

## Sarracenia – Comments DRF 2019

In general, plain type represent text from the manuscript, whereas italics are comments.

*I'd start with a comment that I believe I shared on reviewing Crossing the River; based on the publishing location and the title I did not expect to find this book to be interesting or relevant to me. Mindful, however of my experience in Reading River, I accepted the invitation to read it based on the hope that I would be pleasantly surprised a second time. I was. To me that sends a message: no one outside very narrow disciplines will read this or likely even hear about it, unless you work very hard at that (which AME is good at). But, how many people at a place like the Harvard Forest even knows that Princeton Monographs still exists? I used to pore through them, but over the past few decades assumed they were defunct. How many people will want to read "Scaling Sarracenia". You need a marketing firm.*

*Nonetheless, I always thought it must feel cool to have published one of these. I hope that you enjoy that experience.*

*But, who is your audience? Do you have one in mind? It is not clear, especially after reading through it as the material varies from generally accessible to interested natural historians like myself, to basic like the introductory ecology text dealing with Succession, Food Web Theory, Ecosystem Function, etc., to detailed and complex in the model and statistical sections. Or, is this largely a satisfyingly exercise for the two of you? A complete synthesis to round off the decades of work?*

*I ask because some of your writing, figures and approach assumes that the readers come from your food-web zoological community ecology background, which was originally unfamiliar to me as a botanist and plant ecologist, but your detailed botanical descriptions and terminology assume background knowledge of plants that many food web community ecologists won't have. And, then, some of your terminology is simply arcane. autapomorphic outgrowth of the ventral...*

*Inquiline – Not an accessible word.*

*I appreciate the history of your collaboration and the story of searching for and identifying Sarracenia as a model organism.*

*Recalling the work of Kathleen Donahue and googling Arabidopsis and model system for ecology suggests to me that its application goes beyond molecular biology and evolution to ecology. Same seems to go for a knowledge of its natural history: The plant is native to Europe and Asia ([Hoffmann 2002](#)), and at the same time that Elizabeth I ruled England and Tycho Brahe was documenting the comet of 1577, the physician Johannes Thal was finishing a book describing the plants of the Harz Mountains in what is now Central Germany. Thal's descriptions included a plant he named *Pilosella siliquata* ([Thal 1588](#)). Linnaeus renamed the plant, placing it in the genus *Arabis* and assigning a species name in honor of Thal, hence *Arabis thaliana* ([Linnaeus 1753](#)).*

First, most ecologists are drawn to experimental or observational work in

the field. *That was what I thought until I met Fakhri. This broad generalization and those that follow about pristine sites etc. don't seem to jive with modern Harperian population and Bazzaian physiological ecology. They resonate with me, but I was disabused of those notions in grad school, though I ignored them.*

16. Correct or not, it is [Long-Term Ecological Research](#)

*Fig. 2.1 For a non food-webber, I need to take on faith that this is what it is.*

Like Clusius, people seeing *Sarracenia* and other pitcher plants for the first time often mistake their leaves for flowers. *Really? That makes no sense to me.*

ombrotrophic (“rain-fed”) bogs, poor fens, nutrient-poor streamside wetlands, coastal sandplains, and seepage swamps.... *I'd insert pondshore wetlands, as across the Northeast Kingdom that where is see it most frequently, as in Harvard Pond, but natural sites with little human impact.*

Our generation of [food web, insect, animal? *There are lots of community ecologists and I dare say that few plant ecologists digested that study?*] community ecologists learned of *Sarracenia purpurea* through Addicott's (1974) classic experimental study

*As someone who started as a botanist and learned the biology of Sarracenia early, I would argue just the opposite of you who came from a zoological (?) background. Our field trips at Connecticut College always featured pitcher plants regardless of whether Goodwin or Niering were leading the excursion. But the basic botany and plant (aut)ecology of S. purpurea are less familiar to ecologists. We review this rich history of the plant before turning to studies of its inquiline food web.*

Finally, the very bottom of the *S. purpurea* pitcher is a smooth, gland-free zone whose function has yet to be determined. *Funny enough. I recall reading somewhere that not everything has a purpose or is interpretable through cost-benefit analysis...must drive Tom Givinish crazy*

