Herbivore Pests, Habitat Islands, and the Species-Area Relation

Jorge R. Rey
Earl D. McCoy
Donald R. Strong

Follow this and additional works at: http://scholarcommons.usf.edu/bin_facpub

Part of the Medical Sciences Commons

Scholar Commons Citation
Rey, Jorge R.; McCoy, Earl D.; and Strong, Donald R., "Herbivore Pests, Habitat Islands, and the Species-Area Relation" (1981). Integrative Biology Faculty and Staff Publications. 15.
http://scholarcommons.usf.edu/bin_facpub/15

This Response or Comment is brought to you for free and open access by the Integrative Biology at Scholar Commons. It has been accepted for inclusion in Integrative Biology Faculty and Staff Publications by an authorized administrator of Scholar Commons. For more information, please contact scholarcommons@usf.edu.
HERBIVORE PESTS, HABITAT ISLANDS, AND THE SPECIES AREA RELATION

In "Hosts as islands" Kuris et al. (1980) criticize our analyses of phytophage communities. Here we disagree with some details and implications of their paper.

PHYTOPHAGE COMMUNITIES AND TIME THEORIES OF DIVERSITY

Time theories of diversity argue that slow evolution of accommodation among species accounts for substantial fractions of geographical variation in species richness. Niches are posited to narrow in an evolutionary response to prolonged competition or predation, with increased numbers of coexisting species as the result. Thus, species in communities of more recent origin and those which have been perennially disrupted should not have evolved as much interspecific adjustment as older more stable groups of species, and as a result, younger or more disturbed communities are predicted to be less rich in species. Time theories assume that interactions among species place a major limitation on species richness of communities (Wallace 1878; Fischer 1960; Sanders 1968; Whittaker 1975).

Our analyses of phytophage accumulation by host plants are designed to make operable and test the time theories. We ask, Do older communities have more species, other factors being roughly equal? Have host taxa which have been in a region for longer periods accrued more species of phytophages? We began with Southwood's (1960, 1961) discovery of the species/area relationship for phytophages, together with the long-known fact that plant taxa begin to accumulate pest species very soon after introduction into a new area. Southwood found that greater host range generally meant more associated species of phytophagous insects. This makes orthodox ecological sense; greater range means greater diversity of habitats, climates, and number of other host species, all of which would tend to increase the diversity of phytophage species to which a plant species is exposed. Host range affects B (between-habitat) diversity for phytophages. Many phytophage species are restricted to a portion of a host's range, so reduced range means reduced total number of phytophage species for a host (Strong 1979).

The crux of our analyses has been to differentiate the relationship of insect species number to the host area from any relationship between species number and host age (à la time theories). We did this by comparing species number among phytophage communities of differing ages, after subtracting the effects of host area. Maximum community age was estimated as the length of host residency in a region. As mentioned above, this estimate is justified because insect species begin to associate with plants very soon after they are introduced into new regions. We found that a large majority of pest species on introduced crops are recruited autochthonously from the local fauna within the region of introduction. Only a small portion of pest species occurs in more than one region of the crops that we studied. This means that most pests accumulate from local ecological
processes after a crop's introduction; very few are carried by man from other regions where the host grows.

We have made this analysis with four different sets of data: the insects of British trees (Strong 1974a), the parasitic fungi of British trees (Strong and Levin 1975), insect pests of cacao (Strong 1974b), and the insect pests of sugarcane (Strong et al. 1977). The first two studies compared numbers of insect and fungus species among various tree species in Britain. The last two studies compared numbers of insect species among regions into which these two crops had been introduced. The first three studies inferred community age from the hosts' residency status. Phytophages of native host species were considered to have resided together longer than those of hosts introduced in historical times. The fourth study compared total insect species among regions into which sugarcane has been introduced for different lengths of time. Time since known introduction varies between 150 yr and 2,000 yr among the sugarcane regions for which there are data.

