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Promoting a Paradigm Change

Reflections on Early Contributions to Environmental Sociology

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The author discusses his collaboration with William Catton that led to several early articles aimed at providing an intellectual foundation for a field of environmental sociology. The differing backgrounds and interests they each brought to their collaboration and the context in which it developed are outlined, along with the author's assessment of the major goals of their key publications. The growth of environmental sociology and increased disciplinary attention to ecological problems, spurred by the growing societal salience of such problems, suggests that sociology has begun to shed the human exemptionalist paradigm that dominated the discipline when the field of environmental sociology was launched.

Keywords: *environmental sociology; new environmental paradigm; human exemptionalism paradigm; HEP; NEP*

Collaborations can be difficult, frustrating, and disappointing, but they can also be very rewarding. The best ones reflect the synergy created by two (or more) colleagues with quite different backgrounds, strengths, and interests coming together and creating something that neither would have produced on their own. This is definitely the case for my collaboration with Bill Catton, particularly the early series of articles we wrote in the late 1970s that the editors have kindly chosen as the subject of this symposium. Of course, it also helps to be in the right place at the right time, and our timing was ideal.

Background

I arrived at the University of Oregon intending to become a political sociologist, but became interested in environmental issues in the fall of 1969 when Eugene experienced severe air pollution because of grass-seed farmers' burning their fields after harvest during an inversion typical of the Willamette Valley. The ensuing controversy illustrated that environmental protection was going to be inherently conflictual, particularly because it was seen as challenging fundamental values such as private property rights as well as economic interests, but I was wrapping up my thesis on student political activists at Oregon and couldn't mount a study. An interest in environmental issues was planted, nonetheless, and I found myself becoming even more interested months later when momentum for the first Earth Day began to build on campus. One morning I woke up wondering, "Who are the eco-activists—Leftists with a new cause or a new group of activists?" I had befriended Richard Gale, a young faculty member deeply interested in environmental issues, and together we

conducted a small study of "eco-activists" that launched my career as an environmental researcher (Dunlap & Gale, 1972).

I cannot say it began my career as an "environmental sociologist" because at the time no such field existed, so I listed "Man-Environment Relations" (MER was a popular label for early social science work on the environment) as an area of interest on my vita when going on the market a couple years later. I was hired at Washington State University as a political sociologist, but WSU's Department of Rural Sociology was attracted by my environmental interests and offered me a part-time research appointment soon after I accepted a position in the Department of Sociology.

When I arrived at WSU in fall 1972, I was still working on my dissertation examining partisan differences in proenvironment voting in the Oregon Legislature (later published as Dunlap & Gale, 1974) and thinking of myself primarily as a political sociologist. At the same time I was also becoming involved in a network of sociologists (informally led by Denton Morrison of Michigan State) and political scientists interested in environmentalism and environmental politics, and my environmental interests were reinforced by the Department of Rural Sociology—especially when Don Dillman became chair during my second year. The result is that when Catton joined WSU in fall 1973, we quickly realized we were both convinced that environmental problems were serious matters, were not going away, and deserved (and required) sociological investigation.

For the first couple of years we just talked, but in retrospect I realize that those conversations were vital to strengthening my commitment to environmental research. Although I had been warned about the dangers of tying my career to a "bandwagon" by the chair of sociology ("What will happen when the wheels come off and environmental issues disappear?" he cautioned during an annual review session), I now see that having the support of a highly respected senior colleague like Catton in sociology as well as the encouragement of Dillman in rural sociology put me at ease about tenure and helped me follow my gut instinct that the environmental bandwagon would only pick up momentum.

As happened to Catton a little earlier, my personal paradigm was undergoing a major shift, stimulated by my conversations with him; reading the likes of Commoner, Ehlrich and Hardin; and experiencing the 1973-1974 energy shortage that seemed to validate the thesis of *Limits to Growth* (Meadows, Meadows, Randers, & Behrens, 1972). It took a while to overcome my strongly utilitarian perspective on natural resources, a result of being raised in a Northern California timber and fishing community. But by 1975 I was definitely seeing the world differently, and this reinforced my commitment to environmental scholarship.

