Proposal Review 1 : 2224545

Back to Proposal

Agency Name:	National Science Foundation
Agency Tracking Number:	**2224545**
Organization:	
NSF Program:	LONG TERM ECOLOGICAL RESEARCH
PI/PD:	Groffman, Peter
Application Title:	LTER: Long Term Ecological Research at the Hubbard Brook Experimental Forest
Rating:	Excellent

Review

Summary

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

Strengths:
-HRB has been incredibly productive and has assembled a well-qualified team.
-The most compelling part of the proposal, to me, is the mix of planned and natural experiments. The Ca addition, fertilization experiment (one of the longest running apparently), and experimental ice storms jumped out. HBR is also doing a nice job setting itself up to use the forthcoming emerald ash borer outbreak.
-Related to these unplanned experiments, I enjoyed reading the section on ecological surprises. This is a strength of LTERs in general, because they have long enough data-streams to determine when ecological changes are outside of a historical range of variability. The lack of forest limitation by N was especially interesting.
-Long-term data-sets are adding to the understanding/debate about rapid insect declines, which may turn out to be an (unfortunately) widespread phenomenon.
-The site is engaged in strong within and across site syntheses. The volume and novelty of these is far above average.
-Similarly, the use of process-based models exceeded my expectations.
-Finally, but perhaps most importantly, I found the conceptual framework to be ecologically novel. It also effectively tied together a wide range of research.

Weaknesses:
-With the proposal anchored to the idea of control points, I was expecting the authors to unpack that general mechanisms that tend to create control points. There was a small paragraph on this pointing to all the areas of theory that contribute to the idea that control points exist, but it was rather short (lengthwise) and on details. In this same vein, I think there is a bit more room to bring in complexity and network theory to bolster this understanding of control points. For instance, I would expect nodes to be more likely to be control points when they have high connectivity, average interaction strength, and greater betweenness centrality.
-Really minor point, but the authors describe changing temperature and precipitation as “disturbances.” This really does not match most definitions of disturbance.
-There is a section on experiments and observations to determine the role of mycorrhizal fungi, which seems like worthy goals. The authors might consider taking this to the next level and including fungicide treatments, which have been able to quantify net effects of these mutualisms. It also wasn’t clear to me if this research area, including the use of labelled isotopes, will be connected to the idea of between tree communication and resources sharing.
-I could have used more detail on how machine learning will be incorporated—this isn’t a trivial addition and there wasn’t much detail.
-I’m initially skeptical about using MODIS to estimate light availability in streams. Of course, the high temporal resolution of MODIS is attractive, but the spatial resolution is quite course, so I only see this being accurate in very wide streams.
-A box or two on how you will identify control points, statistically, would have been a nice addition. It looks like some members are well-versed in suitable techniques, such as GAMs, so I am optimistic that this won't be an issue.

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

HBR is doing an exceptional job in this category. Accomplishments include regular meetings with managers, growing connections with indigenous groups, recruiting and training for historically under-represented groups, and collaboration with social sciences. These strike me as very two-way interactions, with an earnest interest in learning from historically underrepresented groups to improve the academics and inclusivity of their program. I saw no weaknesses to be concerned about.

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

-The data management plan looks sound to me. I was a little surprised that some of the time series in the proposal only ran through around 2018. I’d encourage the authors to include fuller time series when possible to demonstrate that all data-sets are up-to-date.
-HBR does an excellent job addressing the five core LTER measurement themes, especially biogeochemistry, hydrology, and net primary production.

Summary Statement

Hubbard Brook (HBR) is centered around mountain forests in the northeastern U.S., with a particular emphasis on the impacts of climate, acid rain, and nutrient pollution. The site has and continues to play a leading role in research on biogeochemical and ecohydrological responses to the pressures. In recent decades, they have expanded their observations of higher trophic levels as well. In the current proposal, the central theme is control points, which are defined in points in time and space where rapid ecological changes can occur. A key example is a recent rapid increase in evapotranspiration. The site continues to be a leader in its field and I believe that will continue to be the case based on this proposal.

Proposal Review 2 : 2224545

Back to Proposal

Agency Name:	National Science Foundation
Agency Tracking Number:	**2224545**
Organization:	
NSF Program:	LONG TERM ECOLOGICAL RESEARCH
PI/PD:	Groffman, Peter
Application Title:	LTER: Long Term Ecological Research at the Hubbard Brook Experimental Forest
Rating:	Excellent

Review

Summary

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

The proposal lays out a novel conceptual model that brings together numerous themes of research at HBR. The proposal has a clear overarching goal, and organizes the proposed research under 3 disturbance themes - changing atmospheric chemistry, changing climate, and changing biota. These disturbances interact with the existing biogeophysical template to change ecosystem function, which in turn can feedback on the biogeophysical template. This novel conceptual framework brings together ecological theory from across numerous subdisciplines in an exciting way.