The results of our analyses are consistent. We found significant relationships between the total number of phytophage species and host area in every instance. In none did we find that great community age affected significantly the total number of these species associated with plant species. During the initial years of growth in a new region, pest species accumulate and numbers of species are correlated with residence time of the host. After this relatively short initial period, however, time quickly ceases to contribute, and only host range remains as a significant correlate of pest species richness. The result is consistent with results of some paleoecological studies which have tested the influence of time on species richness for animals other than phytophages (Bretsky and Bretsky 1976).

Certainly, some individual phytophage/plant associations develop more slowly than diverse groups of species that we have studied. Coevolutionary associations that involve insects with relatively few host species probably develop more slowly, on the average, than do communities recruited wholesale on introduced plants (Strong 1979). In the biotas that we have studied, insect colonization limited by slower processes must be only a small fraction of the total number, and its statistical effect is swamped by the species recruited quickly. Undoubtedly, we have studied species accumulation which involves only minor evolutionary modifications. We are extremely interested in systems which behave differently than the ones we have investigated (e.g., the very slow recruitment of insect species to introduced cactus [Moran 1980]). Results which contrast with ours contribute greatly to understanding communities of insects on plants.

**DISCUSSION**

*Island Biogeography and the Species-Area Relationship*

We do not use the theory of MacArthur and Wilson (1967) in our analyses, and our methods and conclusions stand independently of any assumptions or deductions of the theory of island biogeography. We 'do not consider the equilibrium model (MacArthur and Wilson 1963, 1967) sufficiently operational, or that its predictions are sufficiently distinct from those of other models, to warrant casting
biogeographical dynamics of herbivorous insects on host plants in its mold’’
(Strong 1979, pp. 102–103).

The species-area relationship is merely a simple correlation between two variables. It no more requires special conditions of real islands, homogeneous areas, continuous subareas, or regions not defined by political boundaries than any other correlation requires some ideal set of conditions to be valid. For example, would a nationwide relationship between the number of farm implements and the area of farmland among states be invalid because states are political entities or because farmland is noncontiguous and subdivided within states? Would a correlation between population and urban area of countries be invalid because countries have many different cities, cities are not contiguous, and some cities straddle national boundaries, or because urban places have greatly varying population densities? Our, and most other, species-area relationships make no more assumptions than these analogous simple correlations.

We believe that Kuris et al. do not sufficiently differentiate between species-area relationships, which are simple empirical descriptions, and the island biogeography of MacArthur and Wilson which is one of the several theories using these relationships. The species-area relationship is so general and assumption free that no theory uniquely predicts it and all theories accommodate it (Connor and McCoy 1979). Species-area applications greatly antedate island biogeography theory (Arrhenius 1921, 1923a, 1923b; Gleason 1922; Cain 1938; Goodall 1952), and long have been used to describe all sorts of areal distributions of species on islands, on mainlands, and among quadrats. Obvious evidence of the theoretical neutrality of the species-area relationship comes from within-island biogeography. Preston (1960) believed that ‘‘true isolates’’ with virtually no effective extinction or immigration from outside would give a species-area effect, just as would partial isolates which had some species turnover. Both would have species-area relationships: the only difference would be in slope. Note that Connor and McCoy (1979) show other, simpler causes of slope differences. In sum, we see the species-area relationship as no more than ‘‘a useful empirical descriptive device’’ (Strong et al. 1977, p. 169).

*Islands and ‘‘Islands’’*

We have not used the words or the metaphor ‘‘host islands’’ in our analyses. We have used the word island in reference to terra firma isolated by water, or in reference to other authors’ use of the host or habitat island metaphor. Kuris et al.’s statements that we: ‘‘... use the acreage of crops as islands’’ (p. 570), ‘‘generate . . . ‘geographic range islands’. . . .’’ (p. 571), ‘‘. . . define islands of agricultural crop acreage . . . .’’ (p. 574), and use ‘‘‘Crop islands’ . . . .’’ (p. 575) and ‘‘. . . host geographic ranges as ‘islands’. . . .’’ (p. 578) are, we believe, literal misrepresentations of our work. The quotation marks in Kuris et al. around the world ‘‘island’’ and similar phrases in reference to our work are of uncertain meaning to us.

