WHAT IS ECOSYSTEMOLOGY ?
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Does the coiner of a term own it? I have recently been forced to examine that very question. It involves the term ecosystemology and its definition when it first was coined. What property rights are associated with the term ecosystemology or its definition? Or, for that matter, with ecology?

Let's first consider the now well known term ecology. It is reputed to have been first used by Ernst Haeckel, a German zoologist, around the year 1869.1 He gave us the word oecologie,, along with a very clear definition--good enough to have been carried, largely unchanged, in textbooks and university lectures to the present day. I guess that if Haeckel were still living today he'd be proud of the endurance shown by the term and its definition. And though the field had hard sledding for quite a while, it now has lots of devotees and many pragmatic adherents . I'm sure that Ernst Haeckel would have gotten a charge out of the sudden popularity ecology attained around 1970, just 100 years after the word first appeared in print. What Haeckel might not have anticipated is how ecology would escape from the confines of his zoology field and become entwined with non-biological entities like the mind, politics, commerce, and fear.2

So what did happen to that accepted definition: the study of the relations between organisms and their environment?" Not much, except that relations became the key word, systems was substituted for organisms, and environment got blown up out of all proportion! Connections to OIKOS, the home, have been slighted. Today, ecology is the study of relationships, period. Would Haeckel have cause to complain about the distortion of his coinage? My experiment station project which dealt with better ways to teach ecology. I had been doing quite a bit of reading about the systems approach, especially systems theory, such as papers by Ludwig von Bertalanffy and Kenneth Boulding. But in talking to my ecologist colleagues on the Berkeley campus, I found that this systems stuff was like a foreign language. At least none of them spoke it with me. About this same time I had been collaborating with Frank Pitelka, an animal ecologist, on a study of the arctic tundra and with Hans Jenny, a soil scientist, on a study of marine terraces in Mendocino County. Jenny once made this statement about himself when I showed surprise at his ecological acumen: "About ecology I know very little but about ecosystems I know a lot." When I thought about that statement I realized more than ever that the study of ecosystems was quite a different field than the study of ecology, and not a branch of ecology. Much later, in fact just five years ago, Stan Rowe expressed "the need for a fundamental science of ecosystemology focused on basic units of nature on the face of the earth."3 I went to school with Stan Rowe at the University of Nebraska, 1967-1969; both of us were studying under the plant ecologist J. E. Weaver. I got many ideas from Stan. Maybe, in an embryonic form, the word ecosystemology was swimming around somewhere in Bessey Hall at Lincoln, Nebraska. However, I have never considered ecosystemology to be called a fundamental science .

I had no intention of coining a new word or jump-starting a new field as Haeckel had done. I simply toyed around with ideas for the title of a course I was planning to teach in the Forestry department at U.C. Berkeley. After all, course titles don't usually get elevated to full-blown disciplines, nor are they often listed in glossaries or dictionaries, although such listings may become course titles. However, some time after the word came to me, I thought ECOSYSTEMOLOGY might be a good title for a book that I would eventually write. I wrote a letter about this to Frank Egler, hoping to get some feedback on the idea. I did get some. But also, he spilled the beans by mentioning it in one of his many commentaries that were published in the Bulletin of the Ecological Society of America. This was

embarassing for me because I never managed to get such a book (nor any other book) written.

