

Reasons to Smile

Interview with Brian J. L. Berry, Geographer and Political Economist

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Treviño C., Jesús A. 2004. "Reasons to Smile: Interview with Brian J. L. Berry, Geographer and Political Economist." *Urbana*, Vol. VII-VIII. BRIAN JOE LOBLEY BERRY, born in Sedgley, Stafford, England on 16th February 1934. British by birth, US by naturalization on November 2, 1965, he maintains a dual citizenship. He gave this interview in his office at the University of Texas at Dallas, located in Richardson (Green Hall at UTD-Richardson), on February 13, 2004, three days before his seventieth anniversary.

B. J. L. Berry is Lloyd Viel Berkner Regental Professor and Professor of Political Economy in the School of Social Sciences at UTD. Before joining UTD in 1986, he taught at Carnegie Mellon University (1981-86), Harvard University (1976-81), and the University of Chicago (1958-76). His most celebrated books include *Geography of Market Centers and Retail Distribution* (1967), *Spatial Analysis. A Reader in Statistical Geography* (1968), *City Book Classification Handbook: Methods and Applications* (1972), *The Geography of Economic Systems* (1976), *Urbanization and Counter-Urbanization* (1976), *Contemporary Urban Ecology* (1977), *Long-wave Rhythms in Economic Development and Political Behavior* (1991).

In this interview, B. J. L. Berry speaks about his professors and fellow students, his role in the so-called quantitative revolution in geography, the context of his articles and books, the new economic geography, the new trends in quantitative geography, his experience doing research, and the process of writing.

B. J. L. Berry is among those who always stay in the top of the wave because they have created it, as Ernest Rutherford, the 1908 Nobel Prize winner in Chemistry, would say. His central role in pushing the discipline of geography in a more scientific direction and the honors he has received for his contributions to the social sciences are his reasons to smile.



How did you decide to come to the USA from England in the early fifties? What was the process?

I was finishing my undergraduate education at University College London. I knew other people who had gotten Fulbright Scholarships to pay for travel to come to the USA to do graduate work. That seemed attractive to someone who was interested in traveling and going to other places and seeing other things. But in addition, Professor Henry Clifford Darby urged those of us who he believed were the most promising undergraduate students to come to the USA for graduate work.¹ He said, at that time, the British universities did not have well developed graduate training programs. Darby believed that US universities were much better suited and well organized in ways that provided students with the extra research training that is needed to become mature professionals.

You studied economics in England; why did you shift your interest to geography?

The undergraduate degree in London was nominally a degree in economics and political science. It is a degree in which the first two years you broadly study economics, politics, history, social anthropology, the basic research methods, and so on. In the third and final year, you concentrate in a particular field. So you take two years and then take what is called the Part One Examinations. After that, you have a third year of specialization. I opted to specialize in geography. I became quite interested in location theory. In Europe, location theory, or location economics, is taught in departments of economics. In North America, it is taught in departments of geography. So two things came together: the fact that I had chosen to concentrate in geography in my final year, which in part was the

¹ According to B. J. L. Berry, Henry Clifford Darby was the professor of geography at University College London and probably the most distinguished historical geographer of his generation.

reflection of my growing interest in location theory, and the fact that in North America you have to go departments of geography to study location theory. Certainly, from the external point of view, one of the premiere people in this field at that time was Edward Ullman (1912-1976). He was on the geography faculty at The University of Washington; he moved there from Harvard. He had published a paper called “A Theory of Location for Cities,” in the *American Journal of Sociology* (XLVI, May 1941: 853-64) in which he introduced Christaller’s central place theory to the English speaking audience. He and Chauncy Harris (1914-2003) had also published an influential little paper in the *Annals of the American Association of Political and Social Science* entitled “The Nature of Cities.” I actually applied to the University of Chicago, where Harris was based, as well as to the University of Washington, where Ullman was located. Darby also made me to apply to Wisconsin and to Minnesota, largely because America’s best historical geographers were at those universities: Andrew Clark at Wisconsin and Ralph Brown at Minnesota. I ended up at the University of Washington because they responded very quickly to my application, offered me a teaching assistantship, and wanted a response by a return telegram.