The proposal leans heavily on long term data analysis, but also incorporates an exciting new prediction component. The PI team proposes to use a family of models (PnET-CN/BGC) to model ecosystem responses to changing disturbances, and also using the theory of disproportionality and control point theory - to predict locations in the landscape that are vulnerable to change or may accelerate rates of change. This is a compelling new addition to the body of ecological theory, and has the potential to be transformative in our understanding of how ecosystems respond to anthropogenic disturbances. I think one slight weakness of this conceptual framing is a lack of complete discussion about how these disturbances interact - the authors acknowledge that these things are all changing at the same time, but there might have been more discussion about how they will disentangle the complex (and potentially compensatory) effects of these numerous and interacting disturbance drivers.

The PI team makes a compelling argument for the continued focus on maintaining to collect a set of core data - many of the important discoveries from

HBR have been made by noticing “unexpected” patterns in the long-term data, often years after the disturbance or treatment. This provides compelling justification for continuing to focus on these data sets, but use them to ask new questions.From my reading of the proposal, it seems that most of the data collection efforts are focused on maintaining/continuing collection of existing data sets, with some interesting new additions (for example: N tracer experiment, expanded N/P limitation experiments with focus on mycorrhizal communities, sap flow measurements, enhanced soil respiration measurements). I think the addition of enhanced groundwater/subsurface monitoring on the “biogeophyical template” theme is warranted (given the focus of control points in the research questions) and complements the streamflow monitoring nicely. The proposal to do enhanced monitoring in 3 transects will yield useful plot scale/hillslope scale understanding, but is not likely to help with scaling to watershed scale given the limited scope of this data collection. But some initial data collection has to start somewhere/is necessary for the control point/prediction work.

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

The broader impacts are focused on 4 goals - establishing relationships with stakeholders, recruiting next generation of scientists, supporting DEIA, and driving innovations in research, outreach and education. The proposed activities are largely a continuation of existing activities - in particular the YVoS program, Schoolyard LTER program, “Zoom a scientist” program, and “Hubbard Brook in a box” toolkits, and the Art-Science/WaterViz programs. New activities include CBUR experiences at Virginia Tech and CUNY, which is a nice addition. The use of existing activities is a strength in that it builds on existing programming, relationships, and resources to expand the reach of these programs, making success more attainable.

I think the strongest addition to the BI program is the incorporation of the Critical Ecology approach. I think this is a really neat addition that will not only lead to new theoretical frameworks and broadened participation, but will also bring a more anthropogenically oriented perspective to research at HBR, which is warranted given the research focus on largely human-driven disturbances.

The only weakness that I see in the BI is the lack of a specific plan about how relationships with tribal groups will be built. This is a really sensitive and important task, but there is no clear plan for how these relationships will be initiated, deepened, or sustained. Without more specifics (letters of support from tribal liaisons, inclusion of co-PI with expertise on Indigenous knowledge or land use histories) this part of the proposal feels a bit shallow. If this could be achieved, however, it would be a huge strength of the BI efforts. I sincerely hope that these activities are happening, and perhaps detail could not be included in the proposal.

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

The data management plan is detailed and specific. The postdoctoral mentoring plan is not particularly novel, but does include the basic components to ensure successful postdocs (formalized planning via IDP, opportunities to give seminars, access to informal networking and inclusion on committees, conference support, grant writing opportunities, opportunities to mentor students).

The solicitation states that the site management plan should address the recruitment of new scientists onto the project (along with efforts to integrate non-LTER scientists into research activities).

Some of the co-PIs on the project appear to be new to the LTER (but this is difficult to assess as we do not have a current list of PIs), but it is not clear how new scientists at the faculty or co-PI level will be included/recruited to work at HBR over the duration of the next grant cycle, or how these newly recruited researchers will be recruited to represent more diverse groups.

I will also note that in looking at the budget, it appears that the vast majority of PI level salary is going to the two lead PIs. There are many senior level PIs that are not receiving salaries, but this is particularly notable for early career scientists. I think only a few earlier career co-PIs have any salary included at all. This raises the question of whether resources are being distributed equitably to those at various career stages.

Summary Statement

This proposal is focused on understanding how multiple types of disturbance (changing atmospheric chemistry, climate change, and changing biota) interact with control points on the biogeophysical template to drive subsequent changes in ecosystem function of northern hardwood forests. The proposal builds on a strong track record of success in the previous LTER cycle (in terms of publications and external grant funding) to use/leverage existing data sets in new ways to test novel ecosystem theory. The theory tested spans multiple levels of ecological organization, and touches upon the five core LTER research areas. The proposed activities will support the continuation of numerous successful BI activities, and will include a new and exciting “critical ecology” framework that will build new theory and provide avenues to broaden participation in ecological research. The PI group has demonstrated the continued importance of these sorts of long term data collection efforts for understanding forest ecosystem response to climate change, and present a compelling proposed set of new research questions that build off prior work as the focus for this funding cycle.