In the late 1950's then Chancellor Glenn Seaborg appointed a Committee on Natural Resources comprised of professorial faculty from each of the campus departments who taught and did research in the fields of natural resources. The committee met monthly at the Faculty Club; philosophical and research papers were presented and discussed. Two avowed aims of the committee were to establish a research institute and to develop some inter-disciplinary courses around the natural resource theme. I attended some of the sessions although at that time I was not a professor; my rank and title was Specialist in the Experiment Station (but specialist?, not really!). In 1962 I was asked to present to the committee a short paper which I titled "The Ecosystem as a Conceptual Tool in the Management of Natural Resources." Five years later the paper was published as a chapter in a book. 4 (I reminded my students that Silent Spring was published later that year, 1962. I had written my paper before I knew about Silent Spring .) The paper was considered by members of the committee to be the approach they were looking for, as an interdisciplinary graduate level seminar . I was then asked to develop that seminar. Henry Vaux, then dean of the School of Forestry, suggested it be titled "Natural Resource Ecosystems." I liked that because it didn't promise anything revolutionary or anything I couldn't produce. I had rejected calling it "Ecosystems Analysis" since I felt it should have more synthesis than analysis. I had already used the term ecosystemology in that 1962 paper, 5 but I didn't press for giving the seminar that name. Since I was in the Forestry department, the seminar had a Forestry number (For 224) instead of the more appropriate Interdepartmental Study (IDS) designation, even though it was sponsored by the campus-wide Natural Resources Committee. The seminar was first offered in 1964. It was attended by forty graduate enrollees plus some twenty others who came as auditors. These sixty people represented 32 different majors. In the second year there were eighty students, and by the third year doctoral and masters candidates from 44 U.C.B. majors (as well as students from Stanford, U.C. Davis, and San Francisco State) had attended. The course was mentioned in the Muscatine Report 6 which was written as aftermath to the campus riots of that period. I continued to teach Forestry 224 until a year after my retirement in 1991. During these 27 years it emphasized (openly and strongly) holistic, interdisciplinary, and systems thinking. It was also the spawning ground for two fields brand new to the campus; namely, agroforestry and landscape ecology, both of which I introduced in 1982 and 1987, respectively.

While the Natural Resource Ecosystems seminar (which I informally called Ecosystemology) turned out to be very useful for graduate students, I had reason to believe it was too late for that level. The opportunity to offer an upper division undergraduate course by that name came in 1974; this was first IDS 110, then CRS 110, and now ESPM 164. Except for three years, when I was abroad on sabbatical leaves, the course was offered continuously from 1974 through 1999. During that time it was taken for credit or audited by all undergraduate levels, by occasional Masters and Doctoral candidates, and even by a few high school seniors..

A few additional comments should be made about the F224 graduate seminar. Its success signified the thirst that students had in the 1960's for interdisciplinary teaching. In fact, soon after, in response to this success, several colleges and departments on the campus created their own interdisciplinary courses . However, this was going in the wrong direction; it narrowed the range of interdisciplinarity. It was saying that the social sciences do not really need to include the biological fields to be interdisciplinary, or for the biological fields to say that they were already interdisciplinary enough. Indeed, something like this did happen some years earlier with the creation of the Biology Council and the establishment of a Teaching

Department in Biology This was praised as a far-out, revolutionary step: to integrate bacteriology, botany, physiology, and zoology, as if previously plants, animals, and bacteria had few if any connections with each other, or that protoplasm had been a term too vague to be useful. Most small colleges never have had separate departments of botany, zoology, etcetera, suggesting that their teaching efforts had been "interdisciplinary" from the very beginning. Three decades later, these separate departments at Berkeley were combined into one, the Department of Integrative Biology. And then, it seems, many on those faculties even began to think "integratively."

Starting in 1970 the Graduate Division created the ad hoc Interdiscip-linary Ph.D. Program. Candidates were to study under the supervision of a sponsoring committee of five faculty members representing up to five different departments. On an ad hoc basis, all course and dissertation requirements were set by this sponsoring committee. I chaired the committee for Dr. Norman Myers who was the first enrollee in the program. The ad hoc Interdisciplinary Ph. D. program was axed a few years ago, one ostensible reason being that it was too costly in terms of faculty time. This was in fact true: I devoted as much time to any one of my ad hoc students as I did for any four others of my Ph.D. students together in the Wildland Resource Science program.

