

AN INSTITUTE FOR THEORETICAL ECOLOGY? PART III: WHY WE NEED IT AND WHAT IT SHOULD BE LIKE¹

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Abstract. Part III of the five part series on Theoretical Ecology presents an argument for the need to establish an organization that can be referred to as the "Institute for Theoretical Ecology". Part I (Conley 1990) of the series provided a discussion of definitions of "Theoretical Ecology". Part II (Conley 1991a) presented both a critique and suggestions for design of a large institutionalized "Center" that emphasized an interdisciplinary program of study such as Theoretical Ecology might present. This paper presents the arguments in favor of the establishment of such an institute, along with some of the design details and philosophies that need to be considered. In general, it is argued that this institute should be concerned with fundamental aspects of ecological theory and with some of the methods that lead to better theory. In order for full success in this realm, this institute should not be concerned with supporting data collections. Instead, the institute should emphasize ecological data management (in the broad sense), data analysis and synthesis, and ecological applications of innovative statistical and mathematical developments that would bring a fresh look at pressing ecological and environmental problems. In Part IV (Conley 1991b) and V (Conley and Brunt 1991c), I provide increasing detail on some of the activities and methods that would enhance the chances for success of this institute.

The Role for the Institute to Play

One of the problems of being middle aged is that you began to see the same arguments come around for the second (and third) time. Contention between "theoretical ecologists" and "empirical ecologists" is again bubbling at the surface. It was never buried very deep. Periodic symposia that last a day or two fail to address the inherent fact that these disparate factions are really not working together very much. Within the realm of theoretical ecology and computational ecology, we continue to quibble about details and methods, and therein, lose sight of the goal of predictability for our understanding of nature. A program that would focus activities on theory and analysis in ecology would go far toward resolving some of these conflicts. Such a program would emphasize bringing available data and researchers together. I refer to this program as an "Institute for Theoretical Ecology".

There is a clear need for the establishment of a program in Theoretical Ecology. Such a program would properly fit within the concept of "Center" if it were correctly organized and located. At the present time funding for innovative theoretical work in ecology is seriously sparse compared to the whole for ecological research. Some of this, no doubt, has to do with the classic model for proposal reviews. Proposals that receive a couple of low ratings and a couple of high

ratings have little chance of being funded, and theoretical work has a tough time getting good reviews. Another reason for this lack is, I submit, the general failure of theoretical ecology to come to grips with the fact that our field cannot provide predictions that can be believed and used to solve some of the pressing ecological and environmental problems of the day.

Review procedures extend to publication outlets as well, where papers that receive mixed reviews are likely to be rejected. This emphasis on rejection is a consequence of the intense pressure for funds and publication space in our agencies and journals. Accepting or rejecting a proposal or a paper must not be an election where you seek to please all of the critics all of the time. It can easily be argued that any proposal or any paper that receives a 100% excellent rating from a representative group of reviewers must provide something for everyone, and is therefore more likely to be plain vanilla than a new plane of creativity. In reading reviews of both empirical and theoretical work in ecology, one notices a distinct tendency for empirical ecologists to support each other's work in reviews and critiques. At the same time, the only work appreciated by most theoretical ecologists seems to be their own. This may serve for some purposes, but it does not result in much funding for theoretical work, and it has resulted in some of our leading journals being

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exceedingly dull to read.

Clearly the field of ecology is big enough for both theoretical and empirical work. Just as clearly, there must be support available for people to sit and think, to conjure models, to analyze and review existing data, to draw on work of scientists in fields outside biology altogether, and to hypothesize about irrelevant and irreverent explanations for phenomena of interest. And further, to do so using the tools of mathematical and computational abstractions. This also includes the right to present theoretical work just as it really is - a conjecture, an hypothesis, that might be just as wrong as the conclusions drawn from a somewhere-out-there field sample. If Theoretical Ecology is to proceed, the freedom to present plausible explanations must be retained. What is needed is a better means whereby out-world theoretical ideas can be expunged from the common thought once they begin not to fit the data. It should not take 15 years to digest a theoretical notion and perhaps to push it aside. Few theoretical essays will bother to indicate where the flaws might be - you just cannot get it through the reviewers if you do. Defending an hypothesis is not testing the hypothesis. The defensive side of Theoretical Ecology breeds quibbling, and that results in fewer opportunities for funding, and less productive work being accomplished.

