

G. William Skinner as an Ecological Thinker

Stevan Harrell, March 2010

One of my many vivid memories of being dressed-down by G. William Skinner when I was his graduate student comes from when I was writing my doctoral dissertation, on individual differences in religious belief in a Taiwanese village. I had presented to Skinner a draft of part of a chapter that tried out the idea that these individual differences were not directly correlated with any kinds of independent variables such as a sociologist might use to do what sociologists rather misleadingly call “explain” the variations—age, sex, education, occupation, family members beliefs, and so on. I had posited that perhaps these differences were the result of individual personality characteristics that could not be measured statistically. After reading the chapter, he called me into his office, held my draft in his hands, and looked down his long nose at me over the top edge of the reading glasses he sometimes wore. “I am one of those people who believes the world is not random,” said he, “now you go home and find some correlations.”

“One of those people who believes the world is not random” might be a nice definition of the term “scientist.” And there is no doubt that Skinner always considered himself, and that others considered him, a social *scientist*. But what kind of a scientist was he? Despite his admonition to “go home and find some correlations,” he was not the sort of scientist who thought that single variables “explained” things, that the world he studied—the world of human society and particularly culture—a point I will return to—could be explained by any number of variables, no matter how many asterisks one could put next to the F-values in a multiple higher-order correlation matrix. . He was not, in fact, very interested in explanations by cause and effect, as perhaps again might be indicated by his telling me to find some *correlations*, not *explanatory variables*. He was not interested in what my *xuexiong* Steve Sangren, in a recent paper on Skinner’s legacy, called in his polemical tone reminiscent of the master himself, the “Misleading reductionist epistemologies, including rational-action theory, evolutionary psychology, and the grandiose claims of some geneticists to link all human behaviors directly to genetic endowments, [which] remain a force in contemporary social science.” In other words, he was not a physicist of society, and was totally devoid of what has derisively been called “physics envy,” in the softer, i.e. more difficult, life and social sciences.

If He was not a physics wannabe, not reductionist, not about cause and effect. then, what kind of scientist? I would submit that Skinner was an ecologist, interested not in unidirectional causality but in the way things, in his case social and cultural things, fit together and interacted in *systems*. We all know that Skinner was interested in hierarchies of nested social systems, and in how these systems changed over a complex pattern of cyclical and linear time, what Sangren, again, has called “a sort of Cartesian/spatial-cum-temporal framework for understanding a whole spectrum of social and cultural phenomena not generally approached in these terms.” In saying this I might as well be reciting the Apostles’ Creed—why am I

taking time with this. Because I, like Sangren, am interested in Skinner's contributions to the metatheory or philosophy of science, contributions that can help us both bridge the "two cultures divide" between science and humanities and get us out of a conceptual and philosophical swamp that anthropology has recently steered itself into in an ironically dichotomous detour away from its original empirical path. Furthermore, Skinner's approach to explanation not only has value in its own right, but also because its approach to social and cultural systems is closely parallel to ecology's approaches to natural systems, we can use some recent insights in ecology itself to improve on Skinner's approaches and models, and to fix what I see as one of their most important flaws.

The relationship of Skinner to ecology, in the formal sense of the study of the interaction among various biophysical units in a living landscape, is a complex one. I seem to remember him talking about ecosystems repeatedly, but an admittedly cursory search of his published works reveals only one use of "ecological," referring to soil transfer through erosion from the periphery to the core of a local system, and one of "ecosystem," in the famous passage where he states that Martin Yang invented the people's commune *ex ante*. And certainly there is nothing remotely resembling environmental or ecological determinism in Skinner—unlike the cultural ecologists whose work was in vogue, particularly under the leadership of Julian Steward at Columbia when Skinner was a graduate student, he never explained culture as determined by ecological variables or adaptation to a natural environment. It is not in the content of the thing studied that Skinner resembles an ecologist, but rather in the manner of studying it. It's important to realize, however, that Skinner was not particularly interested in the content of ecology. Instead, he thought like an ecologist. Rather than a cultural ecologist, I think we might characterize Skinner as an ecologist of culture.

