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*Organization Environment* 2008; 21; 471
DOI: 10.1177/1086026608328870

The online version of this article can be found at:
http://oae.sagepub.com/cgi/content/abstract/21/4/471

Published by:
SAGE
http://www.sagepublications.com

**Organization & Environment**
Volume 21 Number 4
December 2008 471-477

10.1177/1086026608328870
http://oae.sagepub.com
hosted at
http://online.sagepub.com

# A Retrospective View of My Development as an Environmental Sociologist

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Initially hoping just to contribute increments of useful knowledge by genuinely scientific social research, this formerly "mainstream" sociologist was alerted to environmental concerns by encounters with urban growth, burgeoning university enrollment, and impacts of overuse in wildland recreation areas. Enlightening research collaboration with foresters, exposure to ecological themes in interpretive exhibits at National Parks in New Zealand and North America, and encounters with significant literature in biological sciences contradicted conventional notions that human societies are somehow exempt from influences by non-social aspects of the ecosystems of which they are part. It became apparent that biogeochemical processes and other extra-societal variables must be taken into account to understand humanity's ines-capable adjustments to carrying capacity deficit after an era of thriving on a carrying capacity surplus.

***Keywords:*** *ambivalence; succession; ecosystem; carrying capacity; paradigm shift*

## In the Beginning

For the first few years of my professional career, I was not an "environmental sociologist." Examples of my work as a "mainstream" sociologist would include such papers as my first *American Sociological Review* contribution (Catton, 1954) and a report in *Social Forces* about an attitude change experiment (Catton, 1960). In my chapter on "The Development of Sociological Thought" in Robert Faris's *Handbook of Modern Sociology* (Catton, 1964), there was still nothing to identify me as focusing on environmental topics.

My first book, *From Animistic to Naturalistic Sociology* (Catton, 1966), did contain some of my findings about patterns of travel to national parks, and by then I was involved in research with John Hendee, a U.S. Forest Service forest ranger pursuing graduate studies with Frank Brockman, a former national parks naturalist become professor of forestry at the University of Washington. We applied attitude scaling techniques to studying the management preferences of wildland recreationists. My own and my family's recreational interests had taken us to many national parks in the United States and Canada and had begun acquainting me increasingly with ecological principles as conveyed by museum exhibits in their visitor centers. The experience of "congestion" in campgrounds and on back country trails increasingly sensitized me to some of the social effects of population pressure.

## A Special Intention

From graduate school days I did have a special desire to contribute to pushing sociology toward becoming more truly scientific. Some therefore probably considered me a "neopositivist" for I tended to be impatient with advocates of so-called "qualitative method" and was especially resistive to the occasional insistence by some sociologists that sociology's subject matter was unsuited to study by methods akin to the way new knowledge is sought in the "hard sciences."

I admired an article published in 1955 in the *American Journal of Sociology* by George A. Lundberg, explaining what he saw as "The Natural Science Trend in Sociology," and I ardently believed such a trend was (or would be) a good thing. It would enable sociology to become ever more useful in guiding human societies toward a better future. I cited that view in an article I wrote for *Sociological Inquiry* (Catton, 1964b) about sociologists' ambivalence toward scientific standards. Invoking the sociological concept of "the marginal man"—a person caught in the conflicting demands of two cultures—I probed an issue expressed by the British scientist and novelist C. P. Snow. In his 1959 Rede Lecture at Cambridge University, Snow had expressed resentment that humanities scholars, who complain of scientists' unfamiliarity with finer points of literary and historical knowledge, were themselves too commonly quite "illiterate" in regard to basic scientific principles. Such principles, Snow insisted, were fundamental to understanding the modern world and the social systems it contains. My article examined what I took to be ambivalence in a sociologist (Robert MacIver) influenced by those two cultures. MacIver had early questioned whether the "social forces" that cause patterns of social behavior were "really" *forces* and whether "social causation" was logically similar to, or fundamentally different from, cause–effect links among nonsocial phenomena. Looked at chronologically, I thought his writings showed a trend—away from his youthful antiscientific predisposition and toward resolving the issue in his later years somewhat closer to the way Snow (and Lundberg) would have wished. Perhaps I was doing wishful thinking, but MacIver's writings seemed to me to typify an *ambivalence* characteristic among many mainstream sociologists.