We do not use the host island metaphor because the careless or casual reader might infer that we had implied more than a simple correlation between host range
and species number. To infer from our use of the species-area relationship for insects on plants that we are invoking Janzen's (1968) metaphor of host islands, or rationalizing our results with the various theories of MacArthur and Wilson (1963, 1967) is to commit the logical error of post hoc ergo propter hoc.

**Patchy Distributions**

"... political boundaries cannot serve to define populations as islands..." (Kuris et al., p. 582).

We believe that this is literally incorrect for every island that is also a political division. For example, Cuba, Grenada, Guernsey, and Mauritius, among many other islands, have land and political boundaries that coincide. Of course, the crops which we compare are within political boundaries; all crops are. Of course there are separate patches of crops within each region; virtually every region has patches of crops; we have never claimed the contrary. Patchy distribution is a normal ecological characteristic of most species at most scales of geographical resolution. Species are generally patchy within continents, within regions, within physiographic areas, clusters, localities, colonies, and ultimately, clumps within colonies (e.g., Krebs 1972, fig. 38). Any geographical unit contains species in patches. Even a host individual contains parasites in patches. If one compares crops at a scale finer than we have, there is both host and insect patchiness at yet a finer scale. Real islands too are internally patchy, and insular habitats for non-parasites occur in patches just as those for phytophagous arthropods. Many islands in species-area relationships in MacArthur and Wilson’s monograph (1967, fig. 2) are political entities, and habitats are certainly not homogeneous over them. Patchiness and habitat heterogeneity are probably important causes of species-area relationships (Connor and McCoy 1979; Strong 1979). Larger islands contain more habitats; species are relatively faithful to habitats; thus larger islands contain more species. This has long been an explanation of species-area relationships (Williams 1964; Hamilton et al. 1963). If Kuris et al. require homogeneous islands for their biogeography, they are excluding the islands used by MacArthur and Wilson and by Preston. Actually, most species-area relationships compare archipelagoes of similar habitat patches, not homogeneous islands of uninterrupted habitat (Connor and McCoy 1979). This makes no difference because larger archipelagoes contain more species just as do larger islands.

This point illustrates what we see as a great problem with Kuris et al.’s analysis; it does not attempt to measure the effects of supposed biases. Are the effects relevant to our question? Are they slight, moderate, or serious? Kuris et al. present no evidence or argument that patchy distribution would have any effect on our analyses of species-area relations, or of time and diversity of herbivores. They only say that patchiness violates their idea of an ideal island.

We found it odd that Kuris et al. would use their figure 2 in objection to our analysis because it is fully consistent with our position; it shows that the total number of species of sugarcane pests is highly correlated with area, and that the correlation develops rapidly, our main conclusion. We found this to be true globally, among many regions; Kuris et al.’s figure 2 shows that correlation of
species with area develops rapidly also within a region (Venezuela). In figure 2, their being impressed that, in one case, species number of insects increases shortly before area of sugarcane, rather than the reverse, is an overly literal interpretation of the methods. We have never claimed that there is an exact, perfect correlation between species and area. We have emphasized the inaccuracies of the data and the high variance of species-area relationships (Strong 1979; Connor and McCoy 1979). Their finding in figure 2 is equivalent to points in a species-area curve which are not on a straight line (correlation coefficient less than 1.0). The degree of variation in species-area correlation that Kuris et al. discuss in reference to their figure 2 is well encompassed within the scatter of points of our figure 2 (Strong et al. 1977).