And it is culture that Skinner was interested in studying; he always said in his classes that understanding culture was the whole point. This may come as a surprise to those who see very little about culture in his writings, full of nested hierarchies of natural systems, discrete hierarchies of administrative systems, cycles, metacycles, regional and sub-regional cycles. There is little about what the Chinese did in these nests or in these cycles. I can think of three things that explain the gap between Skinner's professed interest in culture and the relative lack of culture in his writings. First, the lack is not absolute. Ever since his first fieldwork in Sichuan, but especially since his third fieldwork with the hyper-fertile Chinese in Java, Skinner was interested in family process and the content of relationships of power in Chinese families, a subject that you will be hearing of shortly [or just heard about] from Bill Lavelly. This is cultural content without any question. Second, and this is speculative on my part, I think Skinner was a mirror image of Boas in this regard. Boas's ultimate quest was to *explain* the spread, diffusion, and historical change in cultural traits. But to the end of his life, he never thought we had enough empirical evidence to start putting together more than speculative models of patterns, and Boas, trained originally as a physicist, did not trust speculative models based on insufficient empirical evidence. Skinner's ultimate quest (one of them

anyway) was to explain, in the way that ecologists explain things, culture, but because he was devoted to the ecological model of explanation, he had to have the model right—the local systems mapped and quantified, the cycles placed historically in the right temporal relationships to each other—before he could fill in the cultural content, and to the end of his life he was still figuring out how the systems went together. Third, I have just been reading Skinner's notes from his fieldwork in Sichuan in the fall of 1949 (yes, they exist). And I find the most careful and minute descriptions of cultural things, from how to make a charcoal warmer out of a rough clay pot and some strips of split bamboo to what mothers do to their toddlers when they won't quit bawling—coddle and comfort them for awhile and if that doesn't work, bop them on the bean with the nearest available chopsticks. I know that Skinner spoke admiringly of a lot of interpretive cultural anthropology. But he wanted to study culture in his way, and he was only beginning on the project.

But even if Skinner only very incompletely realized his own approach to culture, I think that this approach could help anthropology get out of the self-flagellating and quite possibly self-defeating mood that it has been in for the past 25 years or so. We used to study cultures (the -s on the end is very significant here), and the premise of anthropology's first hundred years or so was that every society had its culture and each society's culture was different from every other, and since the time of Boas no culture represented any moral superiority over any other, but each needed to be studied and appreciated on its own terms. Since 1985 or so, to talk about cultures has been pretty much taboo in anthropology. One facetious reason is that the papers started picking up the term, culture came into the rainbow curricula of our multi-cultural schools, we had the culture wars—in other words, culture moved from being a specialist word that anthropologists had to explain to being a word in the general English vocabulary, and so in Bourdieuvian terms it no longer carried any intellectual capital for the elite. But more seriously, anthropologists began to discover—*mirabile dictu*—that cultures were not really isolated, that the influence they had on each other was not some newfangled thing of the age of discovery or the age of globalization, but that there always had been boundary crossings, or more accurately there never were very sharp boundaries. The reaction of much of anthropology to this rather “duh!” realization, coming at the same time as the self-critique of anthropology for having been involved in colonialism and imperialism, was to deny the possibility of doing anything scientific about culture—it was impossible to classify, compare, explain. All these were not only otherizing, they were empirically wrong. So But now every graduate student has to memorize the line in today's Apostles' creed that says “cultures cannot be studied as bounded entities,” as if “bounded entities” were actually an acceptable English phrase. All we were left with was hybridity, fluidity, and the de-localization of culture

