questions that are formulated could be expanded to include climate, and the needed data could very well include data acquired by satellite. With the relatively new data streams from the satellites of the A-Train and from DSCOVR, it should be possible to find linkages of the local scale of HBR to larger and up-to-global scales. Empirical observations like that (which are just now coming on line) promise exciting new scientific questions that should appear soon, and that will undoubtedly reach down to the scales of ecosystem in the HBR.

4. Development of predictive models.

Among the most difficult questions are those that relate to prediction of future climate changes. It seems obvious that the forecasts to date are just simple linear extrapolations of current observations, e.g., of temperature. However, if there is anything that we should expect to be non-linear, it is climate and most especially its non-linear responses to anthropogenic forcings. Of key importance and relevance for HBR are those questions that can be addressed all or in part at the scale of HBR, and of added import is the extension of those findings to larger and larger spatial scales including the global one. There are two types of non linear response that might be considered. First is the response to a forcing that changes the climatic state from one stable mode to another. Consideration of changes of

this sort obviously require an ecological component. Second, and something that must be avoided at all cost is the transiton from a stable climate to an unstable or runaway system. Above all else, it is imperative to not constrain models to stable solutions.

Under the directions given early on for doing this review, a section called "LTER Review Criteria" was included. Because I am not an ecologist, and thus not totally familiar with the nomenclature used in these "Criteria", my conclusions that relate to them are more limited. Here I have resorted to the use of CAPITAL letters within the original text as a way to make comments.

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

Outreach and Education including training of students, K-12 Schoolyard activities, application of results to management or policy decisions, outreach to the public, and others as relevant.
Each of these components should be evaluated for quality, productivity, and impact. I AM UNIMPRESSED WITH THE TERM "OUTREACH" AND INSTEAD FALL BACK ON THEIR VERY GOOD RECORD OF PUBLICATION IN REFEREED SCIENTIFIC JOURNALS. IMHO, THERE IS FAR TOO MUCH EMPHASIS ON OUTREACH, K-12, ETC. I WOULD BE VERY CAUTIOUS IN PUTTING RESOURCES INTO SUCH ACTIVITIES UNLESS THERE IS A REFEREED PUBLICATION INVOLVED.

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

Data collection at all LTER sites centers on five core areas:
1) primary production
2) population dynamics and community trophic structure
3) organic matter accumulation
4) inorganic inputs and movements of nutrients through ecosystems
5) patterns and frequency of disturbances.

THE PROPOSAL MAKES CLEAR THAT THERE IS AN ONGOING AND CONTINUING OBSERVATIONAL BASIS FOR THE ENTIRE LTER PROJECT. ONCE AGAIN, MY ONLY SUGGESTION ABOUT A POSSIBLY MISSING AREA OF OBSERVATION IS THAT FROM THE MANY SATELLITES THAT ARE CURRENTLY IN ORBIT AND YEIELD OBSERVATIONS OF THE AREA IN AND AROUND HBR. THE ADDITION OF SUCH INFORMATION WILL NOT ONLY AUGMENT THE OBSERVATIONS DONE AT HBR, IT WILL ALSO PROVIDE A MUCH LARGER CONTEXT FOR THOSE LOCAL OBSERVATIONS.

Proposals should include the following key components, which encompass both Intellectual Merit (#1) and Broader Impacts (#2 and 4) review criteria:

? An integrated, six-year research plan that addresses a set of focused questions. Questions

should arise from analyses of long-term data and advance understanding of key ecological concepts. Justification must be provided for at least 6 more years of data collection to answer these questions. Proposed cross-site or non-LTER collaborative research must fit within this cohesive research plan. A SET OF FIVE INTEGRATIVE SCIENTIFIC QUESTIONS IS GIVEN ON P 13, AND THESE SEEM CLEARLY ANSWERABLE BY CONTINUED DATA COLLECTION FOR AT LEAST 6 YEARS. THESE QUESTIONS APPEAR REPEATEDLY IN THE PROPOSAL, AN INDICATION THAT THEY ARE KEPT IN MIND CONTINUALLY.

? Information Management and Technology, including milestones and deliverable products from data management that result in availability of all data via the LTER Network Information System THERE CAN BE LITTLE DOUBT THAT THE REQUIREMENTS OF THE SCIENTIFIC QUESTIONS REQUIRES AN ONGOING AND CAREFULLY CRAFTED SCHEME FOR DATA MANAGEMENT. THIS MAY WELL BE THE MOST CHALLENGING OF THTE TASKS AT HAND.

? Project Management, including personnel, fiscal, administrative, institutional, and logistical issues. Involvement of new or early career researchers in project activities is encouraged. THIS SEEMS TO BE THE WAY THAT NATURALLY OCCURS TO THOSE RESEARCHERS.

Summary Statement:

To evaluate the scientific goals (intellectual merit) of the proposed research, please consider to extent to which it:

? proposes a cohesive, integrative, and synthetic research plan that focuses on important, general ecological questions. These questions must be motivated by existing long-term data but require additional, long-term data to be answered

? advances understanding of key concepts, questions, or theories in ecology and ecosystem science

? encourages new conceptual frameworks and develop new models that will broaden understanding of site-specific and cross-site dynamics

? if social science is proposed, draws from and contributes to social science theory and understanding

? expands research at a particular site by including cross-site collaborations or collaborations outside of the LTER network and by attracting other researchers, approaches, and questions.

AGAIN AS A NON-ECOLOGIST, I AM UNABLE TO JUDGE THE BENEFIT TO ECOLOGY AND ECOSYSTEM SCIENCE OF RESEARCH LIKE THIS. BUT, IN GENERAL, THE WORK DOES SEEM COHERENT AND WELL PLANNED. AND, WITH THE ONE EXCEPTION OF THE USE OF SATELLITE OBSERVATIONS. THE OBSERVATIONS SEEM WELL CONNECTED TO THE MODELS THAT ARE DESCRIBED. I WOULD LIKE TO SEE MORE EMPHASIS ON THE INFLUENCE OF BIOTA ON CLIMATE, AS STATED EARLIER. AS TO THE EFFECTIVENESS OF THE CONNECTIONS TO THE ARTS (P 32. SECTION 2.9.3) IF ANY ASPECT OF THIS CAN BE JUSTIFIED ON SCIENTIFIC GROUNDS, IT WOULD BE GOOD TO HEAR OF IT. AS A SCIENTIST, I FIND IT DIFFICULT TO UNDERSTAND WHY EFFORTS LIKE THIS ARE INCLUDED.