Kuris et al. also state that H. E. Box was hired by the Venezuelan Ministry of Agriculture in 1947 to study sugarcane borers. They imply that his research in Venezuela commenced on that year and as a result his published species lists of 1953 reflect only a short period of collections in that country. Actually, Box’s studies of Venezuelan sugarcane insects began in 1926, when he started his studies in collaboration with J. G. Myers. This can be found in Guagliumi (1962), one paragraph before that cited by Kuris et al.

Fluctuations in Productivity

Fluctuations in productivity or harvested area of crops do not imply that the geographical area in which the crop grows fluctuates, nor do they imply insect pest species are lost, because crops remain growing in regions that have short-term lapses in production. Insects remain growing on locally fallow or unharvested crops. For example, cacao is produced by a tree only after several years of immature growth. Cacao trees grow independently of local market productivity. For cacao, years of low productivity are years of heavy pest attack, low price, poor weather, or the like. Cacao pests remain in regions when productivity dips. Sugarcane matures for harvest more rapidly, but even with more rapid turnover; dips do not imply that the geographical area in the crop changes greatly. Even in years of low cane planting, the crop grows feral in the fields, at the edges of ditches, and along roadsides. Feral populations of cane support pests well, and subsequent years of heavy planting have local pests ready to go.

Age of Crops

“... date of introduction of cane as an indicator of ‘crop island’ age ... can be misleading” (Kuris et al., p. 576).

We believe that this statement is literally incorrect. Date of introduction is an exact indicator of crop island age (age = present date minus date of introduction). Kuris et al. do not question the accuracy of our dates, but instead follow their statement with a quotation that shows date of introduction to be a poor indicator of area (rather than of age) for Puerto Rico. We completely agree with the implication of the quotation; age and area are poorly correlated for crops in general. This has been exactly the point of our publications in this series, to
differentiate the effect of recent area from any effect of age of crops on insect species richness. The Puerto Rico quotation underscores our reasoning and conclusions: Area of crops and their insect species richness are well correlated, areas are of relatively recent origin, thus insect species richness develops rapidly.

We do not believe it logically possible to deduce a single "correct" index of age for hosts, vis à vis time's influence on species richness. Any single age index will not take into account the vagaries of additional influences (such as the availability of new parasite species, their probability of colonizing the host, and their probability of enduring after colonizing). Our philosophy is that one can only test whichever indicators of age are available, and we have done this (references in introduction).

**Extinction**

We believe that Kuris et al.'s point about extinction is also irrelevant to our analyses. The possible disappearance of previously present species makes no difference to our analysis or results. We do not assume any model of turnover. Species may or may not disappear. We do not assume that they do not. We only set out to test whether more net species have accumulated in older regions, as predicated by the time theory. We find that more species do not accumulate in older regions, thus overly inflated species lists are irrelevant to our question. If we were rationalizing our results with the MacArthur-Wilson theory, finding that species disappear would be encouraging rather than disturbing because the MacArthur-Wilson theory assumes turnover.

Kuris et al. have no evidence that extinction actually occurs on the regional scale of our analyses. This renders their citation of Lynch and Johnson (1974) questionable. Lynch and Johnson's main point is that turnover is quite difficult to establish with indirect data. This is explicitly our point of view (Strong 1979). We doubt that turnover of crop pests occurs on regional scales with great frequency. Some of our oldest species are known not to have become extinct. For example, the species of cacao capsids (Heteroptera), which were among the first pests recruited to cacao from the native fauna in Ghana, are still very much in evidence on the crops there (Dennis Leston, personal communication). Though we have found several examples of pest species disappearing from lists, we suspect that changing cultivation practices, rather than natural extinction, are the cause.