During this period of my tenure, most of my Ph.D. students were in the Wildland Resource Science program, which by its nature was also interdisciplinary, but its requirements were more rigidly fixed than those of the ad hoc program. Here is where I had come to the conclusion that to start inter-disciplinary learning at a post-graduate level was too late. While most of the graduate students endorsed interdisciplinarity enthusiastically as an enlightened way to learn, none would adopt it for broadening the range of their dissertation research. It was too risky, considering that departmental requirements were usually less tolerant of far-ranging theses. Let me give an example.

Norman Myers, from Nairobi, Kenya, took his undergraduate degree in Scotland. He worked in game management in the national parks of East Africa, and among other things had become an expert on the ecology of the cheetah. In 1970 he came to Berkeley to get a Ph.D with Starker Leopold, the Professor of Wildlife Management in the Zoology Department. His intended research project involved the relation of the East African parks to their surroundings. He told Leopold that he wanted to include economics, geography, and forestry, as well as game management in his formal studies, and that he was not interested in taking animal behavior, physiology, or anatomy courses which were then required for all zoology students. Leopold told Norman he could do classwork in the 'periphery' fields but it would just take a few years longer to get his degree; he would still have to take all the required courses in zoology. I suggested that Norman consider the new ad hoc program. It suited his purposes well. His sponsoring committee consisted of Professors S.V. Ciriacy-Wantrup (Agricultural Economics), Bill Longhurst (Game Management), James J. Parsons (Geography), Dan Luten (Geography), and myself (Forestry). He completed the Ph.D. degree in 1973 in Conservation of Special Development Regions; his dissertation was titled "The relationship of parks and other protected areas to their environs in Massai-land, East Africa."

I had come to Berkeley's School of Forestry in 1949, having just gotten my Ph.D. in Botany (Plant Ecology and Soils) from the University of Nebraska. At Cal, up to 1958 I worked on research projects that were politically taboo for forest management in California and volatile within the university itself (e.g., prescribed burning in forests). Later I started to work in the pygmy forest on the Mendocino coast---a whole system study---which held very little attraction to forestry graduate students, and absolutely none for timber forestry interests. Simultaneously, I began the arctic tundra project--another whole system study.

And I began to read systems philosophy books and papers like crazy.

During the spring quarter and summer of 1969 the College of Agricultural Sciences decided, very belatedly, to get involved with the environmental science "fad." Fad is what many in Agricultural Economics and a few in Entomology thought and hoped it would be. A lower division course was planned for the fall quarter, with a new major to start in winter, 1970. The course was to be called "Man and His Environment" (IDS 10); the major would be Conservation of Natural Resources (CNR). I took the lead in developing the course; also, I taught the fall and winter quarters the first year, and the fall quarter only, the next three years (1970-1972). IDS 10 had primarily a guest-lecture format; during each of the terms, I gave only two 90 minute lectures. My teaching associates suggested that I had something more substantial to offer the students than what could be packed into two lectures per quarter. Thus, I stopped teaching IDS 10 and began to teach the new course, CNR 110, (later CRS 110 and since 1995, ESPM 164) which I immediately christened Ecosystemology.

I had been teaching an upper division Systems Ecology course in Forestry since the early 1960's (For. 123, 124). Its primary aim was to teach undergraduates (not only forestry students) how to conceive systems, how to model ecosystems, and how to research them. For this course I leaned heavily on experience and techniques I learned from my own research projects, namely, the arctic tundra ecosystem study and the Mendocino marine terraces/pygmy forest project. (I believe that my Forestry 124 systems ecology course, which I stopped teaching at retirement in 1991, was much like the current Ecosystem Ecology course now offered in ESPM.) Also, I had used this course as the "laboratory" for my earlier experiment station project on how better to teach ecology. But the way I taught it, and because of the too-narrow academic mileu in which it was offered, I felt that something very important was not coming through. "That something" is what I put into the CRS Ecosystemology course.