How did you go from the University of Washington to the University of Chicago in 1958?

I arrived at the University of Washington in 1955, got a Master’s degree in 1956 and then, very quickly, was able to get things together for my doctoral dissertation. In my final year at the University of Washington, which was the year 1957-8, they appointed me as an instructor. I was teaching the big introductory classes while finishing my PhD. First of all, based on my Master’s thesis, my advisor and I developed a series of papers

that subsequently became quite important. They were submitted to the journals in 1957 and ultimately were published in 1958. The discipline was small and the communications worked very well. The chairman of the geography department of the University of Washington, G. Donald Hudson, had received his PhD before World War II at the University of Chicago. He was a good friend of Gilbert White, who had come to Chicago as the new chairman of the department of geography, rebuilding it after a series of retirements. I suspect there were communications about likely new faculty. Late in 1957, I got a telephone call from Gilbert White asking me to come out to Chicago to interview. They flew me back in a piston plane, seven hours in one direction from Seattle to Chicago. I spent a few days to give a talk and to interview. Before I left Chicago, they offered me a job. Late in 1957, I knew I had a job waiting for me in Chicago, at that time one of the two or three most prestigious departments of geography in the country. It was also rapidly rebuilding. The other thing is that Chicago itself is a great institution, particularly for someone who is interested in urban research. So I had the Chicago job in that year; I suppose it was the plum job in the country. I know a lot of people were very jealous. It was 1957-58; the rapid expansion of the American universities in the 1960's had still to come. Before that, the jobs were few and far in between.

Who was your advisor at the University of Washington?

William Garrison. He was an assistant professor and had received his PhD at Northwestern University. He was the man who provided the quantitative focus in geography to a group of new entering graduate students: Richard Morrill, Duane Marble, John Nystuen. We all took the very first course in statistical methods in geography. We, as a group, became the so-called quantitative revolutionaries. Our prime contributions in

the next few years were to introduce scientific geography and push the discipline in a more scientific direction.

What was the topic of your dissertation?

In the summer of 1957 I needed a job, so I went to work in the city planning department of the city of Spokane, WA. The actual job I did for them was to craft the city annexation policy. But at the same time, the city was redoing its zoning ordinances. One of the things they wanted was to redo the system of zoning for retailing and service business. I struck a deal with them. Since I was alone, I said: “lend me a city car and in the evenings and weekends I will do the city survey of retail and service business, which you will have as the basic information for redoing that part of the zoning ordinance. The *quid pro quo* is that I can use the data for my dissertation.” I did an analysis of the commercial structure of the city and developed some new methods of numerical taxonomy for developing the new classification. It was something that had never been done before for internal business structure. The dissertation was basically on the geography of retail and service business within the metropolitan region.

What is the origin of your classic work, *Market Centers and Retail Distribution* (1967)?

For my Master’s thesis I had tested some of the propositions of central place theory. Once I got to Chicago, I was able to get a grant from the Office of Naval Research, which at that time was funding research in geography. This was before the National Science Foundation was created. The research grant enabled me to undertake a series of multicounty studies across the United States: from suburban regions, to intensive agricultural areas in Iowa, to extensive areas in the American West. In each of those areas

I did a thorough study of urban centers and their functions, consumer behavior, travel, trade areas and so on. Out of that, to refashion some of the elements of the central place theory in a broader empirical setting than heretofore. I was approached by a new geography editor at Prentice-Hall. They were planning a new series called the Foundations of Economic Geography. These were short, 160 page studies of different aspects of economic geography. The idea was to enable instructors to make reading selections to create their own courses rather than having standard textbooks. So they asked me to write one study dealing with the central place theory under the strict size limit of 160 pages. I pulled together some of the insights from my earlier research, the theoretical perspectives, the applications of the ideas in retail market analysis, the applications in regional planning, and wrote this little book in only four or five months.²

Your were among people who first published on factor analysis. An example is your article “Latent Structure of the American Urban System, with International Comparisons,” in *City Classification Handbook: Methods and Applications*, first published in 1972 and updated in 1977, in *Contemporary Urban Ecology*. Where did you learn it and what is the origin of this paper?