If a properly organized Institute for Theoretical Ecology could be established, some of these practical problems could be addressed along with the ecological conjuring that would be going on. In Part II (Conley In Press b) of this series on Theoretical Ecology, I provided a critique of large organizational Centers, along with some suggestions for avoiding many of the problems such Centers encounter. In this discussion I describe a model for a new Institute. In order to be successful, the new Institute should be cognizant of the organizational problems described, be designed following the general criteria as described, and should be evaluated on the basis of a suite of emergent properties and products as previously described.

The Institute for Theoretical Ecology should have two parts. There needs to be one or more primary location(s) - the Institute must have a heart, but a distributed heart would probably work if properly organized. These locations provide the service, support, locations for organized visiting and learning and teaching, and working environments where visiting scientists can concentrate on a problem and get some work done. These locations also serve as demonstration sites, providing proof that the program and philosophy of the Institute can be put into practice. The central organizing role is simply several primary locations that provide leadership for the entire program of research, education, and public service. The second part of the Institute is made possible given our current capability with electronic networks. Participants in the global

program of Theoretical Ecology will easily be assembled into an extended or "virtual" Institute. The extended component of the Institute can easily be envisioned as world-wide in scope.

The Institute should be people oriented first, and technology oriented second. This is especially so at the primary locations. The critical resource for Theoretical Ecology is not a lot of expensive equipment, but rather a program that emphasizes knowledgeable people thinking and working together. The shared resource is thus a support staff and a few resident people who make the Institute a place where intellectual innovation and creativity are maximally possible. It should be possible to bring the cutting edges of fields like Computer Science and Mathematics into ecological theory. That requires extended inter-disciplinary relationships. Accomplishing this requires bringing the people who can do such work together in an institutionalized program that has stability.

The Institute should take on the role of coordinating interested groups of researchers. This aspect should be run much as a conferencing program might, responding to requests and inquiries from the discipline at large. Networking for manuscript and data interchange is easy to implement if it is coordinated at central locations. This aspect provides the circulatory system for the Institute, and brings the distributed sites together.

The Institute for Theoretical Ecology should spend more of its funds supporting visiting participants, organizing workshops, symposia, and various gatherings, than it does supporting centralized administrative programs. Resident and visiting cooperators would properly be spending varying lengths of time on specific topics of study. A major role for the Institute for Theoretical Ecology to perform is that of information transfer. This takes on two sides. The first should be called "theory transfer", and you might even call it hypothesis testing. There is a real need to get the theory and empirical people together in the same room to go over the theory with some data. Then go over it again. This would serve to filter out flawed theory. It would also help to sell good theory, suggest critical experiments, and pull the two sides together. That would be good for ecological science. If this works as it might, a critical extension of testing theory and data lies in the many applied problems that await more respectable ecological theory. Bringing the theory and the data together in this manner provides a direct linkage with ecological and environmental problems that will not be solved in any other way.

The second part is technology transfer, the porting of knowledge about how to get things done, the tools and methods, the design of the Institute that promotes creativity and innovation. This includes the psychology of group dynamics in small workshops. It includes the computational environments that support the work,

and the algorithms and philosophies that accomplish and guide ecological analyses and record keeping. These critical aspects of the infrastructure for the Institute are research questions that should be addressed in the context of the activities of the Institute. Done properly, this aspect of the work at the Institute provides for a concomitant enhancement in the support programs along with the explicitly ecological problems that are being addressed. This is one critical way to address the need for the emergent properties required to justify the funding.

If the Institute is fortunate enough to be also associated with sites where field researches are being conducted, then the blending of the theoretical and experimental sides should be a demonstration of how to do it right. Here, the opportunity to put into practice the preachings of the Institute offer an invaluable resource for proving that the Institute programs know what they are doing. This opportunity extends from fundamental data management programs, to actually managing the tract in a wise and appropriate manner.

There needs to be some definition of what products are to be expected from the Institute. I suggest that a new model of workshops (2), where the participants get down to doing the work at the workshop, be explored. Rather than simply presenting their ideas to each other as accomplished facts, participants should do the work, conduct the analyses, develop the models, test the hypotheses, and write the papers as part of the workshop itself. Given such "power workshops" as an operating model and assuming a proper working environment, the expectation would simply be production of some quality papers, proceedings, and perhaps computer versions of models or analyses derived, that could be shared with the discipline at large. A significant portion of this work should involve extensions to real data and real field work brought in by field ecologists. As the Institute and its' programs mature, it will be possible to conduct analyses of the relevant literature in an attempt to uncover the influence that the Institute was having on the discipline. If nothing that mattered was being done the program should be eliminated. It is not difficult to demonstrate that the typical criterion of numbers of reprints produced has little to do with influence. A few papers that are being read and that are demonstrably guiding what people do and think, are more relevant to this question than hundreds of papers that nobody cares much about.