*The early studies identifying the absorption of nutrients from the putrid masses of insects are fascinating.*

*Beautiful photo and drawing p 44-45*

*p. 54. The food web composition is described so matter of factly, when it is so bizarre.*

they are still unlikely to disperse easily between bogs separated by tens-to-hundreds of kilometers. *That seems very long for a typical distance between suitable habitats for PP. Presumably an extreme??*

it is uncertain that these mechanisms  
*But, no specific mechanism has been cited., so how can it be doubted?*

*You might explain the triffid.*

*Fascinating, like rhododendron, not a species one would expect to become invasive. Any thoughts on why? And what limits them elsewhere? Seems like such a neat opportunity to speculate.*

*The explanation of food and nutrition is refreshingly thorough and relatively basic (good).*

*Perhaps a bit too much information...e.g., the Redfield ration?*

*The memories of Tom Givinish during my first year and his year of agony after not getting tenure, and watching Fakhri get hired; sitting in adjacent offices in HUH with him explaining his approach to cost-benefit analysis and its mechanistic predictions. And, having just come from listening to Tilman and his resource ratios.*

Perhaps because of the role of evolutionary history and the potential for morphological plasticity in plant growth and development, the global spectrum of leaf traits has been less successful in explaining the placement of particular species in multivariate trait space or patterns of intraspecific trait variation in local assemblages. *After much clarity, this was not intuitive.*

Carnivorous plants, including *S. purpurea*, have solved the evolutionary problem of maximizing carbon gain while minimizing costs of nutrient acquisition. *But, this is not the immediate evolutionary challenge addressed above in the big size vs leaf size/cost functional trait discussion.* Although the outcomes are different, the same rules apply

*14 house flies! Yuck. Yeah, rotting flies.*

The fluid in these pitchers was almost completely anoxic (see also chapter 13), and  $[\text{NH}_4]$  were almost an order of magnitude higher than in the controls. *Higher? Water levels? Clarify.*

Ultimately, these physiological and demographic shifts can lead to population decline as overall death rates exceed birth rates in environments with high rates of N deposition. *But, this is extrapolated...because the experiments were only run for one year?*

*Chapter 5 is the toughest so far. And, a lot, a lot of not intuitive graphs.*

If we can successfully forecast these range shifts, we also may be able to forecast the species composition of local assemblages (Botkin et al. 2007) and the ecosystem services and functions they provide (Smith et al. 2009). *Is either of these things likely for the time frame we are discussing? Are they truly useful? What chance do we have to predict assemblages in the future? Seems like a gratuitous statement.*

*Of course N deposition in the high deposition areas is going down and is projected to drop considerably in the future. Use overall may increase it more broadly, but not in such an*

*intensive and concentrated way. N deposition is not driven by agricultural N use. Seems that the focus on N needs to be reconsidered given projected trends.*

*Temp is clearly important and showing that a species range includes a much wider range and embraces the extremes of the future doesn't negate that, though it does suggest that the species will cope. I'm not convinced that that section changed the importance of temp in distribution modeling or future thinking.*

*Given that the modeling in Chap 7 is centered on two sites it seems destined to hold less interest than previous chapters. That plus the emphasis on N, which is fading rapidly from focus.*

*The detailed analysis of this chapter is based on many years of data collection and experimentation, but it is only representative of two pixels in the landscape of the eastern US.*

*There may still be great uncertainty in experimental results, depending on which variables were manipulated and what treatment levels were used. But at least our forecasts will not be misled by a simple mapping of species occurrences on only two measured climatic variables. What is the larger message—for Sarracenia and more broadly?*

*Animals are prioritized. Not clear why that is important to point out.*

*but time is not, perhaps reflecting a deeply held view that the natural world is in a balanced equilibrium (cf. Kricher 2009, Ellison 2013). This temporal myopia of (animal) community... Conclusion drawn from a supposition. Time may be ignored because community ecologists are just noting what is there, now, with no thought whatsoever to equilibrium. I don't readily buy that equilibrium views are widely held.*

