Proposal Status | MAIN

Organization: University of Florida

Review #5

Proposal Number:	1754741
NSF Program:	POP & COMMUNITY ECOL PROG
Principal Investigator:	Bruna, Emilio M
Proposal Title:	SG: Are there synergistic effects of habitat fragmentation and drought on tropical plant demography?
Rating:	Good

REVIEW:

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

What is the potential for the proposed activity to advance knowledge and understanding within its own field or across different fields (Intellectual Merit)

The proponents intend to test the 'Fragmentation-Drought Hypothesis' (FDH) ù that the negative impacts of a key environmental perturbation (drought) are more pronounced in tropical forest fragments. This hypothesis has received increasing attention in recent years, and there is already a solid body of evidence to support the idea that fragments are less robust to various kinds of perturbations than continuous forest. However, this evidence basis is currently somewhat piecemeal and we lack a detailed understanding of the demographic mechanisms mediating the interaction.

The proponents will test the hypothesis using a unique, long-term demographic study of the understory herb Heliconia acuminata. They are only planning to work with this one species, but the they argue (persuasively I think) that H. acuminata represents a reasonable æarchetype' for functionally similar species. They have amassed a large multi-population, multi-year demographic dataset, which critically, includes populations from experimental fragments and continuous forest. They also continue to accumulate a large amount of fine-grained environmental data. These data offer a unique opportunity to not only test the FDH, but also drill down into the mechanistic basis of drought-fragmentation synergies. I am now aware of a similar system where this kind of project could be undertaken.

The work plan will address three questions: (1) Do droughts decrease post-seedling growth and survivorship? (2) How do fragmentation and drought interact to influence reproduction, seedling establishment, and seedling survivorship? (3) Does variation in demographic vital rates interact to increase the probability of extinction in fragments relative to that in continuous forest? The first two are essentially statistical modelling exercises, while the third will be addressed by taking insights derived from #1 and #2 to construct and analyse a demographic model (an IPM). While on balance, I think the proposed project will yield a valuable test of the Fragmentation-Drought Hypothesis, the three questions are not as well-integrated as I was expecting and at times there was a touch of æsmoke and mirrors' about the proposed work. Specific concerns are:

1) I understand that the EEMD approach offers a way to unpick lagged effects, but if the ultimate goal is to parameterise an IPM, then why not go for a more traditional parametric model with lagged predictors, or something like a functional linear model. Both of these are a natural fit to the IPM framework, whereas the

EEMD is rather descriptive. It's not clear to me why such very different methodological approaches are necessary to tackle questions #1 and #2. Reproduction may well be subject to delayed environmental impacts, so could well be another good candidate for the EEMD approach.

2) I found the phrase "a flexible Bayesian frameworkà' (used to describe the approach to #2) to be completely opaque. The structural assumptions of the planned modelling approach surely matter more that the method of inference used? It sounds like the proponents are intending to fit hierarchical linear models of some kind to describe reproduction and recruitment, but I can't be sure. There's also no mention of how these models will be fitted ù the mention of wAIC suggests Stan? Finally, the Spiegelhalter is not appropriate here. This again gives the impression of smoke and mirrors.

3) I like the idea of using an IPM / LTRE framework to drive a PVA and decompose the underlying causes of differences due to drought and fragmentation. However, the proposed approach seems rather convoluted. Rather than trying to match estimated kernels to the simulated environment via the SPI, why not simply model the demographic rates as a function of SPI and drive the IPM that way? Rees and Ellner introduced a regression-based LTRE that would be very well suited to this kind of model, and it would avoid the need to 'select the IPM for the year whose SPI value is the closest match [to the simulated environment]'.

4) I am worried by the statement that '[if] there are multi-year droughts effects on growth, survival, or reproduction, we will build temporal autocorrelation in vital rates into the simulations.'Carryover or accumulated effects will generate autocorrelation in vital rates, but this is not the same as living in an autocorrelated environment. If multi-year effects of drought are found, they would be better captured as time-delayed effects. Again, this could be accommodated if the demographic rates were modelled as direct functions of the SPI.

Finally, as far as I can see, the requested resources are reasonable and will enable the research to be conducted as described.

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

The proponents have a very good track record of promoting teaching, training, and learning. Much of this has been facilitated by work in the system used here. The wider project has already generated numerous papers, along with some excellent datasets. I see no reason why this won't carry on. There's the usual outreach / education strand to the project that we're all forced to undertake these days. The proponents seem to take this seriously.

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

N/A

Summary Statement

Overall, I *am* enthusiastic about the proposal. I've raised various concerns about the methodology, but none of these are intended to be condemnatory. It's always hard to sketch out what an IPM analysis will look like until the statistical modelling is done. The team are very good and have a reliable track record, both in terms of working together, and with respect to the study system. I found that the proposal is (mostly) well-written and it tests a very timely hypothesis using state-of-the-art methods. I suspect that it will yield a slew of nice papers.

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