Proposal Review 3 : 2224545

Back to Proposal

Agency Name:	National Science Foun
Agency Tracking Number:	**2224545**
Organization:	
NSF Program:	LONG TERM ECOLOGICAL RESEARCH
PI/PD:	Groffman, Peter
Application Title:	LTER: Long Term Ecological Research at the Hubbard Brook Experimental Forest
Rating:	Excellent

Review

Summary

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

This proposal has a high potential to advance knowledge in several key areas. The focus areas of the proposal identify the interactive factors affecting northern forests, including recovery from acid deposition, invasion from pests, disturbance, and climate change. HBR is uniquely positioned to examine how these interactions affect ecosystem processes and broader outcomes. Perhaps the greatest strength of the proposed research is how it builds upon the wealth of long-term data spanning multiple disciplines, paired with a legacy of cutting edge research interpreting these data. This previous work will be essential in disentangling interactions between drivers of key ecosystem processes. Furthermore, because the drivers of many of these process are emerging (e.g., emerald ash borer) or intensifying (e.g., climate change), it is only through continued long-term data from this experiment that we can fully understand their effects, especially within the context of long-term effects recovery from past air pollution. While the specific combination of the timing and magnitude are perhaps unique to HBR, the framework should produce results that can integrate across populations, communities, and ecosystems.

I found the rationale and approach described in the proposal very compelling. It clearly addresses the five LTER core areas, all in a substantive way, though the overarching theme seems to be “patterns and frequency of disturbance”. The proposal highlights numerous drivers of change that are playing-out in current time at a range of temporal and spatial scales, and explains that some of the ecosystem responses are unexpected and many of the mechanisms need further exploration. Proposed new work on the effects of changing atmospheric chemistry on ecosystem processes and fluxes

(Figure 9) builds upon previous work and utilizes a useful set of new measurements and techniques. The analyses of soil organic fractions and associations using archived soil, as well as isotopic tracers for C and N analyses are important elements to this portion of the proposal. Biogeochemical cycling and biological responses to pollution have always been some of the major strengths of the work from HBR and this proposal correctly identifies and seeks to address the next set of questions emerging from this longterm work.

I was most interested and excited about the new work examining climate change and coupled water and carbon cycles (Theme 2). The complementary approaches (streamflow, eddy covariance, sap flow) for estimating components of ET incorporate established methods to help understand the relative contributions of various drivers to the large increase in ET. These measurements and analyses, combined with the broader-scale examination of variability of seasonality, provide a strong, comprehensive approach for predicting ecohydrological sensitivity to future conditions.

Theme 3's focus on biological communities was based on a logical set of questions and the combined effects of downslope tree migration and anticipated loss of ash trees from the system provide new challenges in understanding and predicting future forest processes. I also appreciated the approach in Theme 4 to focus on the biogeophysical system and hydrologic processes and interactions.

The synthesis section was very condensed and lacked much detail. Specifically in section 2.7.2.2 "Models and prediction", there is a list of proposed modeling work, but I had some trouble getting a clear picture of how the modeling efforts would be integrated the core research work. (I did see some discussion of using PnET within an earlier section.) There will likely be some important insights gained from work on soil C and N, sensitivity of ET to climate drivers, and tree demographic change that will improve modeling efforts, but that may be a broader outcome and beyond the scope of this LTER proposal. I did, however, appreciate the proposal's acknowledgement of the importance of data science (2.7.2.3) and data products in general.

The core concepts and theoretical foundations of the proposed research are not necessarily novel (one might argue that the "control points of change" concept is a variation on ecological tipping points). However, because the combinations of conditions represented by the Hubbard Brook study are novel, the approach to examine interactive effects among drivers of change will lead to transformative science. Specifically, some of the unexpected observations or ecological "surprises", such as downslope migration of high-elevation tree species, dramatic increases in ET, and declining N export, run contrary to previous predictions and will require accounting for interactions among spatially and temporally varying drivers. The continued work to improve our understanding of nutrient dynamics is another section that will test and advance theory. Directly addressing evaluation criteria, the proposed "control points" framework is a very useful approach that is based on observations and processes and helps to improve our predictive understanding across both spatial and temporal scales. Thus, the highlights of creativity and originality in this proposal are that the Hubbard Brook study will continue to be at the forefront of interdisciplinary ecological science.

