August 2017

Response to reviewers’ second round of comments:

REVIEWER 1 COMMENT:

July 8, 2017

Review of Gawel et al. (RSOS-170151.R1): *Contrasting ecological roles of non-native*

*ungulates in a novel ecoystem*

I reviewed the first version of this manuscript several months ago. In that review, I found

the article to be well-written and concise, to address an important question (role of nonnative

ungulates as seed dispersers vs. herbivores in novel tropical ecosystems), and to largely be

sound. I had suggestions for revisions that centered around: (a) lack of information on study sites

(vegetation and soil characteristics); (b) interpretation of results (in a native vs. nonnative

context, instead of a novel ecosystem context); and (c) over interpretation of results (primarily of

feral pigs playing a beneficial role vs. not having a negative impact).

In general, I feel like the authors did a good job of addressing these concerns and suggested

revisions. I still feel like too much importance is placed on native vs. nonnative in the

Discussion, but the authors do eventually provide text and discussion placing the results in a

novel ecosystem context.

The one area where I feel the authors still need to improve is on details of the study site,

specifically information on vegetation composition and importance in the 8 study sites (fenced

vs. unfenced). I originally wrote:

*The article is concise, and I appreciate that. However, I feel like there is some pretty important*

*information missing, largely from the Methods (although at 4x the length of the Introduction, I*

*found the Methods section to already be long compared to other sections). First, I feel like the*

*authors need to provide a fair bit more information on the species composition of the “novel*

*ecosystem” within which they are working. For example, it would be very informative to know*

*the average densities and importance (e.g., via basal area) of the dominant species, both the*

*native and the nonnative components of the overall community. Also, what proportion of the*

*forest do the selected study species make up? As currently written there is a lot of attention on*

*this being a novel ecosystem, but zero information to support that and zero information to*

*support the importance of the selected study species. In addition to information on the overall*

*study site species composition, I feel that the authors need to do the same for the small fenced*

*exclosures they studied. As written, they simply say on lines 79-81 that the fenced and unfenced*

*plots “had similar canopy cover, rockiness, and forest structure”, but provide no data to support*

*this. A table of the dominant species with densities and some estimate of importance (e.g., basal*

*area, biomass, etc.) is warranted at a minimum, but it should also be pretty easy to run some*

*analyses to see just how similar they were (t-test of species composition in paired fenced vs.*

*unfenced sites?). Second, the authors should provide a lot more information on the soils in the*

*study site, particularly to help couch the results about feral pigs. It strikes me that these are very*

*unique soils (karst; “calcareous rock – the brittle, fossilized remains of ancient marine*

*organisms”), and the primary way in which feral pigs impact other ecosystems is via rooting and*

*wallowing. If they are unable to root in these soils, it likely has a huge impact on the results seen*

*(and potential comparisons to other studies). Finally, the authors provide no information on*

*ungulate densities for deer and pigs in the study area. It is very difficult to interpret the results*

*(and compare to other studies) without this information. Also for feral pigs, what are the*

*animals on Guam descendant from (e.g., are they true feral pigs that escaped from*

*domestication, wild boar introduced, or something else?). All three of these items are basic*

*aspects that you would expect to find in the Methods section of any scientific paper, such that the*

*absence from this one is quite striking.*

I feel that the authors did an adequate job addressing lack of information on soils (although soil

taxonomy for the study sites, if it exists (?), remains conspicuously lacking) and ungulate

densities (although I encourage the authors to include the information (and caveats) on ungulate

density estimates from the one study in Guam that estimated deer and pig densities that was

included in the response to reviewers, but not the actual manuscript). In contrast, I feel the

authors still do not provide adequate information on vegetation composition in the study plots.

So I repeat that I feel that the authors need to provide more information on the vegetation of *the*

*small fenced exclosures they studied. As written, they simply say on lines* 94-96 *that the fenced*

*and unfenced plots “had similar canopy cover, rockiness, and forest structure”, but provide no*

*data to support this* (i.e., the same text provided in the original version, still with no data to

support it)*. A table of the dominant species with densities and some estimate of importance (e.g.,*

*basal area, biomass, etc.) is warranted at a minimum, but it should also be pretty easy to run*

*some analyses to see just how similar they were (t-test of species composition in paired fenced vs. unfenced sites?).* I feel this needs to be done to be able to interpret these data.