The paper is an extension of a work that started in 1941 with Chauncy Harris, in his “Functional Classification of American Cities.” His ideas were taken up by the International City Management Association (ICMA) where people like Victor Jones continued the classificatory work. Harris was involved with getting me together with the ICMA group to try to refashion the methodologies and ways of thinking about types of cities.

² This work is considered one of the path-breaking books in regional science (Brigitte S. Waldorf. 2004. “Path-Breaking Books in Regional Science,” *Papers in Regional Science* 83: 59-89). The 1967 monograph and its 1987 edition is one of Berry’s most frequently cited publications.

By that time, factor analysis had not been used extensively either, in that context or in the context of helping to understand urban social geography. There are basically two types of factor analysis. One is exploratory, taking large data sets and trying to identify the independent patterns within them. It is a tool of inductive reasoning. It tries to identify patterns and then raise questions about causes, about the processes that have given rise to them. There is also a maximum likelihood version of factor analysis in which there is an attempt to see how closely hypothesized patterns are represented in the data set. One approach is inductive and the other is an inferential complement to theoretical reasoning, to deduction.

Where did you have contact with factor analysis?

I learned about it by reading as a student at the University of Washington. In the sociology department of the University of Washington there was also a professor named Calvin Schmidt who was interested in social ecology. He had a student who was working on his dissertation and was beginning to explore some of these techniques himself. We met in the computer lab at the University of Washington. I learned a little bit from him, and then much more intensively when I got to the University of Chicago. A couple of other faculty members of the university were using this technique: Duncan McRae Jr. in the Political Science Department and Benjamin Wright in the School of Education. Ben Wright brought in Henry Kaiser, a professor from the University of Wisconsin who was actively developing new methods of this kind. So I learned a lot by associating with these colleagues, sharing ideas. The major application was when I helped Norton Ginsburg with his *Atlas of Economic Development*. What he was doing was mapping and charting indicators of development and developing an Atlas. I said: "Look, many of these things

are saying the same thing. What we need to do is an analysis of these data to see how many different patterns there are, and to develop indicators of the patterns. Then we can ask the causal questions.” So on the big old computer, a 1961 UNIVAC-1, I programmed a direct factor analysis in Fortan I. The computer occupied the entire basement of one building. It was all vacuum tubes. It had a very limited capacity and we used boxes and boxes of IBM cards. To do something that takes microseconds now used to take us almost a week.

In one of your books you gathered contributions on factor analysis from other disciplines. Which author did you include for politics?

Rudolph Rummel, a political scientist. He was associated with a man called Bruce Russett. They were international relations students. Two or three years after I worked with Ginsburg on the *Atlas of Economic Development*, they developed something called the “Dimensionality of Nations” project. They essentially worked over the same ground, used diverse methods of factor analysis, introduced new variables, and so on. Rummel himself produced a book called *Applied Factor Analysis*. He used the output from the “Dimensionality of Nations” project as illustrative material in that book.

Do you keep the database of some of your classic articles?

No longer. I have moved several times. I could not afford all of the moving vans. Besides those old IBM cards, the magnetic tapes. They are long since useless. In many cases we reproduced the database as appendices to the published works to preserve data that way. Many of the papers we are talking about were written in the 25 years before 1980. Remember that in 1980 you had just the ARPANET connecting the major universities, before the internet formed. PCs came in 1981-3 with the 16 bit IBM-PC, and then you get

the Macintosh. It is not until the middle of 1980's that you start to get the internet with internet protocols. You have to move much closer to the nineties before you get much in the way of email. So we are talking about things that had to be stored physically. For example, when I left the University of Chicago and went to Harvard, I left in one office in the University of Chicago wall to wall of boxes of IBM cards with all the data on Chicago neighborhoods and on Chicago housing that were used in our research. I also left file cabinets with all the working materials of the work that we had done on metropolitan areas, commuting areas, and on changes in commuting. I am sure they were kept for a while but I doubt these things exist anymore.

