the city. My relationships with the Department of Development and Planning and other agencies, including the Chicago Regional Port District, were long-standing and the direct outgrowth of previous full-time employment with the city. The Chicago Community Inventory was a major contractor with the City in conducting surveys and publishing basic research data, much of which was geographically useful. Close associations were also developed with the Chicago Association of Commerce and Industry. Two sociologists and demographers, Philip Hauser and Evelyn Kitagawa, who were associated with the Community Inventory and other organizations, collaborated frequently with geographers on local area studies.

At the local neighborhood scale, two organizations, the Southeast Chicago Commission and the Hyde-Park-Kenwood Community Conference—the former dominantly protecting the university's interests in the community, and the latter a "grass roots" citizen organization—were occurring in the communities surrounding the campus.[10] The area, of course, formed a convenient laboratory for studies in all the social sciences, including geography. Geographers not only participated very actively in the operation of these and other agencies, but also produced significant research on the community, which has been widely used in a variety of other areas faced with transition.

With support in part from the Ford Foundation and other sources, and with research contracts and grants from other organizations, the Center for Urban Studies was organized, in large measure in order to serve as an avenue for basic research in urban problems, focusing on Chicago, and especially on the university neighborhood. A comprehensive plan for conservation and partial renewal of the university community and nearby areas was developed and partially implemented. Faculty and graduate students in geography and other disciplines were afforded not only financial support, but excellent "real world" laboratories for many significant contributions to knowledge in several disciplines.

It is an interesting coincidence that the seventy-fifth anniversary of the Department of Geography of the University of Chicago coincides approximately with the seventy-fifth anniversary of the Association of American Geographers. In a sense, modern American geography may be said to have been born with the twentieth century. The two organizations, together with the venerable American Geographical Society and the National Geographic Society, were the cornerstones of the discipline. For many years the Chicago department was the only one in the United States with a graduate program. The second generation of twentieth century geographers, therefore, were virtually all products of that department. An interesting diffusion study could be made tracing the influence of the department on subsequent generations of geographers. From its unique position in the early years, the Department, in large part as a result of its own influence, has become one among many. Yet its standards of excellence, innovation, and leadership continue undiminished.

[10] Peter H. Rossi and Robert A. Dentler, *The Politics of Urban Renewal: The Chicago Findings* (New York: The Free Press of Glencoe, Inc., 1961); and Julia Abrahamson, *A Neighborhood Finds Itself* (New York: Harper & Row, 1959).

PLAYING WITH IDEAS*

William L. Garrison

THE University of Washington in the 1950s was the fifth of eleven campuses on which I had the pleasure of working either as a student or as a faculty member. Each of those places and times was special; all were intellectually stimulating and collegial. So, when colleagues say that the University of Washington must have been an especially exhilarating place during the 1950s, I have to guard my remarks with the caveat that campuses have all been that way.

* I indirectly take my title and stress on playing with ideas from Paul Feyerabend, a philosopher of science.

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I would like to look at some successes and failures regarding the University of Washington's Department of Geography during the 1950s. When I arrived in 1950, the department was of good size and teaching the usual array of regional and systematic courses. I fell heir to a slot in introductory economic geography and an opportunity for an advanced course on transportation. Protected by the then chairman, Howard Martin, from a heavy teaching load my first year out of graduate school, I remember eyeing the terrain and proposing a course to fill the last spot on the map, Canada and Alaska. This was done with great naivete, for I was equipped only by two years of living in Alaska and several trips through Western Canada. (That course never got taught, just as well.)

An early issue in the Department was how to teach introductory economic geography. Having taught with Clarence Jones when I was a graduate student at Northwestern, I was persuaded that a systematic approach was in order and bent the course that way. The Bureau of Agricultural Economics of the Department of Agriculture was active then, and I soon found how to get reports from State Department commercial attaches stationed around the world. These materials served to enrich the course and give it an economic slant. Graduate students were serving as teaching assistants, and the course gave me an opportunity to touch graduate students in a way that perhaps I did not in my other courses. Late in the decade, Brian Berry and I taught the course in tandem. One important thing that never got done was the textbook we were going to write using that experience.

A few months after my arrival, Donald Hudson arrived as the new chairman, and he moved to take advantage of several old and new faculty slots. But recruiting was not automatic; the College had a new dean oriented to programmatic justification and planning, a dean perhaps better fitted to the times of the 1970s than of the '50s. Everything had to be justified and written down. Donald Hudson led a continual process, helping the department to clarify what it was doing and why, a process commonplace today. Sure, there were some rough spots and conflicts and clashes among faculty stakeholders, but it amounted to a stimulating and useful activity, one that I have rarely seen so well guided.