*Nice, (but rather gratuitous) inclusion of the Blake.*

*Seems to me that the animal biologists with a paleo perspective have been just as focused on succession and the ack of temporal coherence of animal assemblages as the plant folks. I'd be careful inferring that community ecologists believe in the coherence of communities.*

*It seems like Diamond got the best deal – he was able to produce a simple analysis and model and move on to equally or bigger ideas while others labored for 25 years to see whether his findings were robust. And they were, for some groups. The extended focus on testing his model is a sad commentary, but all the more credit to JD.*

*Unfortunately, I could not read the rest of the chapter. This kind of prose is tough! Compared to a randomly connected Erdős-Rényi network, small-world networks generate a fat-tailed distribution of linkage distances, which may follow an approximate power distribution with a scale-free exponent of between 2 and 3 (May 2006).*

*I seldom see or think of succession being linked to the word “community”. That does imply a mindset that perhaps you bring to this as animal ecologists. This is especially striking and inappropriate given that succession was originally coined for a process involving individual*

*species. The term “community succession” does not even come up in Google search. It is successional change in communities on p 2, which is very different.*

*Harvey’s succession was not ecological succession it was phenology. Just cite Thoreau who didn’t bring any preconceptions of community theory to his observational studies.*

*I don’t really see the purpose of once again revisiting Clements and Gleason in this text. Why not just start with Henry Thoreau and his focus of explaining the process of succession by examining the individual characteristics of species and testing that by looking at the age distribution provides a powerful approach. Then skip directly to Gleason, Harper etc. The G and C material was fully rehashed twenty-five years ago.*

*In my view the ongoing use of “community” by most ecologists doesn’t have any relationship to Clements’ views of community, or where people side in the C-G debate, but is rather the convenient term to use, an alternate to assemblage. Same is true at broader scales – it’s a convenient descriptor, not implying any coherence to the actual grouping of plants. We still map MV as oak-PP despite the fact that that actual assemblage is much less common and has no integrity.*

*The organization of species into groups that represent communities does, however, provide a useful framework that recognizes the potential importance of species interactions to generate higher-level patterns of organization (Connell and Slatyer 1977, Farnsworth et al. 2017). Perhaps for a small set of ecologists, but more generally that is reading more into the term than most people imply.*

*Note also that as the spatial grain of analysis decreases, models based on community types must collapse back to models of individual species. At a small enough spatial scale, a patch can support only a single species (or even a single individual), so the different “stages” in a community Context model could represent different individual species (Horn 1975, Diamond et al. 2016). This doesn’t really fit well into this paragraph*

*Ideally, each stage would be complete and mutually exclusive (and would include an initial “disturbed” state) and each species would be uniquely assigned to a single stage (analogous to the “relay floristics” model of succession proposed by Egler 1954 and reviewed by Drury and Nisbet 1973). Why would this be considered ideal?*

*In practice, the classification is always messier Do you mean “reality” is....?  
The classification?*

*Why go through a lengthy discussion of a modeling framework that doesn’t fit reality?*

*Community change. That is strange terminology. It is vegetation change or forest change or successional or forest dynamics. Perhaps the focus on communities is typical of animal ecologists but I don’t bump into or read it much.*

many studies of temporal trajectories have not been long enough to exhibit community change (especially for terrestrial vegetation). *But many have – not sure what the point is here* Valid replication may not be possible because of changes [changes??] in land use and climate, and many monitoring studies do not begin from the point of a well defined disturbance or an “empty” patch *but many do – this entire set of sentences is perplexing*

Instead, ecologists have relied on chronosequences: concurrent snapshot surveys of patches at presumed different stages of succession. Chronosequence data assume that the time since disturbance can be accurately dated for each patch, and that patches differ only in the time since disturbance. *It seems strange to explore something so basic and well trod as space for time substitution. Or Markov models. It is interesting history for ecologists who have grown up without them, but is it useful to retread? Like the section of Succession, this background seems elementary and well-trod elsewhere.*

Chronosequences do not always yield the same patterns as replicate observations through time of a single patch (Johnson and Miyanishi 2008). *Yikes – why such a basic truism?*

Figure 9.3: The temporal progression of the entry of species into an assemblage, followed by changes in their abundance and their eventual disappearance, form the core data for studies of ecological succession. *Interesting example that implies one specific model of succession.*