Much of this work and products of the Institute are going to be authored by the visiting participants, rather than the few resident workers at the Institute. Criteria for success involves the extent to which the Institute

for Theoretical Ecology is acknowledged as being an essential catalyst for the ideas contained therein. If the Institute is doing its proper job, there is no expectation that the few permanent residents would appear on any but their own (and fewer than usual) publications. They should be functioning primarily in a support role, and would be part of the largely unseen foundation that made the Institute a success. This group should be kept small, and should include a technical support staff that is evaluated on criteria other than the usual academic ones.

This latter point emphasizes a largely unselfish role for the Institute. It emphasizes unique characteristics for the personnel who are resident at the Institute. Unselfish roles, if played to their full extent, means that it would appear as if the Institute and its' few permanent residents were doing nothing. Careful evaluation would reveal that the Institute was playing a growing role in serving as a focal point for the whole field of Theoretical Ecology. Done well, an atmosphere of creative tension, excitement, and fun, could develop that would tremendously benefit the discipline. This would of course require a revised view of agency manipulation of the Institute, and a radically revised view of criteria for success.

The Institute for Theoretical Ecology should concentrate on being an ecological program. It should not compete with better funded and more knowledgeable programs such as those in atmospheric sciences, geographic information systems, or molecular chemistry. The possibilities for distributed cooperation make this competition unnecessary. The Institute could easily link to other Institutes and Centers where such work was being done, and to various agency data centers and research laboratories as well. When the interests of the Institute for Theoretical Ecology blend into other disciplines, the Institute should know how to draw the inter-disciplinary interests together. In this manner, the best expertise possible is drawn into larger problems such as global change, landscape-atmospheric boundary dynamics, domestic and hazardous waste management, and acid precipitation. The Institute would similarly be in a proper position to join with experimental research programs such as the NSF Long Term Ecological Research sites, Biological Field Stations and museums, the U.S. DOE Research Parks, and other national and international laboratories and centers.

What about the issues that are traditionally considered "theoretical ecology?" Well, they come and they go, but they form the topical core of study for the Institute I describe. Twenty (and more) years ago these would have included stability/complexity, r/K selection, intrinsic/extrinsic population regulation, community matrices, logistic theory, trophic dynamics, and intra-/inter-specific competition and community dynamics. Currently such topics might include hierarchies, webs

2 This new workshops model is described fully in Part IV of this discussion.

and nets and loops, fractals, scale, percolation, risk, chaos, ecology with evolution and genetics, advanced multivariate algorithms for determining structure, and many more.

A primary role for Theoretical Ecology that is to be played by the Institute that I envision is that of turning over the hypotheses as rapidly as possible. This is done by bringing together available data and experts, and really working the data against the theory on the spot, and while tensions and creativity are at a high level. The goal is to get the ideas out and tested as rapidly as possible. This can be accomplished by screening theoretical conjectures by putting them against available data and looking for patterns. In the present scheme of things, available data are far too difficult to locate and access to permit this activity as a general procedure.

Thus, the Institute for Theoretical Ecology explicitly deals with the massive amounts of data that are in fact available, and it explicitly recognizes that we offer hypotheses for the express purpose of attempts to reject. In this context, we have elsewhere described that "... theoretical ecology is dealing more with hypotheses than it is with theories, and certainly ecology currently possesses very few (if any) internally derived laws. One might thus make the somewhat tongue-in-cheek suggestion that 'theoretical ecology' is better termed 'hypothetical ecology' ". (Conley and Nichols 1978). We need to learn how to better address ecological issues in this context, and the Institute for

Theoretical Ecology could help lead the way.

Twenty years from now Theoretical Ecology will have resolved some of the current crop of topical issues and become tired of others. There will be new issues to address. If by that time, the activities of the Institute of Theoretical Ecology have led us to better know how to address such problems, the Institute concept as I have presented it here will have succeeded.

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