Several species of banana pests of the 1950's and early 1960's are no longer of concern to agronomists in the Golfito region of Costa Rica since cessation of wholesale application of insecticide (C. Stevens, personal communication; Ostmark 1974). All of these species still occur in Golfito in low densities on bananas and on related native plants such as *Heliconia* (D. R. Strong, unpublished). Documented regional extinction of phytophages is very rare (Strong 1979). When it occurs, man or man's agents are often known to be the cause. Of course, local disappearances of arthropod populations occur in small clumps of host plants (Simberloff 1978a; Rey 1981), but our studies of regional associations of insects consider a much greater scale in which local extinctions would likely be made good by immigrations among clumps of host plants.
Sampling Effort

We believe Kuris et al. are simply incorrect in their fourth point. Correlations between species, research effort, and area do not create artifacts for species-area relationships, but rather are necessary for them. If larger areas have more species, more effort will be required to search larger areas and find the greater number of species. We call this the "entomologist-area" effect (Strong 1979). Opler's species-area relationship of leaf miners on oaks is one that Kuris et al. accept as valid, and this relationship is based upon an extremely good correlation of research effort and numbers of species. For example, thoroughly searching Quercus agrifolia-wizlizenii over its 56,000 km² requires much more research effort than searching the distribution of Q. duni over its approximately 640 km². Research effort and species number are correlated in all valid species-area relationships. For another example, more effort, authors, and journal pages have been dedicated to the biotas of states than of countries, to seaboards than of states, and to the entirety of North America than to individual seaboards. Are the consistently greater numbers of species reported for the larger divisions an artifact of unequal sampling effort?

Validity of Published Data

"... species lists compiled for other purposes often do not provide appropriate data to construct a species-area curve" (Kuris et al., p. 582).

Kuris et al. give no evidence for, nor any example of, this assertion. Figure 2 of Kuris et al. is a species-area curve constructed from species lists compiled for other purposes. We use species lists which are published by other people, just as have many other authors who have used the relationship, e.g., Kuris et al. (1980), Williams (1964), Preston (1960), MacArthur and Wilson (1967), Schoener (1976), and Ricklefs and Cox (1972), Connor and McCoy (1979). To assume that our species-area relations are artifacts or that those with parasites are so, while others based on similar data or involving nonparasitic organisms are not, is illogical. Are Southwood's (1960, 1977) insect species-host range relationships artifacts? The British flora and fauna is as well known as any. For the relationships to be a mere result of biased collecting, the tree species with very restricted ranges would have to have actually many more than the few species known for them, and/or the widely distributed trees would have to have fewer species than are known. The narrowly restricted trees are curiosities, and attract a fair amount of attention from collectors. If one were to argue an effect of biased collecting, it logically should be just the opposite of that implied by Kuris et al. Broadly distributed host plants are probably undercollected, relative to those with restricted distributions.

Southwood collated his original data over 20 yr ago, and though the newest data available are slightly different, the species-area relationship is, if anything, improved as a result (T. R. E. Southwood 1977, personal communication). Lawton's work (1976) with bracken in England is another case in point. In 4 yr of intense field work with the fern he found one species of associated phytophage which was
not in the earlier species lists (J. Lawton, personal communication). Ward's study of the specialist arthropods of juniper is another example where several years of field work with repeated visits to a large number of sites verified the species-area relationship of insects on plants (discussed in Strong 1979). Ward's data are collected as carefully and with as much detail as Opler's. The significant species-area relationship from Ward's data obtains among bushes at one site and between sites (Strong 1979). Sufficient data are in the literature antedating Ward's study for construction of significant and reasonably correct species-area curves.

Kuris et al. do not attempt to test the effect of any biases on insect-plant species-area relationships. Neither Kuris et al. nor anyone else has demonstrated that any species-area relationship for arthropods on host plants is an artifact. The literature they cite is appropriate for such tests; field work and literature records consistently yield the same result. Studies which have the potential to demonstrate that literature records are too inaccurate for making species-area relations (such as Lawton 1976; Ward 1977; Opler 1974) demonstrate just the opposite. Certainly, literature records contain minor inaccuracies, but to jump to the conclusion that minor inaccuracies invalidate major trends in data is unjustified.