*In the end, how did all of this relate to Sarracenia? In other words, the section ends with Markov, but no explanation of how that illuminates to book subject.*

However, it is much easier to collect spatial replicates for cooccurrence analysis or to construct species-interaction networks than it is to collect temporal replicates to document succession. *Restates a sentiment expressed numerous times.*

space-for-time substitutions dominated studies of ecological change until the advent of the Long-term Ecological Research (LTER) program in 1980 (Willig and Walker 2016). *Is that true – if you plotted up all the examples for LT succession studies would they show a strong centering on LTER? I wonder if that many LTER sites study succession and whether those that do use unique LTER data sets or adopt existing ones, like we have, Andrews has, Hubbard Brook etc. etc.*

Better support for the space-for-time substitution comes from paleoecology (Blois et al. 2013) *I wouldn't agree. I would say that paleo has provided another means of examining long-term trends, so an alternative to space for time.*

We cannot yet test forecasts of forward-looking SDMs, as the future has yet to occur (Vaughan and Ormerod 2005). *Yikes!*

*Seems like too much discussion of space for time. Again, I don't view this as terribly valuable, because it is covered well elsewhere. And, it leads the text astray and becoming less well connected to the subject of the book. The connections are unclear.*

(a time-for-space substitution) *Seems bizarre to call long-term study or an examination of real data or history “time for space substitution”. It is reality not a substitution.*

also have yielded inconsistent results at vastly different temporal scales.

*How does reality become inconsistent? The results are what they are – highly variable, differing considerably, etc? But they are only inconsistent with a prediction or model they are highly consistent with reality.*

*P 281. After a lengthy diversion, brought back to Sarracenia data, is a surprise.*

Across the late Quaternary, the species identity of aggregated and segregated species pairs has been inconsistent through time ???

But the continued focus on studies of closely related species has somewhat isolated studies of succession and co-occurrence from the other major thread of community organization: food webs and networks. *This interesting point, and something that most plant community ecologists will not have focused on, is the major and most important point in the chapter. I’d greatly reduce the successional discussion and beef up this topic, which is only glanced on.*

... Three major research themes can be distinguished in part by the data structures used to organize community data. Co-occurrence analyses use a species \_ site matrix to infer process from and aggregate and pairwise patterns of species associations. *Boy, this abruptly closed an interesting paragraph. I’d like to hear more here. Unfortunately, the data structure aspect overshadows the three major and distinct themes.*

The *S. purpurea* food web is an aquatic, detritus-based (“brown”) food web.  
*Kinda the opposite of charismatic megafauna.*

*It would be nice to review the natural history of the 5 species or to refer the reader back to a section that does that. These are not species that people have kept in mind.*

*Chapter 10 is an interesting change to an experimental reporting style of presentation. But the exploration of the results and of which is correct etc. is rather involved.* Given the limited number of taxa involved, the homogeneity of the newly opened leaf habitat, and the diverse body sizes and trophic roles of the taxa, this result should not be surprising.

Yet, the temporal sequence of species occurrence (figure 10.4; Ellison et al. 2003) did not conform to any of the classic succession models. In short, tools designed to analyze co-occurrence and interactions within a single trophic levels did not capture completely the complexity of the structure of a multitrophic food web. *But these tools don’t work well to describe the sequence within a single trophic level either—do they? As you have discussed and has been explored elsewhere.*

Ecological networks are not constant in space or time. Like single trophic-level ecological communities, stochastic and deterministic processes interact to produce the patterns we see in nature. The networks we observe themselves are contingent

on where and when we sample them, and on our ability to detect the individual species and their interactions with other species within them.

Ecology has few model systems and even fewer model organisms (chapter 1). Levins (1984) summarized three features of model systems—tractability, generality, and realism—that Srivastava et al. (2004) cogently argued are present in natural microcosms such as *Sarracenia* pitchers. *Also familiarity? Or, the ability to understand and relate to the model? Seems like a model needs to be somewhat memorable or charismatic to be effective, in the sense of being relatable and memorable, like lynx and snowshoes or Isle Royale wolf and moose, sea urchin and starfish, or the naked mole rats. Sarracenia as an organism is, but the food web is unfortunately not.*

1. The *Sarracenia* food web and other container webs are “normal” food webs<sup>331</sup> system-independent, “scalable” questions: how does food-web structure vary with spatial extent (Buckley et al. 2003, ??extra line insert??