The British ecologist Tansley coined the term ecosystem in 1935. He argued that community is not the logical unit for ecological study because it does not include soil, rocks, air, and water--that is, abiotic matter--as the main objects of study.. Nevertheless, Tansley admitted that he had a biological bias in conceiving ecosystems. Raymond Lindeman (who was my lab instructor in a 1940 animal ecology class at the University of Minnesota), was the first to give a formal definition of ecosystem.6 Unfortunately, in his definition Lindeman did not include himself inside the ecosystem (even though he lost his life because he was too much inside it.7 Lindeman's analysis turned out to be more reductionist than ecosystemic.

The first ecology textbook to discuss the ecosystem concept in detail was E. P. Odum's Fundamentals of Ecology , first edition, 1951. My own major professor, J. E. Weaver, never mentioned the word 'ecosystem' in lectures or in any of his papers. After I came to Berkeley (in 1949), gradually I began to hear the concept being talked about but not yet as an object to be studied as a whole.

At Berkeley, no ecology grad students were studying ecosystems; no resource managers were thinking in terms of ecosystems,--and nobody else until I met the one professor who had been thinking 'ecosystems' for a long time, Hans Jenny. But Hans didn't know the term ecosystem; he had coined his own term: the larger system. H. Jenny introduced the larger system in his book Factors of Soil Formation (1941), six years after 'ecosystem' was coined by Tansley. I worked closely with Jenny for forty years. He may have had a pedological bias but it wasn't as strong as the biological bias shown by most of the plant and animal ecologists I have known, those who were "working within ecosystems."

When I first came to Berkeley I had many discussions with the ecologist in the Botany department, Herbert Mason, Herbert had less respect for the ecosystem concept than he had for the earlier community concept. "It's all in your head," he said, "and pretty well muddled. There's no such thing as an ecosystem." John Harper, a professor from University College of North Wales, exuded even more scorn. "People who study ecosystems are dumb; they don't know anything" is the way he saw it. And when E. P. Odum and other ecologists began to whoop up the idea in textbooks, and after the International Biological Program spent millions of dollars focusing its ten-year studies on ecosystems, Paul Colinvaux tried to quash its value by writing ". . . . now that the ecosystem fad has finally been put to rest, we can get on with studying ecology" (paraphrased a little bit ; I can't find the exact quotation). But it wasn't put to rest; the ecosystem concept finally began to thrive at Berkeley.

Meanwhile, here at Berkeley, my systems ecology course, Forestry 123, became F124; then it no longer was a required core course and most of the Forestry majors opted not to take it. My arctic study was criticized because it was done "in a 'foreign' country" and wasn't helpful for farmers in California. The pygmy forest study was interesting but thought not practical. I could not find any forestry graduate student willing to use it in thesis research. On two occasions I applied for a research grant from the California Division of Forestry (with Hans Jenny as collaborator) when the CDF had set aside some research money for studies in the Jackson State Forest. My own department chairman was one of the referees; he saw no value in our proposals. But my study continued---on very low budget. Thus, it was obvious that ecosystem study was deemed to be of low value; this, as late as 1980. Some time thereafter, Hal Salwasser, who as a graduate student in Wildlife Management had attended my ecosystems seminar (F224), published a paper in the Journal of Forestry, on "New Forestry." He advocated ecosystem-sized units for resource management. Soon after, Jerry Franklin, a professor from the University of Washington's forestry department, came to talk to our faculty and students about his holistic approach to management, but appreciation was lukewarm among many of the faculty,. After all that, it still took a while for ecosystem-gestation to happen.