## Transition Begins

That same year an article I wrote about problems arising from excess visitation in formerly pristine scenic areas was published in *National Parks Magazine* (Catton, 1964a). It was followed by some articles I wrote for sociological journals that fitted a mathematical model to state-by-state frequencies of visitors to U.S. national parks. In 1968 I wrote an article for the *Journal of Forestry* giving a sociologist's view of then-current proposals for establishing a new North Cascades National Park, and later that year *American Forests* published an article I coauthored with John Hendee on the desires and expectations of recreational users of wilderness areas. That same year the U.S. Forest Service published a 92-page research paper by Hendee, myself, Larry Marlow (another forestry student), and Prof. Brockman about Pacific Northwest wilderness users. But I was not yet really thinking ecologically, still writing essentially from a social psychology perspective.

Early in 1970, discouraged by effects of the overgrowth of the University of Washington, where enrollment had doubled since I first came to that institution, and disheartened by

adverse social effects of population increase in the Puget Sound region, I resigned from the UW sociology faculty and moved my family to Christchurch, New Zealand, an environment significantly resembling an earlier (less populous) version of western Washington. There at the University of Canterbury I taught courses in sociology for the next 3 years, including a graduate seminar in social change, and I pioneered that university's first course in human ecology. As Canterbury's first professor of sociology (though not the first person there to teach sociology courses), I was again associating with foresters, both an ex-forester grad student in sociology and two professors in the School of Forestry. When I was invited to lecture at a conference of New Zealand national park rangers, I became well acquainted with the top administrators in that country's national park system.

## The "Aha!" Experience

Our family's recreational activities had transferred readily from national parks in North America to those in New Zealand. An eloquent exhibit in a visitor center in Westland National Park challenged my prior notions about "succession," a kind of ecological transformation too long misrepresented in sociological literature as an invader-driven process. A tabletop representation of the successive transformation of biotic communities that develop in a valley just downstream from a receding glacier conveyed the idea that pioneer species—the first vegetation to grow on land recently vacated by retreating ice—by using that newly available habitat *so alter* it that it becomes more suitable for their successors than for their own continued occupancy. This "revelation" stimulated me to read more biological literature and concentrate less on the writings of sociologists.

Meanwhile, my wife had been working as a volunteer in a branch public library in the city of Christchurch, and while browsing there one day as I awaited the end of her shift, I happened onto a book by a pair of British authors that contributed to my views on the social repercussions of population pressure: *Violence, Monkeys, and Man* by Claire and W. M. S. Russell of the University of Reading. Their primate research showed how behavior becomes less relaxed and more stressful when population exceeds resources, including space. This led them to a concept of "population crisis." Together with my aversion to the local impacts of rising population pressure in the Pacific Northwest which had precipitated our family's migration from the United States to New Zealand, this book reinforced my decision to read increasingly among the writings of biologists about ecological topics.

So during the 3 years we lived in New Zealand, papers I wrote increasingly reflected this change of focus and my growing conviction that sociology needed to pay more attention to some of the biogeochemical processes and other factors traditionally deemed nonsocial and "irrelevant" to sociology. In short, my desire to put sociology on an ever-more-sound scientific footing was becoming focused on wanting us to take account of causal variables we traditionally ignored as not sociological enough. Such a change of perspective was implicit in articles I submitted from New Zealand to American journals (Catton, 1971, 1972). Most especially, however, the paradigm shift I was undergoing shaped the writing I began in New Zealand on *Overshoot: The Ecological Basis of Revolutionary Change* (Catton, 1980)—a book I completed after we had returned to North America and relocated ourselves at the state of Washington's other major university—Washington State University, away from urban

pressures. In it I took account of an important volume on *Plant Succession* that was published in 1916 by Frederic Clements, who had recognized that the kind of biotic community able to thrive in a given habitat depends on how the characteristics of that environment act on the community of organisms. He also clearly recognized the kind of succession I saw portrayed in that New Zealand national park exhibit. Organisms, in the process of living, react on their habitat, he said. Plants change the soil in which they grow. Tall plants provide shade, which makes the site suitable for shade-tolerant species that could not have grown there otherwise. Plants requiring moist conditions may thrive only after other plants have established a moisture-holding ground cover.

Clements had seen, and I was now learning, how community–habitat interaction was at the heart of the process of succession. Early seral stages "pave the way" for their own replacement by different community types in later seral stages.

A few sociologists (such as Robert E. Park, of the University of Chicago) had quickly begun to apply some of the ideas from Clements and other ecologists in their efforts to understand human experience. But although they talked of succession, most of them missed this key insight into its nature. Park's colleague Ernest W. Burgess, and students of Burgess, came to regard succession in human communities essentially as a process of aggression: Invaders were imagined to be succession's driving force, *pushing out* prior occupants who supposedly might otherwise have thrived forever on a given site. By the time I came back from New Zealand and joined the Washington State University sociology department in the Palouse region of eastern Washington, I was aware that among sociologists who called themselves "human ecologists" most seemed oblivious to the important fact that *occupants of a site may make it unsuitable for themselves* after a time by the use they have made of it.