Shooting back to 1954-5, Walter Isard created the Department of Regional Science at the University of Pennsylvania in Philadelphia. Here W. Alonso wrote his Ph. D. dissertation that developed into his path-breaking book *Location and Land Use* (1964) .

Alonso, who died in 1999, was the first PhD in Regional Science. He had the economics background to apply it in Regional Science. He moved from there to the Urban and Regional Planning Program at the University of California, Berkeley. The early part of his career was there and the later part of his career was in the Center of Population Studies at Harvard University.

What was the relationship between geographers and economists when Isard created the so-called Regional Science multidisciplinary field in the 1950s?

Walter Isard was a Harvard PhD who became very interested in international trade theory and also in long wave theory. One of his early papers was on Kuznets cycles of transportation development. He became very interested in location theory, and was quite disturbed that American economics did not concern itself with space at all. He reached

out to an interdisciplinary community; to economists like Charles Tiebout and Charles Leven; to planners like Barkley Jones at Cornell; to sociologists like Otis Dudley Duncan; and to geographers. This was in the mid 1950s. The first meeting of what became the Regional Science Association was in 1954. Among the participants in that meeting was William Garrison who spent a year actually on sabbatical at Pennsylvania. Without doubt, Chauncy Harris and Edward Ullman were involved on the geography side. Isard was concerned with building a new subject that he called regional science out of the multidisciplinary interests in space and location outside economics. He was able, largely through the force of his own personality, to create a Department of Regional Science in the Wharton School at the University of Pennsylvania. That was largely a graduate Master's and PhD granting department. Alonso was the first PhD. Various geographers at one time or another served in that department. In the early 1960's, Duane Marble, one of my fellow students at the University of Washington who finally went to Northwestern University, was there. I think Michael Dacey was there for a while; he also subsequently went to the University of Wisconsin. Julian Wolpert was there for quite a while, and ultimately ended up at Princeton.

What do you think of the New Economic Geography?

The only reason it is called the New Economic Geography is that the proponents only talk to each other. They do not read the literature; they have no sense of intellectual tradition. Paul Krugman in particular is a self-promoting hustler. From the point of view of economics in strict sense, it is a new economic geography because economics has largely ignored space. But if they ever took the time to look a little bit beyond the boundaries of economics they would find that there is an extraordinarily rich literature

and a very valuable body of research that is exploring exactly the same questions they are exploring, although perhaps they have added a new wrinkle through the use of the Dixit-Stiglitz model of international trade that involves positive feedbacks. I remember for many years economists looked down on the notion of positive feedbacks or deviation-amplifying processes, because “you have to have diminishing returns to have any equilibrium.” Many folks now know that we are dealing with self-transforming systems in which we do not have diminishing returns, but rather positive feedbacks and increasing returns. Self-organizing systems show very rapid shifts of state —very rapid chaotic transformations. I spent five years in the Harvard and Cambridge, Massachusetts, environment. I know how insulated that intellectual environment is. They do not perceive much of merit far beyond the boundaries of the city of Cambridge. Ideas only acquire a *caché* if they are expressed by one of their colleagues. I left Harvard because I was profoundly dissatisfied and uncomfortable with that intellectual environment. I thought that, while there are very able men doing brilliant work there, there are a lot of people who are simply trading on the reputation that comes from being a Harvard Professor and lack any real originality. To them innovation is what they happen to have borrowed, usually uncited, from elsewhere.

Krugman told me that Peter Dicken and Peter Lloyd’s book *Location in Space* inspired his first publications on economic geography. However, in a recent article he does not mention any geographer.³

Krugman never cites his sources. There are others things I can only say off-line. Another example is Jeffrey Sachs with his discovery that maybe the natural environment has

³ Masahisa Fujita and Paul Krugman. 2004. “The new economic geography: Past, present and the future.” *Papers in Regional Science* 83: 139–164.

something to do with economic life. He is stating as an original hypothesis ideas that have been worked out during a century of research in geography. He states his ideas in the crudest of forms. I think cynics say that too often what is viewed as intellectual progress in one discipline involves the importation of well worked ideas from other fields.