These experiments have taken on additional urgency as anthropogenic modifications of the global environment are changing species composition (Dornelas et al. 2019), reducing population sizes, and accelerating species extinctions at local and global scales (Lewin and Leakey 1996).

*Urgency for who? Perhaps cite some papers to illustrate? An examination of the role of food web studies in addressing pressing global issues such as extinctions and the biodiversity crisis would be a good theme to play up and discuss up front.*

*If habitat size is a key factor in controlling food web dynamics (as well as feasibility of study) doesn't that immediately limit the broad generality of the Sarracenia food web model?*

*Habitat expansion experiment – how great an expansion does this [adding some water] represent (%)? How much fluctuation in habitat does a plant and food web undergo through the year? And, e.g., with varying weather conditions? Is this expansion something that happens in nature regularly so is built into the behavior of the system?*

Top-down control is a major driver of food-web structure across ecosystems (Menge 2000, Shurin et al. 2006, Frank et al. 2007, Gruner et al. 2008). *Certainly, the supporting literature goes back many decades earlier?? To all of Val Smith's and Joe Shapiros work on aquatic systems and likely a century earlier?*

Experiments in the last 15 years have identified linkages between trophic structure and habitat size in aquatic container habitats (e.g., Srivastava 2006, Petermann et al. 2015a, 2015b) and terrestrial islands (e.g., Pringle et al. 2019), buttressing Pauly et al.'s (1998, 2000) conclusion that top predator removal *exerts??* strong effects on trophic depth and the abundance of prey species.

When bacterial diversity has been examined explicitly, classical keystone effects of *W. smithii* always have been found. *How does the need to include bacteria affect the use of Sp as a model?*



However, we do know that living container habitats such as bromeliads, Heliconia bracts, and pitcher plants (table 11.1) provide opportunities to study experimentally the dynamics of a food web that re-assembles annually in a habitat that itself is a living, growing organism.

*Fascinating, but also limiting?*

Processing of captured prey by inquilines releases CO<sub>2</sub> and NH<sub>4</sub> that are taken up by the plant (Joel and Gepstein 1985). The plant, in turn, oxygenates the pitcher fluid, preventing a switch to anaerobic conditions, which can nonetheless occur if prey are superabundant (see also chapters 13 and 14).

*Seems like an intriguing and basic fact and set of relationships that might have been presented and expanded on much earlier. I did not see it if it was.*

eclosion in the inquilines (figures 10.4–10.6) *Yikes – eclosion. Good thing for Wiktionary!*

Working at Swift River Bog, a 1.9-ha glacial kettle bog in central Massachusetts, we conducted three experiments to examine the joint effects of environmental variability

*Why was N selected as the environmental variable? Because it was simple? Seems like that warrants explanation, both because it is one of many possible variables and not the one that comes immediately to mind as an environmental variable. More of a limiting resource. More importantly, would this be likely to provide generalizable results?*

with the ease of doing manipulative experiments with *S. purpurea* and its inquilines

*At least of a certain limited type, such a N additions, species modifications. Other types of environmental limitation such as drought, temperature, or habitat size it is less clear that this system is easy to work on.*

Finally, the habitat in which the *S. purpurea* food web assembles—the plant itself—is not an inert container or an external “environment” but rather is a co-determinant of dynamics in this model ecological food web. *Is that a benefit to its model status or does it detract from its value and use?*

Unfortunately, this analogy obscures the true meaning of tipping points, *This is unclear – if Gladwell redefined TP and then used an analogy to help people understand that, what is “the true meaning” of TP? Is it the original definition, Gladwell’s new definition? Did his analogy not fit his new definition? Etc.*

*Seems like if Gladwell redefined it, no one is in a position to say whether he is right, as he is true to his new definition. In any case, it seems to me that bringing Gladwell in creates a real distraction in this section. Why is he necessary?*