This idea was incompatible with what I had begun to call the "culture of exuberance," a culture based on the historical period of exuberant expansion of "Old World" people into a "New World" having what they perceived as underused opportunities. Sociologists who had pioneered a specialty they began calling "human ecology" did not sufficiently grasp fundamental ecological principles, I now realized.

## Generalizing the Insight

It was clear to me now that if a national park could be damaged by overuse, so could a continent—or even a whole planet. I was beginning to see the enormous relevance of such ecological concepts as *sere, seral stages, reaction, succession, climax, food chain, trophic level, dominance*—and especially *carrying capacity*—for a thoroughly nonlocal sociology. So in my book *Overshoot* I wrote,

> The idea that man's dominance of a world ecosystem might be only a pre-climax stage in a sere with other stages to come (not dominated by man) was quite alien to the culture of exuberance. The idea that industrial man's impact upon his habitat might make it unsuitable for industrial man clashed with the prevalent idea that human control over nature was a great achievement in the exploitation of limitless resources. (Catton, 1980, p. 123)

The book was favorably reviewed in a number of journals of forestry, biology, and so on, but social science reviewers often failed to see what such an apolitical treatise could possibly mean by "revolutionary" change.

It took a considerable time in the latter 1970s to find a reputable book publisher who did not suppose "the ecology market" was "already saturated," so several of my articles reflecting my new orientation were published before *Overshoot* got into print. These articles included two that especially announced my own change of focus—(Catton, 1976a, 1976c). That same year I also published two articles explicitly urging my fellow sociologists to open their thinking to a genuinely ecological perspective (Catton, 1976b, 1976d). Two earlier articles (Catton, 1973, 1975) had been transitional.

## Academic Symbiosis

Thus, by the time my family felt obliged to return to the United States, I was ready to benefit from mutually stimulating interactions with new colleagues at Washington State University, foremost among them Riley Dunlap, whose environmental outlook had an equally cogent but usefully different history. In 1978 there appeared the first of a series of collaborative articles I coauthored with this amazingly energetic and insightful cothinker (Catton & Dunlap, 1978). It was apparently destined to be influential as it was reprinted a number of times in various outlets and was quickly followed by two more widely cited pieces (Catton & Dunlap, 1980; Dunlap & Catton, 1979). Each article he and I wrote seemed to spawn another. Our productive collaboration continued for a number of years.

From about 1980 onward, my writing, either solo or in tandem, has sought to spread awareness of the urgent need for everyone, including sociologists, to recognize that our lifestyles, mores, institutions, patterns of interaction, values, and expectations are shaped by a cultural heritage that was formed in a time when carrying capacity exceeded the human load. A cultural heritage can outlast the conditions that produced it. That *carrying capacity surplus* is gone now, eroded both by population increase and immense technological enlargement of per capita resource appetites and environmental impacts. Human life is now being lived in an era of deepening *carrying capacity deficit*. All of the familiar aspects of human societal life are under compelling pressure to change in this new era when the load increasingly exceeds the carrying capacities of many local regions—and of a finite planet. Social disorganization, friction, demoralization, and conflict will escalate.

## Misgivings and Expectations

I undertook to write up these reminiscences somewhat reluctantly, feeling that it was essentially sterile to engage in debate over whether (a) Durkheim had ever truly banned consideration of biophysical environmental variables as socially influential, or (b) Dunlap and/or I had explicitly blamed sociological myopia on Durkheim for promulgating an antireductionist taboo, or (c) whether such writers as Marx with his emphasis on economic facts, Homans with his emphasis on individuals' rational actions, and Garfinkel who stressed "a transitory emergence of negotiated rituals" were already significant evaders of such a taboo (see Rosa & Richter, 2008, p. 183). Such essays may be fun for some to contemplate, but I was skeptical they can really shed light on the human prospect—apart from delineating other ideas that distracted us.

Monumental social changes (and troubles) in the 21st century will be misunderstood (and thus worsened, I believe) insofar as people (particularly sociologists, but also others)

continue interpreting events according to a worldview that insufficiently recognizes human society's ultimate dependence on its ecosystem context.

There will continue to be many issues on which sociologists working within the old Human Exemptionalist Paradigm can continue to shed significant light. But I am convinced many of us need to work from a New Ecological Paradigm if we are to comprehend the most urgent present and future trends and problems. Without the NEP, sociologists would provide inadequate guidance to decision makers and our fellow citizens.

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