

Peter Dicken, Economic Geographer

Peter Dicken is a product of Manchester. He was born there on March 5, 1938. He studied in this city and he still lives and works there. His first book, *Location in Space* (1972), jointly written with Peter Lloyd, has been widely recognized for having brought the behavioral approach to geography. This research interest and collaboration with Lloyd continued during the 1970s; the result was published in *Modern Western Society* (1981).

In the mid eighties, working on his own, Dicken published *Global Shift* (1986). This work, almost ready for the third edition in 1997, is used as a textbook in several disciplines such as political science, geography, economics, international sociology and international business.

Although Dicken's research interest ranges widely, his work always has a concise and strict structure. Having a long teaching experience, Peter Dicken is also a good expositor. In this interview, he effortlessly combines theoretical knowledge and experience in a clear and pleasant conversational style.



Where do you work?

I work in the Department of Geography at the University of Manchester. The university is a good place to work; it is big, it has much variety and a lot of colleagues in the social sciences and business. It is in a city I like very much. Manchester is also an easy city to get out of! It has a good airport and good connections.

What was your first academic interest?

I did my first degree in Geography. In this field, I was influenced particularly by David Smith. I was his first graduate student. He had just started his academic career. David came to the Department at the end of my first year. I did his courses and we got on extremely well. When he left for the United States in 1966, I was appointed to replace him in the same Department. Actually Peter Lloyd and I shared his course.

What influence did you have from David Smith?

Through him I became very interested in the classic locational theories of Lösch, Weber, Isard, Von Thünen. I found it very exciting because it was so different from the old style in regional geography, from the old empirical style. Until then, I was very keen on physical geography. I had done some geology and geography courses, but I got entirely switched on by this new perspective; it was new for us in 1964 or 1965.

Did David Smith have a particular research interest at Manchester?

Yes. He was very interested in regional change, sub-regional change, change in employment; his initial interest was in social indicators. He spent quite a lot of the time collecting data within the North-West region, where Manchester is located, in looking at various indicators of sub-regional variation.

What were your first steps in research?

I had a conventional geographic education, then I became very involved in the locational theoretic material. And the research I did, my first grade research, was on the garment industry

in greater Manchester. I was very interested in firms by then, in decision making. I was very swept up by the kind of early behavioral phase in geography by the works of Herbert Simon, Richard M. Cyert and James G. March. The study of this industry was a mix of quantitative and qualitative analysis. Subsequently, I became interested in firm ownership, in what happens with firms that associate together. For a long time this was one of the major contrasts I had with Peter Lloyd; he did work mainly on individual plants, establishments.

When did you have your first professional contact with North America?

In 1969. I was at Queen's University, in Kingston, Ontario. That was for four months. It was very influential for me because it was the first time I had been outside the UK as a university teacher in a different system. I found the students very different from ours. I found them more inclined to question the grades, and, once in a while, to ask why this has got to be. This is good, I think. That was not usual in England. It was a great shock; I grew up very quickly. I had to teach a class which combined undergraduate and graduate students. Master's students took the course as well with an additional seminar. This combination of students covered an enormous range of ability. It was an economic geography course. I was brought in because somebody had left early, and they had a gap they had to fill. So I filled it for four months. I learned a huge amount out of that experience. When I was back in the UK in the Spring of 1969, I had a letter from Brock University in St. Catharines, Ontario. It was not a well known university; it was very new and small one. Somebody was going on sabbatical for a year, and they were looking for a replacement, so I said, "I will go." Thus, I went back to Canada again in 1970. This time I spent 14 months there. I spent the academic year at St. Catharines, and I had the time to go back to Queen's in Kingston and to teach in the summer school there and in UBC (University of British Columbia), in Vancouver, again accumulating experiences. That was also the first time I made real contacts with US geographers. I went to my first AAG (Association of American Geographers) meeting in 1971, in Boston. I was very taken by that. It just seemed everything was excellent; it was, in a way. American geography, particularly then, was very liberated, very much leading edge. Being trained in the sixties, I certainly was very influenced by spatial analysis, location analysis and the like. And that was the time when we, Lloyd and I, were writing *Location in Space*.

What did you do after *Location in Space*?

