

When I wrote this passage I had Kuhn's concept of "normal science" in mind. Amateurism takes root in those sciences or areas of sciences that lack a dominant paradigm as the exclusive guide to scholarly research. These fields are in a preparadigm stage of development in which several schools of thought compete. Here there is an awareness among professionals of conditions (1) and (2) that there are significant undiscovered phenomena and that many important characteristics and behaviors of known phenomena have yet to be systematically studied. In other words, the need as the professionals see it is for more descriptive data, a requirement they find they are unable to fill without help. If their field also satisfies condition (3), then the stage is set for the development of an avocational wing. It should be clear that I see sociology as such a preparadigm science in which these conditions are met.

Why then, is there no amateur sociology? Among the answers to this question are that we lack a tradition of amateurism, a vision that amateurs could help us with some of our research interests, and a willingness to organize nonprofessional assistance on any basis other than temporary. The same conclusions could likely be reached for other sciences, even though they meet the four conditions, they still have no amateur practitioners.

The present state of knowledge about the avocational sciences is so sketchy that even their extent is unknown. Since there is typically scant awareness of these pastimes in the larger community (ham radio and astronomy being partial exceptions), it is difficult to learn how widespread they actually are. I am surprised at how my own list continues to grow. For example, the July/August, 1978, issue of *Loisir Plus* (p. 15) lists the sciences with local and regional societies organized under the Fédération Québécoise du Loisir Scientifique (Quebec, Canada). These include, in addition to the sciences mentioned in preceding proposal, botany, geography, horticulture, mathematics, meteorology, mycology, and zoology.

In short, the issue raised by Althausser, of how well my proposal explains the way avocational science groups emerge and persist will be settled when we can identify the fields with amateur developments and compare them with fields that lack such developments. This issue, however, is of interest chiefly to sociologists of science. It in no way vitiates the present call for an amateur sociology, even though detailed knowledge about the emergence and persistence of avocational science groups would certainly make it easier to heed that call.

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ENVIRONMENTAL SOCIOLOGY: A NEW PARADIGM?³

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William R. Catton, Jr., and Riley E. Dunlap (1978) have recently advanced a "New Environmental Paradigm" (NEP) as the cutting edge of environmental sociology and perhaps the discipline as a whole. In so doing they argue that "ostensibly diverse and competing perspectives in sociology are alike in their shared anthropocentrism" (1978:41), i.e., the similarities of these previous competing theories overshadow their apparent dissimilarities. Catton and Dunlap term the

old paradigm, which cuts across the many groupings of competing theories, the "Human Exceptionalism Paradigm" (HEP). To them the HEP is not only anthropocentric, but also unrealistic and inappropriate for understanding the fundamental ecological substratum of human societies—the survival limits of human populations in the biosphere. My argument is that neither the NEP nor the HEP embodies a coherent set of domain assumptions, and as a result, the assumptions of each are neither internally consistent nor mutually exclusive. I do not seek to deny the relevance of the distinction that Catton and Dunlap draw, I suggest rather that the HEP/NEP cleavage is part

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of an ongoing, lively debate *within* major extant sociological paradigms, and that the resolution of this debate depends upon the paradigm in question

The HEP/NEP Distinction

Catton and Dunlap argue that practitioners of various theoretical postures in contemporary "mainstream" sociology share four HEP assumptions. The authors refer to the HEP molded by these assumptions as a basically "optimistic worldview." But it is unclear how this worldview adds up to a paradigm that has fostered or will foster a coherent, cumulative (although possibly flawed) line of research and theoretical elaboration. The optimistic worldview cum HEP does indeed embody a *general*—usually implicit—assumption that societal adaptation to the confines of the biosphere is not particularly problematic. However, the HEP "paradigm" as presented by Catton and Dunlap does not specify—nor do I feel that the range of theoretical thought encapsulated within the HEP will allow them to specify—the general or specific modes that this presumed adaptation is likely to take. Implicit within the HEP model of adaptation of course is the notion that material progress is possible and that the barriers to this progress are primarily social—not biogeochemical. Yet I would argue that the roots (i.e., "world hypotheses" or "domain assumptions", Gouldner, 1970:30–35) of this optimistic image of social and material progress are so diverse that it is illusory to emphasize only their similarities and minimize their differences. There are many distinct sociological viewpoints that Catton and Dunlap group under the HEP rubric, there is, for example, the enduring philosophical/epistemological debate about whether this material progress can realize its full expression within capitalism¹ (cf. Aronowitz, 1974 and Bell,

1973). Further, Aronowitz's work clearly casts doubt on the validity of the authors' assertion that "persistent adherents of the HEP [are] accustomed to relying on endless and generally benign technological and organizational breakthroughs" (1978:45). Aronowitz's analysis of the technological and organizational parameters of capitalist society is hardly a benign one. In sum, the HEP does not offer a coherent set of domain assumptions that can specify the fundamental dynamics or "laws of motion" of societies, clearly, HEP sociologists' differences in this regard can best be depicted in terms of the traditional theoretical groupings—functionalism, positivism, Marxism, and so forth—that Catton and Dunlap find of little use.