When you do not quote the sources, the new generation thinks the ideas or the index you use is your creation when in reality it is not. As an example, I read an article on regions in Europe referring to the Locational Gini Coefficient as the Krugman's index. I asked Krugman about it and he said "they wrote it; I did not." The danger I see is that if the new generation has not the proper reference in the new literature, it may consider that an idea or an index is a new creation when in reality it is not.

Krugman, in a succession of his books, keeps stating and re-stating that there are wonderful empirical regularities in the size distribution of cities, and that no one has ever explored possible theoretical bases. His claim is totally devoid of reference to an extraordinary rich, century-long literature. Already, in the late 1950s, Garrison and I, in a paper published in the *Annals of the Association of Geographers*, were exploring some of the work on skew distribution functions that were being advanced by Herbert Simon, based much upon the work of Yule (the Yule distribution). Already by late 1960s we knew we were dealing with skew distribution functions and with the steady states of very simple generating processes that are some times called Gibrat's law.

Well, something similar happened with the Theil and Herfindhal indices in economics and the Shannon-Wiener and Simpson indices in ecology, or the Shift-Share analysis in regional analysis and the Constant Market Share analysis in international trade.

There are times where some topics have great interest to people in different sub communities. They do invent these things independently. But where information is widely available and communication is easy, ignorance is not a defense.

What course did you first teach in Chicago?

The first course I taught at the University of Chicago was cartography. It is very interesting because I had never taken a course in cartography in my life. I hated working with ink on paper. But in the summer before I went to Chicago, Duane Marble and I both took a crash course with John Sherman, who was a very distinguished cartographer at the University of Washington. He brought both of us up to speed so I survived. Later, I converted it into a course in statistical cartography. I persuaded people we very much needed a good cartographer. I did not teach urban geography because the urban geographer on the staff was Harold Mayer. He had been a fellow student of Edward Ullman and Chauncy Harris. I taught the economic geography course. In fact, I do not think I ever taught a course in urban geography there. Later on, we created the multidisciplinary center of Urban Studies, so I added to my teaching responsibilities the directorship of the training programs of that center. We created the new multidisciplinary courses in urban studies. That is the first explicit urban teaching I did in Chicago. In the Geography Department I taught quantitative methods and economic geography.

You actually have a book on economic geography

I probably would not have gotten involved in writing a book in economic geography if my friends Ed Conkling and Mike Ray had not come to me. They had the idea of writing

a book that clearly needed to have a good input of economics and location theories. That is the origin of the book.

It seems that there are a couple of books updating the original version. Am I correct?

Yes. What essentially we did, what the publisher wanted us to do, is rather than having a new edition of *The Geography of Economic Systems* (1976), they wanted a revised version to become a book with a different title. That was *Economic Geography* (1987). And then, after a period of years, it became *The Global Economy* (1993), and then *The Global Economy in Transition* (1997). In each of those shifts, about 30 or 40 per cent of the previous book was brought over and rewritten. If you remember, the first book, in the first half dealt systematically with economics, price theory and location theory. The second half dealt with national economies. When the book came out a lot of the new breed of people really become upset because they did not want that regional stuff in a modern book. So we recrafted it over and over again. The fact that the four books had four different titles was a publisher's strategy.

I have read that you are disenchanted with some left wing geographers like David Harvey. Why?