We started a research unit based in Manchester, specifically looking into industrial change in the Northwest region of Britain. For most of the 70s I was working primarily within the local region, looking in a lot of detail at the dynamics of industrial change. My interest was looking at the firm and at firms within the region: ownership characteristics, sectoral characteristics, clustering and so on. We did a lot of work. We got a big grant from the Social Science Research Council. We produced the most detailed analysis of the region at that time. Towards the end of the 70s we had quite a lot of research money from the government. The government in Britain at that time had started to become interested in the cities, the problems of declining manufacturing cities; so they started an inner cities program in the late 70s. Each of the old industrial cities in Britain was losing manufacturing jobs quite dramatically. The inner cities program began about 1976 or 77. Among other things, the government also established a large research program. It invited academics and consultants to bid for the contracts. We had a contract for two years to study the small firm, the new small firms, their formation and change in the two big metropolitan areas of the Northwest, Greater Manchester and Liverpool. We produced the report towards the end of the 1970s. Also I had become very involved individually in looking at American investment in Britain. I found that looking at the changes in the region we discovered that a lot of the firms were owned by Americans. I started tracing that. We did a paper about it in 1976, which is probably one of the earlier ones that geographers did on foreign ownership, in our region particularly. I realized these were things I wanted to develop further. I felt I had to move outside the region. I was beginning to get interested in things more global. That was a rather unformed, very provisional sort of interest. In 1980, I decided to pull out of the collaboration with Lloyd. At that time, in order to maintain the research unit we had to go looking for more money. If I had gone down that route I would have found myself in it for another two or three years. The timing was determined by the end of one research contract. I did not want to get myself in it again. Around that time we also published *Modern Western Society*. It was a difficult birth. We decided to do it on a broader human social topic. We had different kinds of problems. We did not know what it was. I think, we became involved in other things. We had a contract to publish the book, and we didn't have anything written. We persuaded the publisher, Harper and Row in London,

to do first a second edition of *Location in Space*. We did it in 1976. This second edition was quite substantial and a much bigger book than the first edition. It was exactly the same structure and format. After that we continued the book that we had to finish.

Why do you say that *Modern Western Society* had a hard birth?

We did not show through it precisely what we wanted to do. We both drew drafts of chapters and that sort of thing. But it did not fit together. It was not satisfying. It is quite clear it is not a well focused, a clearly thought out piece of work, but I am not ashamed. It has good things. The structure is OK, but it lacks a conceptual base.

What do you think about collaboration between colleagues doing research?

When Peter Lloyd and I began working together in 1966, we collaborated very well. Around 1968, we decided to write the first edition of *Location in Space*. I was 31. The book was published in 1972 in the States. That was a really good collaboration because we, I think, sparked each other off. It is very good because with very different people you work in very different ways. It was a very complementary symbiotic relationship. We each contributed something which the other didn't have. That was good. In 1980, we stopped working together, We moved further apart academically, and we both had our own agendas of what we wanted to do. Peter was developing the research unit very successfully into a much bigger operation, which continued until he left Manchester in 1988. He is now in Liverpool. And I wanted to develop my own thing, my global interest and so on. I have increasingly felt over the years that I, in general, like to work on my own. I do not mind collaborating. I do collaborate with people still, but especially for book writing projects. I feel the benefits of being in control of the whole thing and not having to guess what the other person might be doing.

While you are working with somebody else, you already remarked on the advantage of sharing knowledge and sparking each other off. But how to deal with the inherent tension that it is in the same package? Don't you think that researching collaborative efforts are joint ventures of intelligence and dullness at the same time?

I think it is true. I can mention three things about collaboration. First, if collaboration does not work, you waste a lot of time. If it works, you gain some large benefits, there is no doubt: the