The team is extremely well-qualified to conduct the proposed activities. The proposed research spans multiple disciplines and will require strong interdisciplinary collaboration and the team seems well suited to accomplish this. The team is comprised of a combination of researchers with substantial previous experience with Hubbard Brook as well as a good proportion of earlier-career researchers (by my count, out of the 45 listed co-PIs, four received their degree within the past 5 years and a total of 13 within 10 years).

The resources for the Hubbard Brook study are excellent and well suited to accomplish the proposed work. Resources for this project include those from the Hubbard Brook Foundation, Cary Institute, US Forest Service, and university resources at co-PI institutions. Several of the proposed activities involve extending experiments started in previous funding cycles and/or that leverage other funding sources. Based on my interpretation, the proposed new experimental work primarily relies on established methods led by researchers with experience using these techniques, and the core questions did not rely on any high-risk manipulations or treatments. Overall, the structure and composition of the team, combined with the administrative and technical resources, are expected to meet the research objectives.

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

The proposal takes a thoughtful approach in translating the core scientific objectives into broader impacts. Similar to my assessment of the intellectual merit, HBR has established a strong framework for translating research into broader impacts and this proposal builds upon this foundation. From an ecological perspective, perhaps the strongest broader impact will be improving our understanding of how interactions among legacies of disturbance and change will affect northern forests. This knowledge is critical for land managers and decision makers. As the proposal notes, there is a need not only to develop but also share this knowledge, and plans describe how to engage key stakeholders. LTER sites provide excellent opportunities for education and training, and the proposal also highlights a continuing commitment to this. I saw a significant administrative and financial commitment to DEI efforts and the incorporation of critical ecology seems like an innovative approach with the potential for interesting and important outcomes. The broader impacts section was short, but concise and I found it compelling with no notable weaknesses.

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

N/A

Summary Statement

HBR has a history of transformative research and the current proposal build upon this legacy to address remaining knowledge gaps, as well as respond to emergent new questions. This proposal outlines a coherent and thoughtful framework to address how interactions among disturbance effects are expected to affect ecosystem processes moving into the future. HBR has a strong, interdisciplinary team focused on scientific discovery, as well as broader impacts.

Proposal Review 4 : 2224545

Back to Proposal

Agency Name:	National Science Foundation
Agency Tracking Number:	**2224545**
Organization:	
NSF Program:	LONG TERM ECOLOGICAL RESEARCH
PI/PD:	Groffman, Peter
Application Title:	LTER: Long Term Ecological Research at the Hubbard Brook Experimental Forest
Rating:	Excellent

Review

Summary

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

The proposal developed for the Hubbard Brook LTER renewal provides a useful research framework to deal with multiple scales of ecological interactions. The overriding themes of drivers include climate, atmospheric chemistry, and biotic changes affecting the ecological dynamics of the forested watersheds of the region. Though it understood that each of these drivers of change act across the various watersheds and ecosystems, the proposal attempts to clarify how each contributes to changes in the watersheds. Further research would be beneficial to identify what synergies and interactions between the drivers can accelerate change in ecosystem and hydrological system dynamics. Synthesis question 2 recognizes the potential interactions, however the proposal does indicate how the studies will address these potential interactive effects.

The proposal deals with each of the drivers as independent variables and provides studies to look at particular ecosystem, hydrological, or biotic effects. These studies will be useful to understand aspects of the system response, but begs the question of how the integrated effects of global environmental change influence and determine the system response of the impacted ecosystems of the biogeophysical template.

The studies of “control points” and cascading impacts are critical areas of research dealing with global environmental changes that manifest themselves over decades of ecological change. This understanding to determine the role of stress and release on ecosystem resiliency and tipping points are salient research questions in ecological response studies.

The potential for model validation/verification studies is quite high and some of that is required for the work to proceed. More intensive process studies with models utilized here to cross validate different model components in way that validates process linkages and representation in the various models will be useful.

The proposal would be more easily followed if a table or matrix figure could be provided that better links research question/activity in the various parts to specific watershed or ecosystem type a study is proposed. Such as table or matrix would allow a reviewer to better understand where in the biogeophysical template a particular study will take place. In addition, this would help identify where synergistic or synthetic questions could be addressed that cover the 3 interlinked drivers of change.

The proposing team seems to be productive and capable of working together to produce high quality research products.

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

The proposal provides a platform to engage with students from a variety of demographic groups, including Native American communities. These engagement activities will highlight, not only the complexity of global change effects, but also provide context for how ecological systems are critical for human well being and provide critical ecosystem services to communities surrounding the study area.

The research also contributes to the theory to practice exchange and creates a co-production partnership that can inform research and management directions equally.