END REVIEWER 1 COMMENT

RESPONSE

*We had counted and measured adult trees at the seedling plots, and have put some more detailed information on the plots (# of adult trees, average DBH and heights of adult trees, etc…) in supplementary information. In addition, within the manuscript text, we rewrote the paragraph in the methods describing how we chose paired plots:*

*“To assess ungulate effects on seedling survival, we set up paired plots in eight selected karst forest sites in northern Guam. At each site, we erected a 1.8-m tall chicken-wire fence around one plot, and left the adjacent plot unfenced, allowing ungulate access. Each seedling plot covered an area of about 3.5 m x 5.5 m. The fenced and unfenced plots were placed so that individual pairs had similar canopy cover, rockiness, and adult tree composition and density. Very little differed between them in topography and ground cover. While species composition of adult trees already present was almost impossible to match exactly, species composition often overlapped, and we found no significant differences between numbers of adult trees, average diameter at breast height, and average height of adult trees between paired plots.* *All of our sites had over 70% canopy cover, and we avoided gaps, depressions in the substrate, or any other features that might have caused a difference between the paired plots outside of our treatments.”*

*To address concerns about lack of information on soil types, we added this to the Study Area subsection of our Methods:*

*“While a variety of karst types exist in northern Guam, our seedling plot and transect sites all occurred on reef facies and detrital facies of what is classified as Mariana Limestone – that is, Plio-Pleistocene reef and lagoon that comprises 75% of Guam’s karst formations [33].”*

REVIEWER 2 COMMENT

Dear authors,  
Thank you for your answers to our questions. After a second evaluation of your paper, I still think that there are major revisions to bring to it.  
Please see my suggestions, comments and questions below.  
Table 1 and model results  
The authors should really present the complete results testing species, time and treatment in a single model and then provide the model retained after the selection process, with the weight and AICc of each model and of the model retained.  
The treatment effect for each species is already presented in figure2.  
Full model : Species+Treatment+Species:Treatment+time+time:Treatment (to test for the different time length of exposure to ungulates) and not the other interactions time:species as it is not fully crossed, and same for the 3rd order interaction.

END COMMENT

RESPONSE

*Thank you. We appreciate your sharp eye and attention to detail, you have caught some very important issues that needed to be corrected and clarified. We have changed Table 1 to reflect models tested rather than focus on species (that information in encompassed in Figure 2).*

REVIEWER 2 COMMENT  
Table 2  
Pb average seedlings/scat is not an accurate measure, the frequency of presence is a more relevant measure or the average seedling/scat should be assessed only when the species has been consumed (within presence).  
Please reshape this table with frequency of occurrence, recalculated average seedling (to be linked to the number of seeds produced per fruit of each species).

END REVIEWER COMMENT

RESPONSE

*We have revised Table 2.*

REVIEWER 2 COMMENT  
L 39Elaphus not elaphas.

END REVIEWER COMMENT

RESPONSE

*Changed.*

REVIEWER 2 COMMENT  
L88-89 Please clarify and rephrase, I do not understand …and especially the absence of scat amongst ????

END REVIEWER COMMENT

RESPONSE

*Thank you, that was definitely confusing, we are glad you caught that! It has been revised to state “…the absence of birds…” instead of “scat.”*

REVIEWER 2 COMMENT  
L122-124 if this is the case, then time will not be significant in the final model. So I do not understand why you put these results in the mat and method section

END REVIEWER COMMENT

RESPONSE

*We removed that line and this information is presented in the Results.*

REVIEWER 2 COMMENT  
L159 among instead of between

END REVIEWER COMMENT

RESPONSE

*Corrected.*

REVIEWER 2 COMMENT  
L 189 P. mariannensis not mariana

END REVIEWER COMMENT

RESPONSE

*Corrected – the species is Psychotria mariana. Not sure how that got mixed up, thank you for pointing it out.*

REVIEWER 2 COMMENT

L231 metrics instead of numbers

END REVEWER COMMENT

RESPONSE

*Corrected.*

REVIEWER 2 COMMENT  
Figure 1. is OK  
Figure 2. is OK put the \* on top of the bars, and ns for the last two species  
If there is a species effect, please show it on the figure with letters (a, b, c …) species sharing the same letter are not different …

END REVIEWER COMMENT

RESPONSE

*Figure has been changed as recommended.*

REVIWER 2 COMMENT  
Figure 3.  
The data in that figure are not accurate, because the proportional abundances (as they are defined in the material and methods) of the different species dispersed by wild pigs do not sum to 1 (it is more than 1) and for deer, there is a similar problem (the sum is less than 1)  
For the species present in the local flora however (left panel), the data presented seem to be OK.