ability to share ideas to spark each other off; you think of something and . . . “I tell you,” “That is really good,” or “I never thought that at all,” or “Let’s develop it in that way.” That is great. That is the really nice part of collaboration, and I think that worked well in that first book we did, Lloyd and I, for the first edition of *Location in Space*. I think it was because we both were young, we both were starting out; neither one of us had many other academic commitments. That was everything we did, and it was great. As we got more involved in other things, and our agendas began to be diverge, the collaboration became more artificial in a way. Exactly as you say would happen: we spent hours some times, . . . “We must do this thing,” we had the board, wrote them down . . . and we did not have a significant result. In this case it is better you go away to get things on paper. No way you put them on a board in a brainstorm; you put them on paper, as a draft, and that is a big discipline. The second thing with collaborations is co-responsibility. Unless you work exactly in the same way, it can be a problem. If I am spending, say, a month working on my part of this project and you are working on something else, I will get resentful, because you should be working in this project. And then, at another time, you are working on this project and I am working in something else, you will get resentful. That becomes a very, very delicate operation. It really does. I just began to feel that, I am sure Peter felt the same. The stresses were becoming greater and the benefits were becoming on the whole less and much harder to achieve. I certainly was going to pull out, but I wanted to pull out in the right circumstances, without acrimony. We remain good friends and meet up regularly for a beer. Fortunately, even when we were collaborating, I was also publishing papers and the like under my own name. Finally, my third comment on collaboration, specially a long established collaboration, is that you lose your identity. In our case, we both have the same first name, we both are called Peter. You would be amazed how many times we were confused one for the other. So you would go to a conference or whatever, one of us, or he would go perhaps, and the chair of the session would say, “I would like introduce Peter Dicken” . . . and he was Peter Lloyd. Some times we both were a bit distressed: who am I, do I exist as an individual? or are we simply two parts of the same animal, some kind of weird mythical creature?.

Let’s talk about the recent material you have been working on for FDI (Foreign Direct Investment). Don’t you think that FDI literature is behind the reality, lagged behind?

Yes. I have made the same comment. The literature in economics and the like on FDI is all the way behind reality, in almost every way. It was not until 1960 that there was a systematic attempt to explain FDI. But Stephen Hymer's primary work was not published in 1960. It was not published until he had already died in the 1970s. All his ideas came out through other people. Most literature that attempted to understand the FDI and MNCs or whatever was heavily influenced by the work from one place: Harvard. The Harvard Business School had the multinational enterprise project with Raymond Vernon as Director. It started in the 1960s; they built a database of the biggest multinational firms. The main criteria for inclusion of these multinational firms was that they had to have operations in more than six countries. So, they selected the biggest firms, mostly American firms. It was a very good work, but it was the only available database for a long time on MNCs. It became the basis on which theoretical formulations were founded. The theoretical literature that developed was for a long time very country-specific (the USA) and dominated by very large firms. It was very useful, but it was very one sided. It took a long time to realize the diversity in the population of TNCs.

Do you mean that the database determined the theoretical approach?

Yes, I really do believe that.

What about Ronald Coase's influence?

He did not work on multinationals. He did a short paper when he was quite young. In "The Nature of the Firm" in *Economica*, he founded the basis of what became much of the analysis on internalization and transaction costs and the like. In fact, he does not mention internalization at all. He wrote on risk, uncertainty, and so on. It is interesting how influential it was a long time after he wrote the paper. It was picked up mainly by some writers on TNCs who were searching for theory, such as the notion of internalization in Alan Rugman, Peter Buckley and Mark Casson and others. In this particular area of work John Dunning has been incredibly influential. He developed his own particular framework back in the 70s. He did a tremendous job in building a conceptual framework on the internationalization of production. Even though his eclectic paradigm is essentially a set of boxes, I have very high regard for what he did.

I think Dunning's contribution is very useful. Do you think so?

That is quite right. The reason I find Dunning's work useful is that he has the courage, particularly as an econo-mist, to go outside the boundaries of his discipline. Because the existing body of theories was not satisfactory, he drew upon work in a variety of different areas. So, he drew upon international trade theory, industrial organization theory, locational theory, and other fields. He did it at a time when it was not fashionable at all, especially in economics. The main problem in Dunning's model, I think, is that you can put everything into it. But it does not tell you much about how they connect together; it does not tell you much about the interrelationships between its elements. At the very end, Dunning is a great pragmatist.

Referring to your book *Global Shift*, what chapters would you recommend for a course on TNCs?

I think that there are three or four core chapters for a course on international businesses. They are the chapters on technology, on the political system, and on the TNCs. At the moment, there is a chapter which summarizes the theories on TNCs; it is separated from the discussion of global organization and reorganization of economic activity by the chapter on the political dimension. I am going to change that in the third edition of *Global Shift*, which is in the next year, 1997. I want to make things tighter, especially those that are the conceptual core of the book.

Would you mind providing more detail on changes to the third edition of *Global Shift*?