Well, finally it did happen. At least, the term has caught on, so much so in fact, that today the term ecosystem has come to be almost a buzzword. Here, in our College of Natural Resources, we now find an organizational unit (division) called Ecosystem Sciences, depicted as containing several already well established fields, namely soils, hydrology, and biochemistry.8 While these old, well-established disciplines have separately contributed to management of resources for a long time, their integrated mission would seem to point to just one Ecosystem Science, not to several. A problem I have with many of the ecosystem studies that have been published is that the investigators use the ecosystem as a container. In other words, they may study processes, populations, or communities within an ecosystem , but not the ecosystem itself as a whole entity, with properties of its own, or in addition to properties of its parts. Most ecologists remain comfortable with the classical methodologies of population or community ecology.

Let me now return to the question, What is Ecosystemology? Remember, for me it is the name of a course that I was teaching at Berkeley, not the name of a discipline. I felt that there had to be at least one course on campus that would unabashedly promote holism--in thinking, in research, in play. I could think of no better model in which to teach this than the model of the whole ecosystem, an entity all of whose parts are interrelated. And since humans are integral parts of most ecosystems, so are their ideas and constructs. Raymond Lindeman's working definition of ecosystem took in only the physical parts (see footnote 6). One of my graduate students in the ad hoc Interdisciplinary program wrote his dissertation on the subject of interdisciplinarity. Sandy Elberg, who was

then the dean of the graduate division, said "No, no, Loren. You can't do that! You can treat a variety of ecological terms and ideas in an interdisciplinary way but interdisciplinarity per se cannot be a thesis in your field!". Loren thought Ecosystemology was his field.

Whenever I have a form to fill out to state what my field is, I have to say I'm an ecologist. My two advanced degrees are in ecology. I was hired at Cal as an ecologist. I taught ecology courses long before--and after--I began to teach Ecosystemology. The Ecosystemology course became my attempt to keep the integrated mission of CRS alive and working. Several years before I retired, the CRS panel, thinking ahead to the time that I would retire, considered searching for a replacement to teach the Ecosystemology course. One of the faculty members asked, "Should we be looking for a biological ecosystemologist or a social ecosystemologist ?" Oh my, I thought, this guy should be taking the Ecosystemology course. When the CRS program was first put together in 1969-70, we conceived it to be the much-needed integration of many fields: natural science, social science, and humanities. And indeed, at first that appeared to be achieved successfully. We had faculty from all the departments in the College of Agricultural Sciences (including Ag Economics), the School of Forestry, Civil and Electrical Engineering, Physics, Chemistry, Political Science, Geography, Puiblic Health, and several others, helping us teach the IDS 10, Man and his Environment course. But alas, it didn't last long. The first split came when the program was divided into two majors: CRS and PENR (now E.E.& P.). And soon after that, PENR was weaned away entirely from CRS and put into the ARE department. CRS itself was divided into biological "tracks" and social "tracks." My attempt to keep this from happening was to teach Ecosystemology. When my advisees who took Ecosystemology filled out their green sheet, they would ask me in which breadth requirement to list the course. I would tell them, just flip a coin, it's only a form; the way I visualized the course, it could be listed anywhere -- or everywhere.

This inability for our faculty to "think integration" reminded me of an earlier experience in the Forestry department. Before the School of Forestry had a Ph.D. program, students would either get their degrees in Agricultural Economics (for a social degree) or in Botany or Plant Physiology (for a biological degree). A later step had the Ph. D. in Forestry with a social or biological major and a biological or social minor (multidisciplinary). Next came the innovative bio-social analysis program. In a final step closer, the hyphen was removed, and there transformation ended.

Briefly I saw some hope that the rest of the College of Natural Resources would "see the light," and act on it. An article in Harper's Monthly magazine called for the design of a "new academy" and an end to division by department, written by Frederick Turner.9 The article caught the attention of Professor Al Weinhold who sent copies of it to the department chairpeople in the College, including Joe McBride in Forestry, who in turn sent it to me. Weinhold once chaired the CRS department. Turner, who is a poet, wrote that the recent great advances that have been made in all the intellectual disciplines (natural sciences, social sciences, and the humanities) have begun to tear down the barriers and distinctions between those disciplines, but in our universities and colleges, the result has been nothing but further fragmentation and specialization. A new book (published in 1998) by Edward O. Wilson, titled Consilience 10, devotes 298 pages to this same kind of message. Maybe Wilson's stature will help to get the idea across to "the academy."