The Collaboration

I still recall the official beginning of our collaboration, sometime in 1975, when I showed Catton a little diagram in which I had classified all of the arguments I had found about the "primary cause" of environmental problems into five categories: population, technology, cultural system, social system, and personality system (see Dunlap & Catton, 1979b, 1983). Catton immediately responded, "You've got an expansion of the POET model,"[1] to which I replied, "What's that?" I had foolishly failed to take Walter Martin's course in human ecology at Oregon and knew little of the field, but with Catton's guidance began to delve into it with vigor. Our conversations became more focused now that we had concrete issues and tools to discuss.

In the meantime, our professional context was changing rapidly. As an ambitious young faculty member and natural networker, and with the encouragement of James Short, I had initiated the formation of an Environmental Problems Division within the Society for the Study of Social Problems and served as its founding chair from 1973 to 1975. Catton in turn was appointed to an American Sociological Association (ASA) Ad Hoc Committee charged with developing guidelines for sociological contributions to environmental impact assessments, which the chair C.P. Wolf used as something of a Trojan Horse to promote a broader area of "environmental sociology." Wolf succeeded, as in 1975 his petition to form an ASA Section on Environmental Sociology, which Catton and I and six others endorsed, was approved (Dunlap, 2001). The following year the section was officially established after quickly garnering 200 dues-paying members, and Catton (a rare "environmental type" with national visibility) was elected the first chair and I was elected to the council.

I was delighted to see "environmental sociology" being treated as a field of study. I had shed the sexist MER label and with the enthusiasm of a new convert I was eager to become an environmental sociologist. There was only one problem—I didn't know what that would involve! Even though we were forming an ASA section, it was quite unclear if there was a meaningful field of study or just a growing group of people with interests—quite diverse at that—in environmental issues. Answering that question, and in the process helping define and legitimate the field, became a personal quest and gave renewed impetus to collaborating with Catton.

Catton was charged with organizing a session for the 1976 meeting, and he reluctantly agreed that we could include a joint paper in a session he would chair. Getting it on the program was the easy part, writing the paper proved difficult. We were exploring how a field of environmental sociology could build on sociological human ecology but concluded that human ecologists had failed to deal meaningfully with the physical environment, so our title was "Environmental Sociology: Why Not Human Ecology?" The title also implicitly acknowledged a difference between us, as I was eager to help develop a new field whereas Catton was torn between getting human ecology "back on track" and establishing environmental sociology. Being cast into a leadership role may have tipped the scales, as Catton soon became thoroughly committed to environmental sociology. Alas, the paper was never really finished, as I procrastinated until summer, and when I was finally ready to write Catton was departing for his usual summer hiking vacation (producing one of those "frustrating" aspects of collaboration!). Nonetheless, the presentation contained the seeds of our subsequent papers.

Goals

Looking back it appears that our collaboration involved a series of evolving goals. Certainly for me, the first priority was to "define" and help legitimate a field of environmental sociology. I was keenly aware that many members of the new section were studying public opinion, environmental activists, movement organizations, governmental policy making, and the like—all topics that could easily be dealt with by traditional areas of our discipline. At the same time most major areas were defined by the phenomena they investigated, with political sociology, for example, being demarcated by its focus on political phenomena as (in the parlance of the times) independent or dependent variables. It therefore seemed that

a "real" environmental sociology, as opposed to a sociology of environmental issues (Dunlap & Catton, 1979a), should entail examination of "environmental variables," and Catton certainly agreed. We therefore quickly decided that environmental sociology should be defined as "the study of interaction between the environment and society" (Catton & Dunlap, 1978a, 44), or simply societal–environmental interactions or relationships.

While eager to help define and codify the field, we realized it would be difficult to convince sociologists that the study of environmental phenomena was legitimate because of outmoded disciplinary traditions. Durkheim had understandably justified the need for a discipline of sociology by emphasizing its unique focus on "social facts," and his antireductionism dictum served to guide uniquely sociological investigations. Although crucial for establishing our discipline, the "taboo" against environmental and other reductionisms seemed outmoded by the 70s, and our second contribution was to highlight this point. Doing so led to a widely misinterpreted aspect of our work. We never suggested that Durkheimian theory was irrelevant nor that Durkheim was responsible for our discipline's anthropocentric orientation, but simply that by the mid-20th century his dictum against examining nonsocial factors (which he himself violated) had become institutionalized and served to discourage sociological attention to the physical environment. Fear of being labeled an "environmental determinist" was well founded (Catton & Dunlap, 1980).