Hudson's skills enabled us to bring Edward Ullman from Harvard and to recruit several young faculty from various universities with different intellectual and research styles. (Marion Marts was also from Northwestern, but our days as students overlapped little.) As an administrator, Hudson was a marvel at getting things done, listening to and protecting young faculty, and promoting faculty interests. Ed Ullman's style was different; he probably could not have administered himself out of a paper bag. The special thing about Ullman was his interest in ideas—his and anyone else's. Ullman worked with many ideas at a time, and his ideas were good for hours of conversation every week. Having sat in classrooms with Russell Whitaker and others who loved ideas, I quickly learned from Ullman the joy of playing with ideas, including how to discard many as not very useful toys and how to continue to play with and develop those that were more fun.

At the beginning of the 1950s there were still World War II veterans, typically on leave from teaching positions, among the graduate students. These veteran students were well on their way, and I had little interactions with them. My chance was to work with the next generation, a mixed bag, that began to fill up the assistantship space during the middle 1950s. Some were Korean veterans like John Nystuen; others were fresh out of college like Duane Marble and Michael Dacey. For some reason, perhaps because of differing backgrounds and styles of the faculty and what each could do for students, there was no feeling of belonging to particular faculty members. Students were not closely identified with faculty until the dissertation writing stage came, almost the end of the line. Thus, I never think of "my" students, but rather "our" students. Today, for example, I cannot remember what Waldo Tobler wrote his thesis on (it was with John Sherman), yet I think of him as a student with whom I had much intellectual fun.

Like other universities, the University of Washington landscape had its pin pricks of excellence and its not-so-lively scenes. Both were assets; students from not-so-lively departments came around looking for ideas. That's how I got to know and work with David Huff. My first try at a professional paper was on the optimum size of cities, a paper I worked out with a student fugitive from another department. Area studies were strong and supportive

of several young faculty members in the department. My opportunities were with Robert Hennes and Edgar Horwood in transportation engineering and with Arnold Zellner in economics, a neighbor of John Eyre's. Zellner was working in a new field (econometrics) in a traditional department, and he welcomed our students to his courses—a brilliant, cooperative, and generous man.

My contacts with transportation engineering soon extended nationally and internationally as did my contacts with regional science. I got to know Walter Isard while I was on leave at the University of Pennsylvania for a year early in the 1950s (Isard was based in Cambridge then), and through that contact and encouraged by Ed Ullman, I became involved early with the paradigms of regional science and with professionals excited by those ideas.

A high level of extramural contacts was supported by Hudson who was skilled at getting travel money for young faculty. Also, improvements in air transportation made a high level of extramural contacts thinkable and workable. At the beginning of the decade we were traveling by DC-4, on jets by the end.

The question I am asked most often is, "what stimulated quantitative work during the 1950s?" A more proper question is why so much interest in methodology in its traditional sense, causal paradigms, processes, and the logic of method? This latter question has been confused and hidden. Methodology has been confused with methods and the conclusion erroneously made that the only methods worthwhile are those that involve quantification. Part of the reason is obvious. Many processes are stochastic in character, errors of measurement often must be considered. These were matters of concern, but not of as central concern as was the logic of method.

Why our interest in the logic of method? Partly, it was the ambience of playing with ideas, and partly, for me, it was disenchantment with my Ph.D. thesis which imputed causality from taxonomic evidence. But there were many contributing factors. Torsten Hägerstrand spent an academic year with us, the Hartshorne-Schaefer debate was on. Several of the ingredients were in the department for the whole decade. There, the paradigms of regional and systematic geography were in clashing encounters; paradigms for the study of urban areas, tranportation, and regional science were accosting one another. Confrontations bubbled up, and we played with methodological ideas. And the accomplishment at the University of Washington during the 1950s was the production of a generation of students who liked to play with ideas about the logic of method.

What did not get done is unfortunate. Except for William Bunge's work, no one took the time to try to write down the results of play. Such results are hidden in the starting points of articles and other publications and in the minds of persons at the drama. They are present in the priority given to paradigms and in the way research questions are given priority, shaped, and tackled. This is good, but this is all there is.

An opportunity was missed because not enough attention was given to the development of techniques appropriate to the logic with which they were playing. I gave a course in quantitative methods which was a critical borrowing and review of techniques. Much more could and should have been done. The needs expressed by the methods we wished to use set a stage for creative development of techniques, a stage that was diffused as players moved around the country.

There seem to be two lessons from the University of Washington in the 1950s. One is that any stage can be an exciting and productive one if players are willing to take advantage of opportunities. The other lesson is more sobering. Although playing with ideas is fun and of value to the players, one should consider larger values and undertake the hard task of writing it all down crisply. Otherwise, much of the value of work is not transferred. But not everyone can do everything. Perhaps a missing ingredient at Washington during the 1950s was a Shakespeare to write the play for others to read or Boswells to tell parts of the story.