There are several reasons. First, a fundamental matter of assumptions about the nature of the world. I firmly believe in the advantages, the benefits, of market-based democracies. I believe this kind of system provides the maximum opportunities for individuals who are willing to work and that it provides the environments that foster innovation, transformation, and change. The Marxist critics may be well focused if one deals with early Nineteenth Century capitalism. But their theses are quite inappropriate in a modern

world. I do not believe one should rely on the labor theory of value. There are many other things going into a production function: labor, capital and entrepreneurship. Labor itself is not simply the number of people; it is human capital. The reason we get positive feedbacks, increasing returns, is because the production function has both human capital inputs and outputs. If human capital is successfully incremented, it drives the machine. On the political side, I believe in the political power of decentralized democracies. I do not believe a particular group can select themselves as the individuals who know what is best for everyone else, believing that only they have the capability to plan and direct. I find particularly disgraceful, I have said it many times and in writing, the kind of left wing intellectual who moves from England to the USA, who lives very comfortably in the USA, making a comfortable living by being a left wing dialectician criticizing the very basis of his material confidence. It is remarkable how many left wing radicals who came to the USA from elsewhere were quite adamant about not becoming American citizens until just before the retirement age. They did it to defer the taxes they would have to pay if they were US citizens. In other words, they are not so much opposed to personal capitalistic accumulation and to personal material gain.

What do you think about the new trends in spatial econometrics, the new trends in quantitative geography?

When Duane Marble and I put together a collection called *Spatial Analysis* back in 1968 we included a couple of papers dealing with the problem of spatial autocorrelation. The problem is that if you are using geographic units (countries, states, census tracts) as units of observation, you may run into problems of statistical inference because the observations are not independent of each other. It is a problem that has been largely

ignored. Economists realized that, when they were dealing with time series data, they had to make all sorts of adjustments and changes to take care of serial autocorrelation. The spatial autocorrelation problem was pointed out but no one ever did anything about it because it raises very complicated issues. These issues could not be tackled until very large-scale fast computing became widely available. It was only in the 1990s that scholars well trained in statistics and econometrics began to take these issues very seriously. Luc Anselin has taken the lead in developing both the methods and the supporting software. Dan Griffith is another scholar making very fundamental contributions. Now we are at the stage, largely via Anselin's efforts, that we have user-friendly software available. Many more people are learning how to undertake the analysis, and many more are learning that they should check for the presence of spatial autocorrelation to eliminate the bias and inefficiency of estimates that would otherwise occur. The Center for Spatially Integrated Social Science (CSISS) at the University of California at Santa Barbara (funded by the National Science Foundation) has given some summer courses, short courses. I think there is a major breakthrough taking place right now. Tragically, much of this breakthrough is occurring in economics where economists are adopting these techniques very rapidly; in the field of real estate economics, the hedonic modeling of housing prices has been advanced considerably. Some political scientists now become well aware of the possibilities. There are only one or two geographers: the key technical advances have been made, but you do not see very rapid development at all of this new matrix of techniques within geography. I think in part it is because after the real burst of quantitatively trained people in the 1960's and 1970's the extent of quantitative training in geography has lagged. Geography is a discipline that

looks in many other directions, most of which are overtly antiscientific, antianalytic, and adoption of new technologies is therefore very difficult in departments where the other ideas are very strongly held; it has become very difficult to get young people properly, thoroughly and competently trained.

There is a problem with new generations that do not know what is behind the results of the software they use. How do you see this issue?

It is not simply in the spatial econometrics arena that this happens; it is also to a very high degree in GIS where there is very attractive software now. People can take a course and learn how to push the buttons to use the software, but they do not understand the concepts relating to spatial distribution and to surfaces and gradients, the cartographic essentials. They do not understand any of these. They simply push the buttons, get the output and that's it. This mechanistic use of software also applies with conventional statistics, whatever the package may be. I think there is an equivalent mechanistic misuse in many other arenas. If you look at the so-called critical social theorists, they also have formulaic procedures. There is a language they learn and a sequence of steps. They know that, once they deploy that language and sequence of steps, they will produce the sought-after results. Whatever is in the text is the result of the powerful screwing the powerless. The same sort of formulaic quality applies in what the postmodernists are doing. Again a language, a set of steps. Students who learn how to go through these steps, from step one to two and three, do it. Relatively few think to go beyond this to say "What the hell am I doing and why am I doing it?" Why should the ideas of a decrepit group of particular French philosophers determine my view of the world? They are now in all sorts of

disarray because one of their leading lights, Edgerton, has suddenly said “*mea culpa*”—that there has to be a reality out there, it is not all dreams.