Our argument that disciplinary traditions had created a set of "blinders" that caused most sociologists to ignore the sociological significance of environmental problems quickly evolved, thanks to the popularity of "paradigms" at the time, into a recognition that mainstream sociology was based on a taken-for-granted set of assumptions about our species and modern industrial societies that was inherently "unecological." Catton had already made this point in 1972 (Catton, 1972), I later discovered,[2] but we fleshed it out in far more detail and labeled it the "Human Exceptionalism Paradigm" (HEP; Catton & Dunlap, 1978a). We argued that mainstream sociology was premised on a view of humans as such an exceptional species—an assumption given credence by the quarter century of incredible technological advances, growth, and prosperity experienced by the United States after World War II—that it seemed as if modern societies had become independent of their environments and free of ecological constraints. The HEP could be regarded as our third contribution.

Formulating the "HEP" led automatically to thinking about an alternative paradigm, something that Catton had also called for in general terms back in 1972. But 5 years later we were able to tie the call for a new paradigm to the emergence of environmental sociology. We argued that a field focused on societal–environmental relations would not only violate disciplinary traditions but also, at least implicitly, reject the HEP by its recognition of the sociological relevance of ecological conditions and in the process embody a new paradigm. The difficult part was trying to clarify the latter.

More Context

Here is where contextual factors enter again. While Catton was writing papers that applied real ecological concepts (as opposed to the sociological human ecology versions) to modern societies in the mid-70s, I was conducting empirical research to justify my research appointment in rural sociology. Working with Don Dillman had strengthened my predilection for survey research, and naturally I began to conduct environmental surveys.

In 1974 I taught my first graduate seminar (the title included both sociology and environment in some fashion that escapes me) and used Pirages and Ehrlich's (1974) *Ark II* as one of the texts. Given my initial interest in American values and early work on partisan and ideological cleavages over environmental protection, I was intrigued by their concept of the "Dominant Social Paradigm" (DSP), which they described as a set of beliefs and values that constituted an inherently antiecological worldview in American society. It included not only core conservative values such as individualism, laissez-faire government, and private property rights but also faith in technology, resource abundance, unlimited growth, and endless progress.

In 1975 I set out to operationalize the "DSP" and measure commitment to it among Washington State residents and enlisted Kent Van Liere as an RA. By then my personal worldview had become much more ecological, and this helped me see that *some* environmentalists were not simply pushing for environmental protection policies but were challenging the DSP in fundamental ways. From calls for a "land ethic" to widespread discussion of "limits to growth," ideas that challenged the DSP seemed to be gaining momentum and credibility. At some point I realized that one could discern a "new environmental paradigm" emerging from environmentalism and decided to try to conceptualize and measure it along with the DSP. The result was a very preliminary measuring instrument, the New Environmental Paradigm Scale (Dunlap & Van Liere, 1978), which because of its multidisciplinary use has become my most widely cited publication (Dunlap, 2008), as well as a (long-delayed) companion piece measuring commitment to the DSP and its relationship to support for environmental protection (Dunlap & Van Liere, 1984).[3]

It was therefore easy to transplant Van Liere's and my societal-level version of the NEP to the disciplinary level, and thus elaborate a "New Environmental Paradigm" with Catton that, in a nutshell, highlighted the ecosystem dependence of modern industrial societies and put more meat on the bones of his earlier call for a new paradigm. Proposing an "NEP" to replace the HEP was our fourth contribution and another controversial and widely misunderstood one. Because I have given my perspective on the ensuing controversy in detail elsewhere (Dunlap, 2002), there is no need to repeat it here. Suffice it to say that despite the ambiguity in our article I did not see an ecological *paradigm* replacing sociological *theories*, and despite my hope that environmental sociology would embody a new paradigm I gave little thought to whether the entire discipline would ever come to embrace the NEP.

In sum, as our thinking evolved, Catton and I definitely benefitted from the synergism created by our differing backgrounds and perspectives. We shared a desire to help launch a field of environmental sociology and quickly agreed on what it should look like and how bringing it to fruition would entail violating disciplinary traditions. Catton's past work had already pointed to the need for a paradigm shift, and he took the lead in clarifying the HEP. (As an untenured assistant, I might have been hesitant to argue that all of sociology was flawed!) In turn, I think my work with Van Liere was crucial in helping us develop the NEP. The key point is that I doubt either of us, particularly me, would have developed these ideas on our own.