A final observation on the HEP is that Catton and Dunlap might well be ascribing more consistency to the four HEP assumptions than actually is the case. In particular, it is not clear that assuming the unique cultural character of human societies and the diversity of cultural expressions leads to any necessary assumption about the lack of limits of survival base or the perpetuity of present trajectories of technological and material "progress." The four HEP assumptions, then, are more loosely organized than Dunlap and Catton argue—further depreciating their presumed leading role in shaping sociological inquiry.

The assumptions underlying the NEP reflect a more "ecologically realistic" worldview than those of the HEP. Catton and Dunlap are correct that most environmental sociologists readily accept the premises of limits and ecological constraint, and I agree with the importance of incorporating these assumptions into the mainstream of sociological analysis. However, the critical question in my view is whether environmental sociologists—defined as those who agree on all or most NEP assumptions—represent the fundamental theoretical consonance that is ascribed to them.

To repeat some of the illustrative citations made by the two authors, I would

¹ Thus the HEP (as well as the NEP) abstracts beyond and provides no insight into longstanding domain assumptions concerning issues such as the extent to which "society is precariously or fundamentally stable, that social problems will correct themselves without planned intervention [or] that man's true humanity resides in his feelings and sentiments" (Gouldner, 1970:31). Also unclear is the

relation of the HEP and NEP to the issue of the autonomy of social structure (Gouldner, 1970:51–54).

argue that the perspectives of Charles Anderson (1976) and Allan Schnaiberg (1975, 1977), on one hand, and Samuel Klausner (1971) and William Burch (1971), on the other, diverge fundamentally, although all four environmental sociologists are solidly within the "NEP" tradition I am inclined to characterize the major contours of these divergences as being between "critical-Marxist" and "functionalist-organistic" NEP practitioners (see Buttel, 1976, Buttel and Flinn, 1977, for related schemas) Nevertheless, my contention here is that sharing the three NEP assumptions does not presuppose either theoretical consonance or cumulative inquiry on the part of environmental sociologists The working domain assumptions of the NEP and environmental sociology are still matters of controversy

In this regard it is worth special mention that Catton and Dunlap tend to group academic Marxists as being supporters of unbridled economic expansion and critics of environmentalism—in other words, non- or anti-environmental sociologists Again I feel that there is ample evidence that a substantial minority of Marxists have quite readily adopted their overall theoretical posture to a meaningful (NEP-based) environmental sociology (see, for example, Anderson, 1976, Ezenberger, 1974, Molotch, 1976; England and Bluestone, 1973, Deutsch, 1976, Applebaum et al 1976)

Environmental Sociology and Social Stratification

Catton and Dunlap's application of the NEP to the analysis of social stratification, class structure and the sociopolitical morphology of conflict illustrates the limits of assigning paradigmatic or pre-paradigmatic status to environmental sociology The authors' primary point is that existing U S economic institutions are fundamentally implicated in an expanding, nonredistributive treadmill Why this is the case is unclear from the three NEP assumptions set forth by Catton and Dunlap Again, this ambiguity seems to arise because the NEP assumptions have little to say about the dynamics and laws of motion underlying socioeconomic transformation

Both the dominant class and the working class tend to find their interests best served by economic expansion (or the promise of future expansion), even though the working class absorbs the bulk of the "costs" (especially pollution and the destruction of urban residential environments) of such expansion (Burch, 1976 Schnaiberg, 1975 England and Bluestone 1973, Molotch 1976, Morrison, 1976) Dominant class support for economic expansion and general opposition to recognizing the limits of the biosphere are quite understandable, because corporate profits (and the salaries of high-level managers) are directly or indirectly based on growth and environmental destruction (Hardesty et al , 1971) But since the impacts of environmental reform ("planned scarcity" in Schnaiberg's [1975] terms) tend to be substantially regressive (see also Sills, 1975), the subordinate classes tend to join their more privileged counterparts in pressing for further economic growth (Schnaiberg's [1975] "economic synthesis" of the societal-environmental dialectic) However, following NEP logic, the economic synthesis is fleeting and temporary because it implies exponential economic and materials growth in a finite planet Many environmental sociologists are now coming to realize that a "sustainable society" (Stivers, 1976, Pirages, 1977, i e , Schnaiberg's "ecological synthesis") requires a far more egalitarian social structure than presently exists in the U S In other words, *redistribution* in some fashion seems to be a prerequisite for breaking the chains of repeated return to the ecologically disastrous economic synthesis (see also Stretton, 1976)