Do you have a method of working?

Not really. Not intentionally. If there is a problem I think may be interesting, I think about it. I try to get into the literature to see what other people may have done about it. I try to identify an approach that may lead to a resolution, especially one that may lead to an extension of theory or in some other way might be an increment to knowledge. I keep on thinking and reading. If I think the resolution of the problem can best be undertaken with an empirical enquiry, I worry about data sets, whether it is something that can be addressed using official data sources or whether it requires some kind of original data collection. Then I want to get to the stage of actual research. I am happiest with enquiries that are of an inductive, synthetic kind. I have never been comfortable working in an axiomatic-deductive manner. I like working with the data, the patterns, testing inferences. I have to get close to my data. I think you learn a great deal that way. Now of course I experiment in various ways. I try things out. Sometimes I do it and set it aside.

Do you work at home?

My wife says I work all the time. I always have several projects, several things going on simultaneously. Some early in start-up, some near completion, some in analysis. And on a fairly wide range of topics because there is something about the thought process where I can be worrying about a particular topic and suddenly the ideas on another project will gel and I will say “Of course that’s what has been worrying me about that project.” It is the interplay of ideas, the tension that comes from that, I have found to be creative.

How do you combine teaching and research?

First of all you have to be very efficient. What is the first thing you notice in this office?

It is very clean, very well organized and with some empty shelves. . . .

Yes. There are no piles of papers because I have to be ordered, efficient. If I am disorderly I cannot be efficient. That is something that is very important to me. When I finally come to the stage of writing, you will see this table with various piles of stuff that represent different components of the project. I do not write to an outline. If there is a problem, I know roughly where I am going, but the working materials are shuffled as I go. I think this is where the creativity comes. I grew up in England where everything was done by hand, handwriting. When I came to the USA I learned how to use the typewriter but I cannot compose using the keyboard. I still write things on yellow pads, by hand. I have scissors, stapler and tape. So the bits and pieces change around many times and my assistant, Heja Kim, rolls her eyes when I finally give her that manuscript to process, because it is a big papyrus roll of things that have been cut and pasted together. Once in that stage, then I will take the typed draft and maybe work through it once more fairly heavily. But once it has reached that stage, it is done.

What particular advice do you have for people beginning to do research?

To develop a sense of the problem, what is the question, and to express it in a simple researchable manner. If you do not have a sense of the problem, you are going to spend a lot of time running around, going from here to there but not leading anywhere. There has to be a strong sense of problem.

What topics best describe your intellectual life?

I have none. My early work was in location theory, central place theory, theories of urban location and the spin offs; the strong advocacy and demonstration of spatial analysis; the work in urban structure and urban ecology; the work in regional development processes; the work in urban economics and hedonic pricing and, related to that, the work on environmental issues. Recently, over the past 15 years, the concentrated program of work has dealt directly with the long wave question.

Are you still working in the long wave topic?

Yes. Using some of the newest methods of digital image processing we finally were able to do some analysis that had escaped us all along because of the nature of the data, the nature of the methods. That sort of closed things out. But some political science colleagues and I have been working on some intersection of economic and political rhythms. We did one little book a few years ago, *The Rhythms of American Politics* (University Press of America, 1998). With the new analysis and some other things, I think the three of us (Euel W. Elliot, Edward J. Harpham, and me) will do continuing work on that theme. I also have had a long continuing interest in the design of pre-modern cities.

What do you mean by pre-modern cities?

Teotihuacán, Copán, Tenochtitlán, Cuzco, Chinese cities. Paul Wheatley (1921-1999), my former colleague at the University of Chicago, did very innovative work when he talked about the designs of the Chinese city. There is other work in other cultural realms. The pre-modern capital cities were basically designed as microcosms of the universe,

places joining the heavens to the underworld and the middle world, with explicit attempts to observe the natural world and to reproduce it. Arrangements in space provided ways of time-keeping. So I think I will spend some time pulling together a book. It will be called “Heavens on Earth. Time and Space in Premodern Urban Designs.”