END REVIEWER COMMENT

RESPONSE

*Thank you very much for pointing this discrepancy out. We made an error in describing the way that the values in that panel (scat) were calculated. We revised in the text to state:*

*“We used a similar approach to calculate the proportional abundance of seedling species found in pig and deer scats: the total number of scats that has a given species of seedling germinating from it, divided by the total number of either deer or pig scats that we had collected. For example, Carica papaya seeds germinated in 16 out of 31 pigs scats. Therefore, the proportional abundance was 16 divided by 31, or 0.52. Because multiple species occurred in some single scats, the values for proportional abundance in scats do not necessarily add up to 1.” (Lines XXX – XXX)*

REVIEWER COMMENT

Figure 4.  
Concerning defecation rate, please check Picard et al. 2015 (<http://onlinelibrary.wiley.com/doi/10.1002/ece3.1512/abstract> ), where you will see that your statement does not hold as defecation rate is not so different among animal species (for instance between red deer and wild boar).  
The density of scats for pigs is very small ( from 0 up to 4 per 800m² and an outlier with 10 scats per 800m²) and variable at all. Putting pig and deer on different X scales lead to ambiguous reading of the results.  
I would erase that figure or present it with two boxplots: with and without pig scat. In no manner you can use these results to determine that wild boar has or has no effect on the vegetation.  
As I pointed out in my first review I would suggest using linear regressions with both types of scats to see if wild boar can significantly explain something once deer has been taken into account.  
The fact that boar do not wallow on limestone karst forest and are not browsing animals just mean that you can not really assess their direct role in this kind of forest.  
Then I would conclude that the comparison you do between deer and pigs role is not really relevant.

END REVIEWER COMMENT

*There were only three out of 14 sites that had no pig scats. They spanned our y-axis for all forest characteristics, with no pig scats characteristic of low, medium, and high abundances of all types of seedling abundances. Therefore, we do not think that a boxplot would provide more insight into relationships between pig scat abundance and forest characteristics.*

*We are unsure why boar and deer defecation rates are similar in captivity, according to Picard et al. 2015. This may be because they were fed identical diets. Studies focused on wild boar and multiple species of deer consistently report that deer have higher daily defecation rates (DDR), see list below. Therefore, we think it makes sense to keep pigs and deer on separate graphs.*

*1.) Wild boar DDR 3.8-4.3:*

*Ferretti, F., Storer, K., Coats, J. and Massei, G., 2015. Temporal and spatial patterns of defecation in wild boar. Wildlife Society Bulletin, 39(1), pp.65-69.*

*2.) Captive axis deer DDR: >13*

*Dinerstein, E. and Dublin, H.T., 1982. Daily defecation rate of captive axis deer. The Journal of Wildlife Management, 46(3), pp.833-835.*

*3.) White-tailed deer DDR: 34 in wild, 11-14 in captivity*

*Rogers, L.L., 1987. Seasonal changes in defecation rates of free-ranging white-tailed deer. The Journal of Wildlife Management, pp.330-333.*

*4.) White- tailed deer, (wild females) DDR: 26.9*

*Sawyer, T.G., Marchinton, R.L. and Lentz, W.M., 1990. Defecation rates of female white-tailed deer in Georgia. Wildlife Society Bulletin (1973-2006), 18(1), pp.16-18.*

*5.) Wild boar DDR: 4.2-5.8*

*Plhal, R., Kamler, J. & Homolka, M. Acta Theriol .2014. Faecal pellet group counting as a promising method of wild boar population density estimation. Acta Theriologica. 59: 561. https://doi.org/10.1007/s13364-014-0194-9*

*6.) Mule deer: 13-15*

*Smith, A.D., 1964. Defecation rates of mule deer. The Journal of Wildlife Management, pp.435-444.*