*And, with all the ongoing debate despite the very many studies and loads of money spent, it makes one wonder as to the entire value of the regime shift thinking. If we don’t even know it when we see it, or know how to discuss it, does it really help our thinking? But outside of lake ecosystems, experimental evidence for CSD remains limited, and the theoretical literature continues to grow faster than the data supporting it.*

*Is it necessary to bring in Lotka Volterra? I get Peter Abrams nightmares and this seems like the succession, and Markov model sections, dredging up old material that is well worn and of limited utility.*

*Are political regime changes really helpful or accurate analogs?*

But in the context of global climatic change, regime change following an increase of [CO<sub>2</sub>] above 400-ppm, again seems like a bad idea. *I don't understand this, especially following the discussion above.*

*Figure 12.2 Rather than a hypothetical, it would be nicer to have a real example.*

*The lengthy review of regime shifts is a bit like that of succession. It seems very basic relative to the complexity of other material being presented (like the modeling and statistical analysis) and so I wonder at its utility. Is such a review needed here and how does it mesh with the detailed experimental examples and results explored in other sections?*

Even basic terms such as “stable” and “non-linear” mean different things to empiricists and modelers when they link data to analytical models (Grimm and Wissel 1997, Donohue 2017). The vocabulary of popular metaphors, such as regime shifts and tipping points has muddled the waters even further (chapter 12; Petraitis 2013, Angeler et al. 2016). *There is nothing reassuring here about the entire subject! Seems like we really are not going to learn anything of real value, but might get mired in theory or academic discussions.*

it is doubtful that monitoring of traditional indicator variables can provide enough lead-time for intervention to prevent a regime shift. *Again, if there is little promising then why continue with this line of work and discussion.*

Because most ecological systems are dynamic, a stable state is neither a pattern of stasis nor one simply of no relationship between a driver and a state variable. Rather, we suggest that the simple model of environmental tracking (figure 12.4 top) is an appropriate null hypothesis. If the state variable tracks the driver, then we may still see changes between states that could appear non-linear (Ratajczak et al. 2018). There might even be time lags present between a change in the driver and a change in the response variable. However, those time lags should be relatively constant and not differ for high and low levels of the driver. *Where does this leave us? Anywhere but discourage?*

Our work with the *Sarracenia* microecosystem discussed in the next two chapters illustrates the power of experimentally manipulating a model system to understanding tipping points and regime shifts (Boettiger and Hastings 2012). *It boggles the mind to entertain the thought that after decades of work by Carpenter and others on lakes etc. that leads to a completely inadequate connection between models and management and policy that anything revelatory will come from pitcher plant systems. That seems to be headed in the wrong direction and back towards theory and abstract.*

1For he was stirring up billows in a ladle. The presumed origin of the phrase “tempest in a teapot”. *This seems like an interesting analog, but one not directly connected to the origins of the phrase as there is nothing about a ladle that relates to a teapot.*

Ecological communities do seem to display some level of stability and resistance to small, incremental changes in driver variables. *Yikes. A truism?*

The *Sarracenia* microecosystem is especially well-suited to investigating alternative states. *At least as they pertain to Sarracenia food webs. It is much less well clear that they can be used to understand others.*

the *Sarracenia* microecosystem has three clear advantages relative to other, larger aquatic ecosystems. *Yes, but isn't each is a huge limitation? And none suggest that the results will be broadly applicable or...useful? Except in a general theoretical way. I'm trying to cast this in light of the pressing need for the ability to deal with real world problems that was described at the top of the chapter.*

In short, studying tipping points and alternative states in the *Sarracenia* microecosystem yields rapid, cost-effective data and insights. *That could be applied to managing a...Sarracenia food web? ☺*

Finally, we will explore new work on metaproteomics, which may reveal candidate proteins and other biomarkers that could give us a better early warning indicators of state change than simply measuring oxygen. *Is there an analogy for the real world and Carpenter's lakes?*

Various typologies have been proposed to distinguish among ecosystem functions, services, and goods *Not clear why there is an immediate jump from EF to EF, S, G? Why not just focus on function? There is also a shift to ecosystem services that is not anticipated or explained. Why? When Bormann and Likens studied function they did not start with services? This seems like an unhelpful and unnecessary diversion into a new topic that is unrelated to foodwebs and Sarracenia.*

*Similarly, the jump into superorganisms and health seem like another unrelated and somewhat illogical step. There is no direct connection between ecosystem function and Clementsian thinking or services and health and no obvious reason to explore these here.*

*Why the jump to cybernetics? Again, why not just speak directly to function?*

But how can we extract a concept of information from these processes? ???