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

Summary Statement

Overall, the quality of research proposed seems to very high and the research team to be more than adequate to conduct the breadth of research being proposed.

The questions related to control points and cascading effects are important ecological questions that long-term research efforts such as this can provide greater insights into due the comprehensive set of studies and observations available.

Proposal Review 5 : 2224545

Back to Proposal

Agency Name:	National Science Foundation
Agency Tracking Number:	**2224545**
Organization:	
NSF Program:	LONG TERM ECOLOGICAL RESEARCH
PI/PD:	Groffman, Peter
Application Title:	LTER: Long Term Ecological Research at the Hubbard Brook Experimental Forest
Rating:	Very Good

Review

Summary

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

Strengths (likely positive impacts)

Hubbard Brook is a leader in long-term ecosystems research, and this is a strong proposal. I felt that the acknowledgement of "surprising" results really highlights the thoughtful approach HBR is taking, and its openness to new ideas and avenues of exploration in ecology. So, even though HBR is a long-lived LTER site, I don't think its research is becoming staid, and there are many avenues left to explore. HBR is also making excellent use of the long-term manipulations and measurements made by the USFS since the 1950s. This seems to be a very productive partnership.

The control points concept is interesting, and seems like an fine way to reframe and build on concepts of ecosystem sensitivity and "hot moments", as well as to leverage the variety in the biogeophysical template and disturbance patchwork that HBR has. I'm also very intrigued by the increase in ET and proposed research seems to well planned to focus on this. HBR also has numerous long-running experiments addressing critical areas of ecology that will be continued and expanded in this proposal.

Information management is very strong at this site, and the IM's experience and commitment shows in the very thoughtful Data Management Plan. There are are a few strengths to mention:

Many of the long-term datasets, particularly streamflow and stream chemistry datasets collected in coordination with the USFS, are up-to-date and well documented with rich metadata. There are also some nice ways to access those outside of EDI (though I'll admit I found https://hbwater.org/ clunky). One suggestion I would make here is to link these extra data access apps back to EDI when possible so that people can continue citing the data.

HBR is doing a good job connecting data to publications. This paper https://doi.org/10.1002/hyp.14300 is a recent example that does a great job of including long-term datasets in the "Data Availability Statement" and the Reference list. This is always a challenge, and it suggests that there is good coordination between IM and researchers. I found other good examples of this in recent papers.

The sample archive that HBR maintains is a valuable component of the HBR program. I'm sure there are some questions about long-term stability and usefulness of things like water samples, but just the fact that HBR has invested in collections-based research this way is a very nice example for the LTER network.

Weaknesses (and suggestions on how to remedy)

There were also a few weaknesses in data-oriented parts of the proposal and the HBR program.

The requested data availability summary in "Results from Prior Support" of the proposal is really just pointing to the required supplementary table. I will give credit that published datasets (such as the bioacoustic dataset) are mentioned once or twice in the proposal, but this oversight did reinforce a minor concern that investigators are not yet thinking of datasets as valuable HBR research products to report on.

Related to this, I have some moderate concerns that PI's might not be taking enough responsibility for publishing data in a timely manner. There are certainly many good examples of up-to-date HBR monitoring datasets being published regularly, particularly from the USFS side of things. Important anchoring measurements like the EC tower are also relatively current. But, HBR has implemented some really masterful long-term experiments over the past couple of decades, and is building on them in this proposal, and some of these datasets appear out of date. Data from the CCASE experiments, for example, do not look very up-to-date in the data catalog. My understanding is that this has been running for about 10 years, and there are several publications already, so there should be a stronger commitment from the researchers towards publishing the data on a frequent basis. I'll echo this for other long-running experimental datasets.

I also noticed a long-term dataset that was embargoed on EDI (data download not allowed, only metadata visible). The MELNHE soil respiration dataset collected since 2008 (https://doi.org/10.6073/pasta/eb37cd72ccaa3e9197c461f0c1c734eb) is embargoed even though even the EDI metadata show that some of the data have already been published in a 2016 journal article (https://doi.org/10.1007/s10533-015-0172-6) that does acknowledge the HBR LTER grant. I would consider this contrary to the LTER data access policy, so if there are other similar embargoed datasets please reconsider that.

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

Strengths (likely positive impacts)

* Broader impacts at the site are very well thought out and seem impactful. In fact, they are quite exhaustive, and the links in to resource management and policy are quite laudable. I think the Critical Ecology effort will be a good way to support the already thoughtful DEI+A activity at the site. Committing dollar funding amounts for the elements of these broader impact initiatives is a positive sign that they are not just passing ideas.

