

**Reflections on My 25 Years Before the Mast  
of the Environmental & Technology Section**

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A. BACKGROUND STRUGGLES BETWEEN ABSTRACT AND SOCIAL PROBLEMS CONCERNS

I was born and brought up in 'science' era, graduating with a BA in Chemistry in 1960, and later working in the transportation sector, on metallurgical engineering team. From here I decided to go to grad school, to teach...something. Sociology was my pick, in part because I had one sociology course, and found that the materials I absorbed there helped me understand much of what was happening in the airframe company I worked for (e.g., shopfloor culture, worker insecurity because of layoffs).

My University of Michigan experiences involved struggling with population problems (which provided my fellowship support), and what role they played in international inequalities. I also struggled with my disillusionment about survey research, which arose during my Malaysian family planning data collection experience. I also had problems with publishing my prospective thesis, because of ideological conflicts among Malaysian racial groups and their family planning program.

My thesis turned to a study on Turkish women, using data collected by my mentor

David Goldberg. This study devolved into a study of modernization . It was in some ways a move away from a "social problem" mode of working, and into a more theoretical synthesis (but an empirically grounded one). I think this was the first time I moved close to a "sociological imagination", as described by C. Wright Mills some ten years earlier. My modernization work combined reflections on the linkages between urbanization, stratification, and modernization, and challenged much of the previous work on modernization (which emphasized a kind of "culture of poverty" approach versus my own on the "structure of poverty").

By the time I graduated in 1968, I had become farther removed from "relevant" research. I stayed away from population issues because I became uneasy about the potential for regressive and repressive actions, and quite conservative research. I felt the analytic and political linkages between population growth and all economic and environmental problems, was too facile, and represented a kind of "blaming the victim" approach.

Yet I still felt sociology was a social science, and therefore I had social obligations, in return for the SOCIAL support I had received.

## B. DRIFTING INTO ENVIRONMENTAL ANALYSIS

When I came to Northwestern in 1969, I became peripherally involved in the anti-war movement. I used my Malaysian experience (and related reading) to become a speaker to community groups, who wanted to know what Northwestern students were protesting about. But I felt really like an academic "imposter", having only a peripheral knowledge of Vietnam and Southeast Asia.

At the same time, I soon joined a Northwestern faculty-student "study group". We focused on problems arising from our local utility's air pollution from its coal-fired plants. Environmentalism seemed a much more progressive field, in contrast to populationism (victims were more clearly identified, especially the urban poor, suffering from air pollution). The political impact of our analyses led to the firing of the staff member who had suggested forming this study group. This reminded me of just how political "environmental analysis" could be. Among other factors, the Chair of our Board of Trustees was the CEO of this utility, and probably had not anticipated our study group's criticisms.

With my chemistry background, and my engineering/science experience, which I felt I could bring this arena along with my social science interests and skills, I had the potential to offer a unique synthesis in "environmental problems".

My colleague John Walton offered me an invitation to write a piece for an urban book he was editing. He encouraged me to explore my new environmental interests. At this point, I made my first departure from more quantitative empirical work to qualitative syntheses. Using social observations from other published studies, I wove these into an argument about the four strands of "environmentalism". This was clearly an intellectual watershed for me, drawing me away from the detailed quantitative empiricism in which I had been trained.

I felt so personally integrated after doing this, that I began to explore other opportunities to continue environmental work. My goal was twofold:

- (1) to raise ecological awareness among sociologists, and
- (2) to apply the sociological imagination to the origins of problems and social

outcomes of alternative solutions to ecological degradation.

It is also important to note that virtually none of this reflective work has been funded by any outside institution. I later argued that this low-capital approach to research was my own form of "sustainable development".

### C. THE CREATION OF THE ASA SECTION: AMBIVALENCE

When there was a proposal to form an ASA (and SSSP) section on environmental problems, I resisted this. I felt that this would deter us from directing our efforts towards consciousness-raising among other sociologists, and attempting to offer broad social science models for resolving some environmental conflicts. Instead, we would wind up being "just another section". Cynically, I feared we would just work to protect our own narrow professional fiefdom.

Yet Riley Dunlap argued that young scholars (by this time I was tenured). They needed some arena within which to work and to be promoted. Under this perspective, I reluctantly agreed, and even became the president of the section some years later.