I tend to agree with the basic outlines of this analysis and Morrison's (1976) earlier presentations of its major elements However, I feel that Catton and Dunlap's analysis of stratification and class structure remains incomplete in one important respect Given that movement toward the ecological synthesis is necessary to achieve a sustainable society, and that redistribution is in turn necessary to achieve the ecological synthesis, the question becomes how can we best conceptualize and help bring about these social changes Catton and Dunlap argue that the American Left (trade unions spokespersons for

the poor, and Marxists) has abandoned hopes for "real" redistribution in favor of settling for a "fair share of a growing pie." My concern is that Catton, Dunlap, and others have abandoned the Left as a progressive force for achieving the ecological synthesis. I would argue that the Left is the only group (or congeries of groups) that can unite around "real" redistribution, the dominant class (including environmentalists at the fringes of that class) can hardly be expected to support such a program of redistribution and environmental control. To count out the Left is to ignore dominant class interests in the hegemony of growth (Molotch, 1976) and the possibility that Marxian-type conflicts may be necessary to bring about the transition to the sustainable society that environmental sociologists ostensibly advocate. This difference between Catton and Dunlap, and me, on the possible role of the Left, is the basis of my argument that all possible NEP-informed analyses of social stratification and class structure are not directly deducible from NEP assumptions.

In this regard it is interesting to note that Catton and Dunlap agree with Schnaiberg (1975: 9-10) that "the synthesis adopted will be influenced by the basic economic structure of a society, with 'regressive' (inequality-magnifying) societies most likely to maintain the 'economic' synthesis and 'progressive' (equality-fostering) societies the least resistive to the 'ecological' synthesis." Sweden, a society with a long history of class conflicts that can be legitimately regarded as proto-typical Marxian-type conflicts, and with lower levels of inequality as a result of these more heightened class antagonisms (Anderson, 1976: 246-251), is suggested by the authors to be farthest along the road toward the ecological synthesis. This circumstance should suggest some reconsideration of the present and, especially, the future role of the working class and the American Left in securing ecologically beneficial changes (see Deutsch, 1976). This is not to say that all manifest or latent ecologically beneficial initiatives must necessarily come from the Left or that the Left must inherently be a demonstrable pro-environmental force. Rather, I wish to suggest the importance

of realizing that since the subordinate class derives the smallest benefits and highest costs from the economic synthesis, the Left is logically the nascent force for redistribution and ecological protection—the ecological synthesis.

Conclusion

Catton and Dunlap's paper deserves great attention because it does alert us to some key concerns of environmental sociologists and major issues that face both society and sociology. However, I believe that environmental sociology is not and can never be a paradigm in the sense of producing a cumulative, coherent literature. More specifically, the sets of assumptions associated with the HEP and NEP "paradigms" have virtually nothing to say about the key social forces leading to change and transformation. The HEP/NEP cleavage, to be sure, *is* real, but *within existing paradigms* (Friedrichs, 1972). Thus I do not wish to deny the validity of the HEP/NEP distinction, but rather to indicate that the intellectual "action" is equally lively within, for example, Marxist and functionalist circles, as well as between NEP sociologists and their HEP opponents generally. I do believe that environmental sociology is more than just another sociological sub-area such as political sociology or the sociology of religion, but this does not in itself confer paradigmatic importance upon environmental sociology. The emergence, then, of sociological inquiry in the NEP tradition need not lead to a reduction of the influence of existing sociological paradigms, in fact, existing work in environmental sociology leads me to believe that fundamental differences may well be reinforced. In sum, environmental sociology in my view will continue to be pervaded by theoretical—that is, paradigmatic—diversity.

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PARADIGMS, THEORIES, AND THE PRIMACY OF THE HEP-NEP DISTINCTION

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A paradigm, as we have used the term, is an image shared by members of a scientific community telling them the nature of their science's subject-matter. Theories are not paradigms, nor does each paradigm generate one and only one theory. Yet Buttel mistakenly equates

"theoretical diversity" with "paradigmatic diversity."

¹ This mistake can be attributed to the varied usage of the word paradigm in sociological and other literature. For examples of diverse meanings quite unrelated to Kuhn's (1962) see Gross