Weaknesses (and suggestions on how to remedy)

* I had initially thought that the Critical Ecology program was something that would guide research questions and activities at HBR within this funding cycle. I was a little disappointed to realize that it is mostly going to manifest as a symposium series, with somewhat unclear links to the scientists working at HBR. I hope it inspires some research, perhaps at the cross-site synthesis level in how power and privilege drive ecological disturbance.

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

Summary Statement

I recommend funding this proposal. Hubbard Brook has been a really excellent and influential ecology program since its inception, and this proposal doesn't change that. There are several valuable long-term experiments that are being extended and added to here. The addition of the control points framework is good, and I think the broader impacts of the site are excellent.

Proposal Review 6 : 2224545

Back to Proposal

Agency Name:	National Science Foundation
Agency Tracking Number:	**2224545**
Organization:	
NSF Program:	LONG TERM ECOLOGICAL RESEARCH
PI/PD:	Groffman, Peter
Application Title:	LTER: Long Term Ecological Research at the Hubbard Brook Experimental Forest
Rating:	Very Good

Review

Summary

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

Strengths
The research plan is generally well organized, overall.
The proposed research leverages historical insights and data streams to identify new questions for investigation that will provide crucial insights to forest ecosystem change.
The research team is very well qualified and possesses adequate resources to carry out the research.

Weaknesses
The control points focus could be better integrated with the three disturbance themes, and described in greater detail. This component seems to be focused largely on soil water and related biogeochemical processes, but I was imagining control points to encompass other aspects of the ecosystem.
More explicit links between the proposed thematic research and the four integrative questions would be useful. Some of these integrative questions align very closely with the thematic questions, while others do not. This combined with the fact that the integrative questions are included in the introduction of the research plan had me wondering exactly where these fit in. They're repeated again towards the end of the research plan, and made more sense to me there, but still could have been linked better with the core research themes.
The research plan lists a host of guiding theories in the introduction, but few of these are meaningfully invoked in the details of the plan.

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

Strengths
Indigenous Knowledge & Networking subcommittee shows genuine commitment to engaging local indigenous communities
Art-Science program is innovative and a real strength
The Critical Ecology Framework is intriguing. It would be nice to hear more about how indigenous stakeholders and/or scholars from underrepresented groups will be recruited/engaged/funded. But overall this seems like a positive component.
Weaknesses
Mechanisms for strengthening K-12 outreach unclear.
Undergraduate curricular expansions seem relatively small relative to the number of project PIs - will materials/modules developed be transferable/made openly available?
Having multiple mentors is a recognized best practice, at all career stages, but is especially important for postdocs. An additional mentor associated with HBR and a few more specific on the components of the IDP would be good. For example, how might a postdoc enhance their teaching skills at Cary?
The proportion of early career PIs/personnel is rather low, which suggests that greater efforts to engage/recruit new ECRs to HBR may be warranted.
Metadata guidelines in DMP lack specificity. Archiving data is relatively easy, however making it useful for others takes a bit more work and having detailed formal protocols/guidelines is important in this regard.

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

Summary Statement

Ongoing work at HBF focuses on how disturbance impacts the structure and function of forest ecosystems in the northeast US. Specific disturbance types include atmospheric chemistry, climate change, and changing biota, which serve as three themes around which the research is organized. These long running themes carry over from work/funding cycles at HBR. A new focus for this proposal is control points - these are places/times within the ecosystem that exert disproportionate influence on ecosystem processes of interest.

Within each of the three research themes there are well formulated research plans driven by a series of compelling and important research questions that are hierarchically organized in ways that illustrate how detailed process level questions will help answer overarching system-level questions. Synthesis questions, some of which stem from surprising findings at the site, provide opportunities to achieve integrative understanding of key ecosystem processes that cut across the overarching disturbance themes. These questions are compelling and important, but could be better integrated with the questions driving the research themes.

The broader impacts include a variety of important activities, with several components that focus on equity and inclusion in important ways. As I note my specific comments, there are some areas here and related to data and information management that could benefit from a bit more detail and specificity.

Overall I think this is a very nice proposal outlining a tremendous amount of work that will improve our understanding of complex process interactions that drive change in forest ecosystems. In addition, the proposal touches on all five of the core LTER research areas in rather meaningful ways. As I note in my detailed comments, there are some places where a bit more information would be helpful, and some opportunities for greater integration among the different conceptual elements of the proposed work. However these are relatively minor in scope.

Proposal Review 7 : 2224545

Back to Proposal

Agency Name:	National Science Foundation
Agency Tracking Number:	**2224545**
Organization:	
NSF Program:	LONG TERM ECOLOGICAL RESEARCH
PI/PD:	Groffman, Peter
Application Title:	LTER: Long Term Ecological Research at the Hubbard Brook Experimental Forest
Rating:	Very Good

Review

Summary

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

*Results from Prior Support:

Strengths:

HBR is able to present substantial evidence of atmospheric chemistry, climate, and biota over the past 60 years or so. They track wet deposition, stream chemistry, tree biomass, ET, elements of the green and brown food webs, and species composition of key populations. The record is impressive and supports the proposed research well in most cases. The oncoming decline of ash trees is a sad but anticipated biotic change they are well placed to capture, among other things. Use of bioacoustics to monitor vertebrates is exciting as well.