Key Publications

Having charted the evolution of our thinking and goals, let me turn to the three articles that are the subject of this symposium (while commenting on a few related but far less-cited ones).

Our initial publication offered our definition of the field, explicated HEP and NEP, argued that environmental sociology reflected the emergence of the latter, and then illustrated our argument by reviewing NEP-based work on social stratification (Catton & Dunlap, 1978a). We submitted the paper for a special issue of *The American Sociologist* on "new theoretical perspectives" that had severe page constraints (22 I believe), but the tradeoff was worthwhile as *TAS* was then an official ASA publication with a wide readership and a theory symposium likely received extra attention. The fact that we presented it at the 1977 ASA meeting generated some prepublicity within the new section, and being (to the best of my knowledge) the first article with "environmental sociology" in the title clearly gave it cachet.

The article received a good deal of attention, especially among environmental sociologists, including an immediate friendly critique from Fred Buttel (1978). Our paradigm argument proved controversial, in part because the terseness of our presentation created the impression that we saw the NEP as a theory rather than broad paradigm.[4] However, the plausibility of our call to shed the HEP has increased over time as global environmental change has made it apparent that modern industrial societies are ecosystem dependent and far from "exempt" from ecological constraints (Dunlap, 2002). The article has been reprinted in six volumes, including two on sociological theory, and the HEP–NEP distinction continues to be discussed in a wide range of books, especially environmental sociology texts.

Our second key publication was less provocative and no longer receives the same degree of attention, but in the formative years it played a role in helping establish environmental sociology as a legitimate specialty by appearing in the *Annual Review of Sociology* (Dunlap & Catton, 1979a). We were excited by the invitation to write the piece, no doubt a result of James Short being on the Editorial Board, not only because of the prestige of *ARS* but also because it offered an opportunity to demonstrate that a rapidly growing body of sociological work on environmental issues represented the arrival of a new sociological field.

We quickly summarized our paradigm argument and renamed HEP and NEP (as the "Human *Exemptionalism* Paradigm" and the "New *Ecological* Paradigm," respectively), suggested that the POET model widely used by sociological human ecologists provided a useful framework for conceptualizing societal–environmental interactions,[5] and then summarized a wide range of literature into two categories: work employing traditional sociological perspectives such as studies of environmentalism, termed the "sociology of environmental issues," and work examining in varying degrees societal–environmental interactions, termed "environmental sociology." We cast a wide net for the latter category, ranging from work on natural disasters to the built environment. Indeed, in those days the most explicit analyses of societal–environmental interactions were micro-level studies of housing and the built environment, helping account for the fact that the ASA section provided a home for both "built" and "natural" environment specialists (Dunlap & Catton, 1983). Although the *ARS* piece was cited frequently in the early years of environmental sociology, over time it has received less attention as the field developed and newer reviews appeared (e.g., Buttel, 1987; Buttel & Gijswijt, 2001; Dunlap & Marshall, 2007; Goldman & Schurman, 2000).

As Catton and I were developing our paradigm argument I was struck by parallel trends in other social sciences and in 1979 managed to organize a symposium on "The Emerging Ecological Paradigm in the Social Sciences" for the 1980 meeting of the American

Association for the Advancement of Science. I assembled a strong lineup, highlighted by Herman Daly representing economics, and Sage readily agreed to let me edit an issue of the *American Behavioral Scientist* based on the symposium papers (Dunlap, 1980). I was frustrated that our initial presentation of HEP/NEP had been so brief and also stimulated by Buttel's critique to clarify our argument, so the *ABS* issue offered the perfect opportunity for Catton and me to revise our paradigm argument (Catton & Dunlap, 1980). We attempted to respond to Buttel's critique and other feedback by providing a far more detailed analysis of how disciplinary traditions had discouraged sociological attention to environmental issues, offered improved versions of both HEP and NEP, and then emphasized that we did not see the NEP as an alternative to traditional sociological theories (as Buttel and others had interpreted) but as a foundation for the development of theoretical perspectives that recognized the ecological bases of social life (Dunlap, 2002).

I am pleased that this article has gradually garnered almost as many citations as the 1978 *TAS* piece, as I believe it offers a much better depiction of our paradigm argument, but its length discourages reprinting, and my sense is that it is less often cited in textbooks. Again, the first publication on a topic has an inherent advantage in garnering attention, even when it is replaced by a stronger one.