Kuris et al. selectively choose evidence to discredit some species-area relationships for insects on plants. Perhaps the most obvious example is their incomplete citation of Claridge and Wilson's (1978) finding of a lack of species-area relationship for leafhoppers on British trees. The problem with the citation is that a major conclusion of the Claridge and Wilson paper is that our original finding of a species-area relationship for a large sample of British arboreal phytophages is valid. With different data, Claridge and Wilson found essentially the same relationship that we did (p. 452): "There is, then, good evidence to suggest that the numbers of associated insect species are closely correlated with the recent abundance of their host plants." On the matter of our test of the time theory, Claridge and Wilson also find the results of their analysis in agreement with ours (p. 452): "Thus, there is now little published support for the geological time theory in this context. . . ."

Claridge and Wilson's (1978) insignificant species-area relationship for mesophyll leafhoppers is the result of undercollecting; it results from sampling efforts not being correlated with host distribution. Hopper species are only from three sites in a small section of South Wales, but their species-area relationship used plant distribution data from all of Britain. Since their first paper, Claridge and Wilson have added insect data from two other sites, one in Scotland and one in England. These new insect data grouped together with the old do give a significant species-area relationship of British trees (M. Claridge and M. Wilson, personal communication).

How large-scale geographical species-area patterns translate into patterns of insect and plant abundance is an extremely intriguing and unsolved problem (Futuyma and Gould 1979; Strong 1979), but local patterns do not invalidate correlations found over larger areas. Howard Cornell is now studying local abundance of oaks and their cynipids (Hymenoptera, Chalcidoidae) at many sites in California, in order to compare relationships between local and regional patterns.
of insect species richness. This study is extremely useful because Opler’s (1974) data concern the same host populations but different insects (leafminers).

Hosts as Islands

Even though we do not base our studies upon MacArthur and Wilson’s (1963) biogeography, and do not invoke the hosts as islands metaphor, we find Kuris et al.’s “fundamental theoretical difficulties” (p. 580) with the hosts as islands notion (Janzen 1968) deserving of comment. (Our numbering follows that of Kuris et al.)

1. Although the terra firma of islands does not resist colonists, as plants might resist colonists with secondary chemistry, or animals with immunity, etc., islands have biotas that can resist colonists. Lowered invasibility of islands, that results from species already present is a large part of MacArthur and Wilson’s (1967) theory. Certainly the mechanisms of resistance are not homologous for individual plants and the biota of islands, but Janzen’s point was that they may operate analogously. For example, host plants have predators among their associated animal species, and they have parasitoids which could have substantial influence upon new phytophages (Lawton and McNeill 1978).

2. Although islands per se do not have a physiology that would yield age-specific defensive responses like host species, islands do have biotas that could function in this way. Succession of vegetation on islands can give an “age-specific” effect to animal residents (Willis 1974). Neither we nor Janzen would argue that islands have life histories parallel to host individuals, but only that any assumption that islands are passive environments to nonparasites is unjustified.

3. Obviously, islands per se are not eliminated by colonists; the organisms of islands which support species may be eliminated. Extinction of plants caused by goats or cattle could make some islands effectively “dead” to a bird resident which required the plant for nesting or for food (Lynch and Johnson 1974). Again, organisms which live on real islands affect those islands’ ecological characteristics for nonparasites. In this regard islands and hosts are analogous.

4. Janzen’s analogy explicitly refers to populations and ranges as well as to individual hosts. Host populations are the means of support of many parasite populations, and host populations persist long enough for parasite immigration and extinction rates to be a valid concern.

5. Distances among islands do not change (nor do those of individual im-mobile plants change) but the abilities of species to reach and colonize islands vary with winds, source population density, and the condition of the biota already on the island. The effect of this is to impart tremendous indeterminancy to the colonization of real islands (Simberloff 1978b). Stochasticity of the colonization process is the biogeographic property that vagility of animal hosts would affect.