Weaknesses:

The Prior Results takes up ten pages of text (ignoring the first half page, which was an introduction really), some of which is wasted white space in figures. A more synthetic and concise summation of previous work would leave more space for the justification for the proposed work. In many places I felt additional methodological details would have made it easier to judge the value of the proposed work.

*Response to Previous Reviews

From the information given, the Mid-Term Review was largely supportive of the progress during the last funding cycle. The team seems to have taken to heart the suggestion from the Site Review that more be done to investigate the role of P-limitation of in ecosystem dynamics. Then ten significant papers take up more than half a page, something that could be flagged with little extra space (read this to mean I have never been sure what the ten papers really adds to the review process).

*Conceptual Framework-

Strengths:

The conceptual model presented in the proposal does an adequate job of supporting the proposed research. Key drivers are shown as well as the outcomes mitigated by an understanding of the biophysical template (which determines the abiotic/biotic mechanisms involved) and feedbacks from outcomes to the template. Changes over time are depicted as well.

The approach is to define “surprises” from the latest round of funding that require additional long-term data to address, a strategy HBR has utilized effectively in the past. For the current research they will focus on “points of control” in the biophysical template where there is mechanistic “traction” to understand landscape processes (though reference to other key theories is made throughout). They will also employ a “critical ecology” approach to Broader Impacts.

Weaknesses:

There is a vagueness to the conceptual approach that leaves the feeling that you could use it to justify just about anything under the sun. To be sure, the drivers are site-specific and the outcomes are broader than some LTERs are able to cover, but the biophysical template is very general. What is it about the soils or underlying bedrock that make the ecosystem particularly responsive to acid rain (or not), what is the nature of species or climate that make them particularly responsive to climate change (or not), etc. While I sometimes feel that too much is made of the conceptual framework for LTERs, this limitation would have been easy to address.

*L-T Data, Experiments & Modeling, Related research & Synthesis

Strengths:

The pioneering watershed studies initiated in the 1960s makes HBR an iconic site for ecosystem studies and points to the value of ecological research for driving policy decisions (i.e., the need to reduce acid rain). The Ca-addition studies have also been key, as they show the ecosystem response when the major impacts of acid rain are mitigated. Of the five “surprises” listed in the proposal, changes in ET and N oligotrophication are the most interesting and backed up by prior results discussion.

The increase in ET is likely to be related directly to climate warming, which has resulted in longer growing seasons. How this exactly sorts out will have profound implications for the future of these forests.

N oligotrophication is also consequential for understanding the future of northern forested ecosystems, if not the glove. A recent article in Science showed that carbon fertilization, apparently, is causing declines in available N worldwide. HBR looks like a good site to investigate the full implications of this

The importance of long-term research is that you are in the right place all the time, not just some of the time. Invasion of insect pest, such as emerald ash borer is a good example of this, as well as the wooly adelgid. Being able to describe the impacts of invasive pests is another great benefit of this LTER.

I like the concept of control points and it is likely to serve the team well as they continue to synthesize their results, but I raise some concerns below.

The immense breadth of platform for monitoring forests, soils, and streams is a major strength of the project and quite impressive for what is has and can reveal about the responses to changes in atmospheric chemistry (Section 2.2.1). I especially found interesting the approach to soil C dynamics and storage (Section 2.2.2) and for the focus on changing C fractions especially. This will likely reap important benefits in understanding control on the C cycle.

The finding that the northern forest represented by HBR may be co-limited by both N and P (Section 2.2.4) is a challenge to the prevailing theory that these forests should be N-limited only. The continuation of MELNHE is key to addressing this idea.

I found myself wondering if increased ET on a regional basis could lead to increased rainfall at HBR via recycling of rainfall from forests to the west. Is there any evidence this might be going on?

For changing ecosystem seasonality, HBR will continue the CCASE project looking at soil warming and reduced snowpack on critical soil processes. They will also continue to follow a series of plots along the elevation gradient, incorporating a site with high resolution sensor measurements.

Weaknesses:

Of the five surprises, the first (shifting tree distributions) did not appear in Prior Results (though it was discussed later in the proposal in Section 2.3.3), the decline in abundances of many species is not a surprise as it has been evident since the 1970s, and how “early warning signals” are measured and how it affects ecosystem resilience was not made clear. A reference to the topic did not address the issue.