Catton and I continued to write a few things together over the ensuring years, even after Bill retired. The first was a plea for the built-environment and natural-environment "camps" within the nascent field to work together, highlighting the common problems they shared in investigating societal–environmental interactions and arguing that an "ecological framework" would be helpful to both (Dunlap & Catton, 1983). After a long hiatus, the second was a hastily written piece for a conference in Japan that reiterated much of our earlier work but briefly argued that the emergence of global environmental change offered a superb opportunity to investigate societal–environmental relations at the macro level and teasingly suggested that "ecological sociology" might be a more appropriate label for the field (Dunlap & Catton, 1992). The third piece quickly followed, offering a short history of the field, reiterating the importance of focusing on global environmental change, and decrying the dominant constructivist focus of early sociological work on global environmental change that threatened a reversion to human exemptionalism (Dunlap & Catton, 1994). Our last joint piece, prepared for a symposium comparing "environmental sociology" to rural sociology-based "natural resources sociology," employed our concept of the three functions of the environment (supply depot, living space, and waste repository) to compare the two fields (Dunlap & Catton, 2002).

Impact

It is apparent that the three publications highlighted in this symposium remain our most important contributions, and it is enormously rewarding to see that our goal of helping provide a foundation for the field of environmental sociology seems to have been achieved. Like Schnaiberg's (1980) "treadmill of production" (which epitomizes an NEP-based "theory"), HEP/NEP seems to have become—thanks in part to Fred Buttel (1987)—part of the lore of the now vibrant field of environmental sociology and we are honored to be in such esteemed company.

Nowadays our early work receives more attention in text books than in journal articles, and I suspect this reflects the fact that exemptionalist thinking—which was prominent in sociology in the 1970s (Dunlap, 2002, pp. 335-336)—has become rare among sociologists. Certainly within environmental sociology, the combination of obvious ecological problems ranging from toxic contamination to global warming *and* a new generation of scholars who take the significance of these problems for granted, makes the ecosystem dependence of modern societies seem self-evident. In an era when the human "footprint" on the global ecosystem has become so undeniably large that analysts argue the Earth has transitioned from the Holocene to the Anthropocene epochs (see, e.g., Steffen, Crutzen, & McNeill, 2007), there is little need to note that one's work is premised on an ecological rather than exemptionalist view of the world—and increasingly little fear that one will be charged with environmental determinism for treating ecological conditions as independent and not just dependent variables.

When the *American Sociological Review* published an article employing latitude as a key variable (treated as a proxy for climate) predicting variation in the ecological footprints of nations (York, Rosa, & Dietz, 2003) it became apparent that (a) Durkheim's outmoded taboo against employing nonsocial variables has given way to recognition of the societal *and* sociological significance of ecological conditions, (b) human exemptionalism no longer dominates our discipline, and (c) environmental sociology has arrived. Having my early writings with Catton recognized as playing even a small role in bringing about these changes—clearly minor compared to growing recognition of problematic ecological conditions over the past three decades—is deeply gratifying.

Early on I did tie my career to a potential bandwagon topic, but environmental sociology is becoming a freight train that keeps picking up momentum thanks to the growing number of new and highly talented scholars joining the field. What a joy and privilege it has been to see environmental sociology develop. Now if we can just begin to spread our ecological awareness and insights more successfully to the larger society.

Notes

1. The POET model was developed by Duncan (e.g., 1961) as a highly simplified and human-oriented model of the ecosystem and is designed to show that a human population (P) adapts to its environment (E) via social organization (O) and technology (T).

2. I cannot recall when Catton called my attention to his earlier article (a sign of his inherent modesty) but am struck by the fact that it was not cited in our first article (Catton & Dunlap, 1978a).

3. A revised and improved NEP Scale (Dunlap, Van Liere, Mertig, & Jones, 2000) is fortunately receiving far more use at present.

4. Our first attempt to correct this erroneous interpretation came in our reply to Buttel (Catton & Dunlap, 1978b), but that clarification and subsequent ones (Catton & Dunlap, 1980) failed to prevent continuing misinterpretations (Dunlap, 2002, pp. 337-342).

5. We elaborated the POET scheme as a "framework for analysis" for environmental sociology in a companion book chapter (Dunlap & Catton, 1979b).

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