Each of these “problems” is rather an obvious physical difference between real and host islands; each is caused by what we see as Kuris et al.’s very literal reading of Janzen (1968). None is a difference which would necessarily cause the net process of immigration and extinction to be qualitatively different for organisms which inhabit the two sorts of isolates. Janzen after all entitled his paper
“Host plants as islands.” “As” implies analogy, not homology. His paper meant to suggest that there are similarities in biogeographic processes between islands and host plants, not that the processes are literally identical. Here again, we believe Kuris et al. raise issues without attempting to weigh their effect. They have presented no evidence of any consistent difference between the biogeographic processes of real and host islands.

_Coevolution_

The following quotes from Kuris et al. summarize their arguments on coevolution: “Insect plant coevolution may explain the density and diversity of insect species” (p. 580). “Coevolutionary considerations are poorly understood....” (p. 581). “... coevolutionary models are too poorly developed to contend with the complexities...” (p. 581). “Fortunately, fruitful avenues to enhance our understanding of parasite species richness...” (p. 581). “Coevolutionary nature of the host-parasite associations involved” (p. 581).

Their early trust in coevolution wanes then resurges for an upbeat conclusion, but what is coevolution? What is not coevolution? Janzen (1980) warns about the fuzzy thinking that invokes coevolution as a panacea of understanding in ecological and evolutionary studies. Coevolution is only that fraction of evolution that is caused by reciprocal influence between species. We believe that a reasonable null hypothesis for coevolutionary studies is that many associations between insects and plants can occur without much evolution, and that much evolution can occur without any coevolution in either plant or insect. For the case of crop pests, species accrue very rapidly, much more rapidly than the sorts of features could evolve that have been given as evidence of coevolution. Even if one or two abundant pest species do affect the evolution of the crop (either directly or via man’s selection programs), the majority of pest species are quite rare and would have little if any evolutionary influence on the crop. So, species richness would not affect crop evolution, let alone be affected by coevolution of both crop and pest. We believe it important not to beg the question on coevolution. What sort of patterns would one expect in the absence of coevolution? Do real patterns differ from these expected null states?

_Acceptable Research Methods_

We do not believe that: “... interacting [lower] levels [must be] sufficiently characterized [in order] to permit an inductive search for theoretical modifications or alternative models” (Kuris et al., p. 581). To us this is frighteningly authoritarian. Hypotheses should be allowed to spring from any source, and be tested vigorously, independently of knowledge at other levels. Did astronomy have to know mineralogy, geology, and geography of planets before hypothesizing about trajectories or orbits? Did physiologists need data on atomic composition and molecular shape before hypothesizing that hemoglobin carries oxygen from lung to tissues, that auxin affects cell elongation, or that juvenile hormone prevents metamorphosis of some insects? A close-to-home example of the hindrances that
would be created by Kuris et al.’s research formula is in paleoecology. Most individual and population information about communities of the past is lost forever. Should paleoecologists close shop?

We feel that there is no formula for science, save making hypotheses, testing them freely and repeatedly, then tentatively accepting, modifying, or discarding them.

ACKNOWLEDGMENTS

This work was supported in part by NSF grant no. DEB-7921828 to D. R. S. We thank Armand Kuris for discussion on these matters.

LITERATURE CITED


Jorge R. Rey*  

Department of Biological Sciences  
Florida State University  
Tallahassee, Florida 32306  

Earl D. McCoy  

Department of Biology  
University of South Florida  
Tampa, Florida 33620  

Donald R. Strong, Jr.  

Department of Biological Sciences  
Florida State University  
Tallahassee, Florida 32306  

Submitted September 17, 1979; Accepted May 2, 1980  

* Present address: Florida Medical Entomology Laboratory, P. O. Box 520, Vero Beach, Florida 32960.