I have been challenged to justify my putting too much (i.e., the whole universe) into what I think ecosystem is. In order to justify the course title Ecosystemology, the concept of ecosystem needed to be revised. I had no qualms about "improving" Tansley's definition of the term. Anthropology had already made such a revision.11 I feel that one reason the ecosystem concept had been slow to catch on was because ecology itself was a slowly accepted discipline before

1970. If the concept had been pushed by philosophy (as wholeness), by systems theory (through modeling and design), and by methodologies (from other fields) instead of only from ecology and management, it would have been a broader idea from the start. Figure 1 on the next page shows that ecological "seedbeds" are not the only ones appropriate for generating the concept ecosystem. Essentially, this is why ecosystemology is more than ecology.

Finally, let me return to the question that I posed at the beginning of this paper, does the coiner of a term own it? I said it involves the term ecosystemology. I was wrong to add '…. and its definition when it first was coined.' The truth is that I have never had a definition, at least not a concise, textbookish, one sentence one. There is no short answer to the titled question, What is Ecosystemology? The long, and I think the best, written answer is contained in the Ecosystemology Reader, all 250 pages of it. Of course, most of my students know the answer to the question but they find it difficult to explain when their parents and querulous roommates ask "What in the world is it?"

I invented the word ecosystemology in 1960. To my knowledge it had not been used before by anyone else. At that time I was contemplating revising

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_____ 1 I say "reputed" because the first use of ecology has been attributed to another person and dated at least ten years before Haeckel used the term. This person was none other than Henry David Thoreau who in a hand-written letter (no typos!) to his aunt in Concord inserted the word "ecology." The letter was dated 1858. The sentence in which the word appears was about a meeting Thoreau attended. This implies that there may already have been an ecological society at that time. After only a little scrutiny, the claim for Thoreau was repudiated. The word as written in his letter looks more like geology than like ecology - and it was geology, by golly. Ah, but you know how environmentalists are, how loosely they interpret things!

2 Recall books entitled "Steps to the Ecology of Mind." "The Ecology of Politics," "The Ecology of Commerce," and "The Ecology of Fear," among others with lesser biological flavor.

3 Rowe, J.S., and B. V. Barnes. 1994. Geo-cosystems and bio-ecosystems. Ecological Society of America Bulletin: 75:40-41

4 The Ecosystem as a Conceptual Tool in the Management of Natural Resources by Arnold M. Schultz in Natural Resources: Quality and Quantity, edited by S, V. Ciriacy-Wantrup and J. S. Parsons, University of California Press, 1967. The book was published five years after the paper had been presented.

5 Ecosystemology was a highly tentative proposal; in an earlier paper I had proposed "ecosystematics" and in whimsier moments had toyed with "ecosystemantics." I felt that the name should be as short as "botany" and as forceful as "physics."

6 Education at Berkeley, report of the Select Committee on Education (also called the Muscatine Committee) 1967. See page 164.

6 "The ecosystem (is) the system composed of physical-chemical-biological processes active within a space-time unit of any magnitude." What, no human processes?

7 Lindeman's research involved day and night surveillance of a lake in the middle of winter in Minnesota. He died in 1940. His paper was published posthumously.

8 Ecosystem Sciences, A report prepared for the College of Natural Resources Conference on Excellence, by Professors, Harte, Matson, and Sposito, August 1995.

9 Frederick Turner Design for a new academy Harper's Sept. 1986, pp 47-53 Frederick Turner is Founders Professor of Arts and Humanities, University of Texas.

10 Edward O. Wilson. Consilience, the Unity of Knowledge. 1998. Alfred A. Knopf, New York