A good discussion of the importance of disproportionality in relation to controls points was followed by a laundry list of other theoretical concepts. While it is not a problem that other theories have been used to underpin the project in the past, for me, it created an unnecessary theoretical muddle that shows up elsewhere in the proposal.

I was surprised that more could not be indicated as to the cause of the increase in ET. It has been ongoing for five years at least and the data to distinguish the three competing hypotheses has been in hand over this time period. Some sort of preliminary indication of the answer(s) would have been helpful.

HBR will continue to monitor key biota including trees, insects, birds, bats, and salamanders as well as other wildlife via motion detecting cameras (section 2.4). On the one hand, this seems wonderfully ambitious and on the other perhaps somewhat ad hoc. For example, the proposed salamander work fits the theme of climate change well but seems added on to show “we work on evolution, too.”

The issue of control points is a good one for the HBR team to pursue. It does relate theoretically to much of what has been developed earlier (e.g., mosaic theory). As applied to HBR, it wasn’t clear to me that they were considering control “points” as much as control settings. They have essentially divided watersheds into three portions in relation to the depth of bedrock, water table, stream water permanence, lateral flow, etc. When I think of control “points” I think of, for example, riparian forests and their importance in denitrification, the size of the area outweighed by its influence on a key ecosystem process. Thus, is this a good test of control point theory? It will undoubtedly be enlightening even if it is not a critical test of the theory. As aside, this is an example of where extra space afforded by a shorter Prior Results section would have been useful. In one throwaway line they say they will follow the quantitative approach of Bernhardt et al. (2017). I would have liked to see examples of how this quantitative approach will be used and in what way it would reveal which of these settings or points disproportionately affect critical ecosystem processes.

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

Strengths:

HBR demonstrates an impressive array of BI including liaison with local resource manager, tribal outreach, several apparently successful science education programs, an art-science program, and a robust DEI&A program. Each of these activities are worthwhile to continue. Working with the Critical Ecology Lab (CEL), they propose to address the gap between biogeochemical and ecological sciences and that of societal ideologies and value systems. This a new effort led by an HBR alum that includes earth scientists identifying as female, nonbinary, queer, trans, Black, Asian, and Indigenous. This will be explored via workshops and theme-based symposia.

Weaknesses:

I frankly had a difficult time grasping how the Critical Ecology approach will lead to any conceptual breakthroughs. Not that it can’t and maybe this is another example where some added text would have made the ideas more accessible. The engagement of the diversity represented by CEL and the interactions with the broader HBR community vis-à-vis the creation of safe, comfortable, and supportive atmosphere will likely be the greatest benefit of this activity.

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

PROJECT MANAGEMENT

Strengths:

The HBR consists of some 70 collaborating scientists, the Committee of Scientists, led by a Scientific Coordinating Committee (SCC) with nine

members. Functioning as the executive, the SCC includes assigned members (4) and five elected members. One nonvoting member is external. The COS meets quarterly, often at the Cary Institute in fall, winter, and spring and at HBR in the summer. An annual Hubbard Brook Cooperators meeting provides an opportunity for outreach to USFS land managers, non-HBR scientists, and NGOs. Leadership changes have been carefully considered and managed through time. Having two co-lead PIs is a good model for project direction.

The site is managed in collaboration with USFS, who maintain buildings, roads, weirs, etc. A Research Approval Committee evaluates proposals for new research at the site. A comprehensive procedure to handle complaints, including potential harassment issues.

Weaknesses:

The structure of the SCC seems exceedingly complex to me, but this is often the nature of democratic institutions.

INFORMATION MANAGEMENT

The IM purpose, process, and evolution is thoroughly described, much in the context of the data cycle. HBR, like many LTERs, takes advantage of EDI as a data repository. EDI is transforming the way we do IM. Sufficient resources appear to be in place to support this critical function. The sample archive at HBR is legendary and continues to generate new analyses and publications.

Summary Statement

HBR is emblematic LTER site for its pioneering work on watershed studies that continue to reveal how northern forested ecosystems respond to changing atmospheric chemistry. Recent efforts to account for the effects of a warming climate interacting with a changing biota continue to reveal important results that challenge conventional wisdom and test ecosystem theory. Robust outreach and education programs are being combined with concerted efforts to create a supportive social environment for underrepresented groups is coupled with a well-considered management plan and strong IM. The work is guided by a reasonable conceptual framework and a new theoretical focus on “control points.” Both the framework and discussion of theory could have been presented in a more specific way, on the one hand, and in a streamlined form on the other. The jumble of ideas sometimes competing for center stage served as a distraction from an otherwise stellar set of long-term studies. Better balance between the text devoted to Prior Results and details supporting the proposed activities would have been an additional improvement.