The rise of an energy awareness during the several "energy crises" of the mid to late 1970s made me even more determined to pursue my goals of sociological consciousness raising about ecological problems, and social science approaches to evaluating policies around socially-defined environmental problems, and the social impact of proposed policies. Many of the natural science "solutions" to the energy crisis seemed very socially naive and unworkable. Yet at the same time, this new awareness by natural and some social scientists seemed to open a path for us to study a genuine socio-environmental dialectic. Painfully, I found little sustained commentary or research on

energy issues across the section, and was disheartened.

My sabbatical in Santa Cruz for 1975-76 was a period for attempting to draw together many of the ideas I had been developing for 5-6 years. It was then that I inductively drew out the core model of the treadmill of production (see ETS newsletter for more details). It took three more years to finish it, and I had gone through several life changes, including a department chairmanship, and a painful divorce.

The book was a mixture of aggressive attacks on existing arguments, and some new attempts at genuine intellectual-theoretical syntheses --reflecting several different facets of my own personality.

My major synthesis -- the treadmill of production -- drew together a number of theories and empirical trends. In some ways, this was the equivalent of my doctoral thesis work on modernization. I integrated data from a variety of fields -- industrial sociology, world-systemic work, stratification research, sociology of science, and macroeconomic theory and analyses. Now I sought to explain why and how environmental problems seemed to have increased so rapidly in the post-world war II period in industrial societies. The concept of the treadmill visualized a political-economy driven by several core factors.

First, there was a social and political assumption of the need to continuously expand industrial production and economic development. Economic expansion was generally viewed as the core of any viable social, economic, or environmental program. Economic expansion was thought to increase the profits that corporate managers and their investors require for capital outlays. Workers benefitted from these outlays because they led to increased production, which created new local employment opportunities both in

direct industrial production and, more indirectly, in the construction and service sectors. The service sector was thought to grow most rapidly due to the economic multiplier of having more workers with higher wages living and spending within a community. Capital outlays also led to higher levels of productivity--a precondition for rising wages. Finally, local and national governments viewed economic expansion as increasing the taxation capacity of the government, allowing it to distribute compensatory benefits to displaced workers and dependent citizens. Governments believed that tax revenues would rise more rapidly than citizen demands, and thus government officials and agencies increasingly shared a stake in the economic expansion of the private sector .

Second, the treadmill was structured by the need to ensure that consumption kept pace with production. If economic growth comes about through increased production of goods, consumers need to have the disposable income to purchase the goods. The state worked with private capital to make low interest loans available to consumers for the purchase of homes and other items. In the United States we have now seen 20 years of state/private deregulation, making credit cards and mortgage loans easier to obtain. In fact, since World War II, per capital consumption in the U.S. has steadily increased.

Third, there exists the sociopolitical belief that social and ecological problems are best solved by ratcheting up the treadmill's pace. Social problems were generally thought to be best solved "through the market." Thus, there was a magical sense that any type of economic expansion would reduce social and ecological problems. Poverty would be reduced by a growing economy, because there was an expanded job base and an increase in wages. A growing economy also supported government social expenditures (for education, housing, and other needs of the poor to move up) .

Fourth, economic growth was tied to a commitment to an expansion of what we term 'corporate-centered development.' In this model, nation-states and cities prioritized the needs of private capital over the needs of the state itself and its constituent citizen-workers. The assumption was that economic expansion could only be fostered through the growth of large firms--what are often referred to as "core firms." Large firms were thought to be the engine of the economy. Their growth created the most demand for jobs, and it created a secondary demand for supplies that fuel the growth of smaller entrepreneurial firms. The wages paid to the large labor pools provided consumption needs in the stores that keep local merchants in business.

Fifth, and finally, all of these elements of sociopolitical belief were reinforced by substantial economic and political socialization efforts on the part of core firms and their dependent institutions (trade associations, advertising, educational efforts in promoting "free trade", etc.). Although there exists a substantial literature on the degrees and types of "corporatist" or "non-corporatist" forms of industrial states, I sensed in 1980 that virtually all industrial states were evolving into a common commitment to the types of growth policies that characterize the treadmill of production. Moreover, with growing transnational investments, there appeared to be an intensification of such commitments, as capital and employment flowed from more industrial societies of the North into the emergent economies from the South. There was an alliance of private capital, trade associations, and governments to promote these goals.

In general, I found the section to be supportive of the book when it emerged. There were very positive reviews published by a number of colleagues, as well as other environmental analysts. And I was even awarded the Distinguished Contribution Award

for the book. Yet I found little incorporation of the central tenets of the treadmill into environmental sociological literature. I felt largely peripheralized intellectually by the section.