Skinner gets us out of this silliness. We can start with his fieldwork near Chengdu in 1949. There is his famous factoid of seeming cocktail-party irrelevancy—that young women on the Chengdu Plain in 1949 cross-stitched their trousseaux with patterns that varied from one standard marketing community to

the next. And I am struck in reading his notes by the part that the English language played even in the market town of Gaodianzi—mahjongg-playing loafers in the town had studied it a bit in school, and Skinner became famous and in great demand for being able to write people's names out in Romanization. In other words, the purely local and the undeniably global were all part of a pattern of boundary-making and boundary crossing, and as Sangren rightly points out in his paper, Skinner saw a temporal pattern of cycling back and forth between periods of relatively sharp and relatively fluid boundaries. Cultures were never "bounded entities," and G. William Skinner knew this in the 1940s—that it took so long for anthropology in general to realize this has more to do with the neuroses of our outlook and the politics of tenure-track point-scoring than it does with either the facts of culture or what was being written about it. As Sangren so rightly observes, "the penchant of some contemporary theorists of globalization to suppose that the world has moved recently along a trajectory from autonomous, bounded, local cultures toward de-territorialized, trans-national, global forms" is simplistic.

To show just how ecological Skinner's manner of thinking really was, but also to admit to some of the holes in his model and how they might be patched, it is useful to talk explicitly about ecosystems ecology (yes, there are other kinds) and specifically about the models that are now known as "resilience theory" or resilience ecology. These models, developed initially by University of Florida ecologist C. S. Holling (Skinner's age-mate—check) and now increasingly applied to socio-ecological and occasionally just social systems, bear striking resemblance to Skinner's combined spatial and temporal structuring. The basic premise of resilience ecology is that ecosystems do not, as previous theories held, have a natural state of equilibrium to which they revert in the absence of continual disturbance. Ecosystems are always changing, and although they have cycles, they rarely come around to the same state in successive cycles. Rather any system in the absence of major disturbance will go through a process of increasing productivity and increasing connectivity that can be graphed on a logistic growth curve, which by the way was one of Skinner's favorite geometric metaphors outside the hexagon. As a logistic model would lead us to expect, a system's productivity growth will eventually be slowed by the necessity to put more and more energy into the maintenance of the system, and when this happens, the resilience of the system, its ability to withstand disturbance and retain its basic functioning, becomes reduced. A smaller and smaller magnitude of disturbance is required to collapse the system, to send it into disorganization and chaos, out of which, however, it eventually reorganizes. Depending on the processes happening in the phase of reorganization, it may end up in a very different state from the one it started out in—for example, a forest may become a grassland on which new trees do not grow, or a clear lake may become a cloudy, eutrophied mess of blue-green algae that suck up all the oxygen and don't allow fish to live. Or the next cycle may be more or less like the last one.

As in Skinner's model, cycling systems are tied together in a nested hierarchy, which Holling and his colleagues have famously called a "panarchy," and the different levels in the panarchy influence each other. A disturbance originating

at a higher level may send a lower-level system into a reorganization phase, but if the higher level itself resists reorganization, the lower-level system is likely to reorganize in more or less the same manner. And a series of reorganizations or collapses at lower levels may send the higher-level system into a reorganization phase, out of which it, too, may emerge in an alternative stable state.

A hierarchy of nested local systems cycling through time in and out of phase with each other—there seems to be little that is different between Skinner’s model and that of the resilience ecologists. But perhaps it is fitting to close with another remark I remember Skinner making to me in 1969 or so, right after the 9th party congress, when I asked him if the compliance cycle that he and Ed Winckler had described so provocatively in their article was coming back to a remunerative phase after an extraordinarily fierce and tenacious one. Since this was at a party rather than in his office, he was not so stern with my silly question as he had been with my silly assertion earlier. “Of course,” he said, “what goes up must come down.”

This is where I think resilience ecology and resilience concepts could help fill out the Skinner spatio-temporal model. He would, of course, never have tried to maintain that cycling was always as smooth as the curves he drew in the Skinner-Winckler piece on compliance succession or in the magisterial AAS presidential address on The Structure of Chinese History. He never would have discounted the importance of discontinuity. But his models didn’t really have a systemic way of dealing with it. If we plug them in from resilience ecology and use the adaptive cycle as our model of the cycle, then perhaps the model will come closer to the perfection that was always just a little bit out of reach for G. William Skinner.