*This entire paragraph seems rather contrived to get to a place where biodiversity and function are related when there is no obvious logical reason to get there. There are many other places dealing with ecosystem function where this section could have landed, but getting to biodiversity and function by way of services, good, Clements and information theory is a bit of a strange landing place.*

*It seems like with succession, and food webs, and regime shifts, that there is a desire to explore the notion of diversity and function. Is this necessary for this work?*

In particular, we might speculate that a healthy ecosystem is one that has a greater number and diversity of biologically active proteins that should allow for better homeostatic control in variable environments and provide more products and services that are important to humans. *Seems a bit gratuitous – you might also speculate the opposite, or something else...*

it is the activity of microbes that translates into much of what we ultimately characterize as ecosystem function. *But, we never defined ecosystem function.*

*Deep in the proteonomics it seems a long distance from those pressing global problems.*

Alternatively, the imprint of the past was been lost and ecosystem function was no longer the same. *New language and terminology here. Imprint of the past? What is an imprint? Just the initial conditions? Why expect a return? Wouldn't there be a legacy of the historical trajectory of the recent past that would prevent a return?*

Much current research is focused on defining and measuring ecosystem function, but most definitions ultimately reflect categories of ecosystem services that are beneficial for human health and well-being. *It is as if 50 years of ecosystem science has been ignored that had no connection to services. Why not refer to that and to function.*

*Proteins is not where one would obviously start with function broadly. It comes across as if, there was need for a metric, proteins were readily available as a diagnostic and so the definition of ecosystem function was contorted to yield proteins as the answer instead of all of the classic parameters for function.*

*It is not clear how these results pertain to the prior results, to Carpenter's search for an earlier warning signal or especially to the pressing interests of society. Shades of LeGuin?*

More generally, we have tried throughout this book to make the case that *S. purpurea* and its inquilines is a true model system for ecological research.

*This I would say has been accomplished. But, it is a system that seems to have fairly limited applicability beyond testing models and specific theory, rather than leading to direct application in management, decision-making or grappling with larger issues. As of yet, it has not been brought back e.g., to the lake studies to show how it improves on their work or application. And, in contrast to other models it is challenging to understand, remember or to seize the imagination because the system is so small and the organisms are relatively difficult to understand and to name.*

*The emphasis on developing the *Sarracenia* model and understanding of the system is a bit worrisome. Why? Here is the place to demonstrate what has been learned and can be applied to other systems. But, much of the writing in this section seems to focus on improving this system for further study or to show how it validates or conforms to other systems or known models rather than to show its application and utility. Does the inclusion of intraspecific variability*

alter food-web dynamics in the *Sarracenia* microecosystem, as it appears to in many others ...However, the precise control over the tipping point that we had hoped to achieve has so far proven elusive. More experimental work, both with *Sarracenia* and other ecosystems, is needed if we are going to use models of tipping points and regime shifts to manage and control ecosystems in the face of rapid environmental change.

*How would the latter be accomplished? So far, there appear to be large challenges to simply working out the *Sarracenia* model and making sense of it. Given that, how would we begin to apply it to help manage and control other ecosystems? And when might the *Sarracenia* system itself be worked out? In 10-20 years, or 50 years? And, when that is accomplished, when might that then be applied more broadly – 100 years? I'm trying to forge a realistic scenario that extends beyond simply applying for more NSF grants in order to continue to work out a model system and to focus new effort that allows researchers to apply what is learned from this system in a time of a global emergency.*

First, ecologists usually interpret the shift from one state to another and its subsequent return to the initial conditions as a return to the original state. *This kind of statement has existed for decades, but are there ecologists who believe this, or ever did? What fools are these? No one really talks about returning to original conditions, do they? If so, they must not be connected to anything real.*