Apart from this, and some new ventures into critical evaluations of "appropriate technology" (the grandmother of "sustainable development"?), this was not the richest professional period for me. In addition to other factors (including a painful divorce), I generally felt intellectually unconnected with the section, which seemed to have moved into a prematurely institutionalized structure.

When there was a proposal to change the name to incorporate "technology", I argued against this, since few section works (other than mine) had really addressed technology. {"What's in a name? (More than you might think)." *Environmental Sociology* 45 Summer 1986:4-5.}

But section members felt technology was a "hot" area, which only reinforced my sense that the section had become more ritualistic than innovative. Interestingly, with about 15 years hindsight, I think I was on target then (and now, alas).

#### D. TOWARDS MY NEW INVISIBLE COLLEGE

By the end of the 1980s decade, I felt burnt out and relatively anomic. With the entrance of Ken Gould into NU, I began a process through which I formed a small working group of "environmental sociologists". First Ken, then Adam Weinberg, and most recently, David Pellow. Moreover, with their energy and enthusiasm, we wound up producing three books, elaborating and applying the treadmill of production to a range of contemporary environmental issues and problems.

These included appropriate/sustainable technology (1994), cross-national pollution conflicts, recycling, and community mobilization around toxic wastes and wetland destruction (1996). Most recently, we published our analysis of urban recycling organizations and sustainable community development (2000). [see ETS Newsletter, spring 2001, for more details].

This productive period was also among the most satisfying stages of my professional life. We had ongoing intellectual debate and exchange, constructive criticism, and a sense of shared purpose. I personally felt the tension of wanting to sustain the group, but realized that my former students needed to spread their wings in the larger section and beyond. Each pursued some of their individual work, along with some collaborative efforts, in section presentations.

For me, in addition to the pleasure of intellectual collaboration and probing, this work represented a fresh empirical test of the utility of the model of the treadmill of production. Since most other environmental sociologists cited the treadmill, but never tested it in their research, I felt it was incumbent upon us to evaluate how useful this model was to explain contemporary environmental conflicts. Although I had done some preliminary work on recycling, when I began a collaboration with Adam Weinberg, and later, David Pellow, we chose to frame recycling as one example of "sustainable development". If this shown to indeed represent this change in production, recycling would represent a genuine empirical challenge to the theory of the treadmill. In fact, over the past five years, we have documented the power of the treadmill to absorb many of the political, social, and ecological drives that initially moved recycling to the national agenda.

## E. DISAPPOINTMENTS WITH MAINSTREAM SOCIOLOGY

In retrospect, I think I have developed my work in environmental sociology as

- (a) an opportunity to use sociological wisdom to explore and impact relevant environmental problems facing real communities,
- (b) an opportunity to advance sociological theory, and
- (c) a chance to work with smart and dedicated people.

For me, these have been somewhat inseparable parts of my own mission and career.

In contrast, within Sociology and within the ASA especially, I have been disheartened by the increase in technical skills of analysis (particularly quantitative), and somewhat of a decline in reflective analyses of social change and social problems. To some extent, this reflects larger societal trends, and heightened "professionalism". But to me it represents a loss of the potential of sociological research to contribute to social welfare.

## F. DISAPPOINTMENTS WITH ENVIRONMENT & TECHNOLOGY SECTION ACHIEVEMENTS

I was pleased, in a minor way, that two of my books made the "top 10" in the sections "most influential" books. But I was also chagrined to see how few members nominated books (10-25 in a membership of about 400!). I hope that some section members will further explore the value of the treadmill, comparing it systematically to competing models of environmental change.

To further ground these observations, let me note that I have been chairing the Boguslaw Award committee for a number of years now. Frankly, there have been few nominations for the award, which is devoted to "humanistic approaches to technology". For a section that is self-defined as Environment and Technology, I think this indicates a lack of serious research on the institutions that undergird technological change.

Moreover, I hope we can achieve a more vital debate at the section meetings than "how many sessions can we get next year?", or, "how do recruit more members to get more sessions?" I have often been disappointed with the "scholasticism" of the ASA, with papers addressing the vital issue of "how many angels can really dance on the head of a pin". Adding more sessions of such papers is not a way to expand the intellectual development of the section (and will certainly not draw the attention of other ASA members).

I am nearing the end of my own career, and I feel proud of the environmental sociologists I have helped to move on to important issues. For they will be my lasting contribution to the section and the field, and they will in turn help support still younger members whose enthusiasm and risk-taking will move us to more challenging paradigms.

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