It is a large book. The publishers do not want it to be bigger. At the moment, I am rethinking the next edition and a number of things to reorganize. One of the things that I have become more and more interested in is the interrelationship between firms and states. However, the broad frame will stay very much the same. I will make much more explicit how the TNC-states interdependency shapes the global economy and to stress more the interaction elements within the context of technology. I will also rethink the issue of bargaining, which is more important than the credit I gave it. I would certainly relocate that in a different part of the book, much more in the context of the firm-state interactions, about the middle part of the book. The strong empirical chapters, two and three, one is on trade and the other on investment, I am going to compress into one chapter and I am going to summarize them a lot more. I can make more room

elsewhere for additional material. I want to put in other parts of the book things other than simply moving things around. The structure will remain the same; it works. So I am going to change the order of those conceptual core chapters, as I said before, take some things out and writing some more things in.

***Global Shift* covers a wide range of topics. How did you prepare yourself for such a broad intellectual task?**

When I stopped working on the local material in 1980, I had a couple of years where I did not do anything much more than read and think; I did not write very much. I worked a little, just trying to reorient myself. That preparation was not particularly directed; I just was aware that I needed to collect data, obviously, that is straightforward. The main problem was trying to develop an explanatory framework. I spent a lot of time on that. I spent four more time trying to develop the structure than I probably did in writing the book. Once the empirical data were collected (it took a lot of time) and I had the framework, I could write it relatively quickly. The frame was the problem. It was trial and error, without any doubt at all. When I decided to do the book, I did not have any conception of the risk I was taking. I knew what I wanted to do, but I really did not grasp the scale. If you start writing something with “global” in the title, you suffer to produce such immense scope. In the first edition, I did not really fully appreciate what I had done at the end of it. I will tell you what I mean by that. Occasionally people tell me about things I wrote, and I think “Did I really say that, did I really make these points?” So I learned quite a lot from what people said I had done . . . Sometimes the best person to judge is not yourself, but somebody from outside.

Do you remember which chapters came first?

I think I wrote it through more or less consecutively, more or less. I am sure I did.

Do you find different the Peter Dicken in 1996 from that writing *Location in Space* in 1972?

I can recognize myself in the 1970s. I do not think I have changed that much in the way I think. I like to believe I continue to develop intellectually; I like to think I continue to be open to ideas, be responsive to change. At the same time, I also like to think that I have pursued, at the core of what I do, a fairly consistent line: this particular interest in firms. When I see many people

moving away from what they were doing, I think “Maybe they are right,” but I feel encouraged by the kind of things I always I have been interested in for 25 years or more. My interest is much broader than before, for sure. But I can recognize the central thread. If you put it the other way around, if you had asked me in 1972 where I thought I was going academically, what I would do in the next ten years, I would probably have said: publishing a book on the Northwest of England.

Are you now less ambitious than in 1972?

I was never ambitious, honestly. Partly because I came into academic life older than most teachers in England. I came into it by accident; I never intended to have an academic life. I came into the system in 1966 when in Britain there was a sudden opening up of job opportunities in universities. There was a whole number of universities being formed; the system was expanding. I had offers of three jobs. I went back to Manchester. I was very lucky, I got into the system really by accident; I did not see myself as being driven by such thoughts as, “I must achieve this,” “I must achieve that.” I just came into it, I liked it, I enjoyed what I did. I really enjoyed researching and writing. I have never been ambitious in the sense of deliberately seeking out promotional things; I *have* been ambitious in the sense of wanting what I do to be respected. My ambitions are to do what I have been doing, to interact with people in different environments and to gain all the benefits of an academic existence without being a closet academician.

What is your daily work routine?

I am a creature of habit and routine, a fact which amuses many of my colleagues. I have a very clear routine. I go into the Department every day, partly because I am chairman, head of the Department, so I need to be there anyway. But I probably will do the same in the future. I get into the Department about 7:15-7:20 a.m. I am the only person there at that time, usually. I make myself some fresh made coffee, very strong, I get out my *Financial Times*, I put on Radio 3, which is the classical music channel, and I drink my coffee. I read through the *Financial Times*, and I clip out material I am interested in, while I am listening to music. There is no telephone. That occupies me that part of the morning. I file my *Financial Times* within a huge system with material from it and the *Economist*. Then, either I continue reading or I write in the mornings as

a rule. I have found the mornings up to lunch time the most productive. I have found it much harder to write creatively in the afternoons. I eat a banana about 10:20 a.m. I probably would have a cup of tea with colleagues at the Department by 11:00. Then, I have an early lunch by 12:15. Since I have a very early breakfast, by 12:15 I am hungry. I only eat a light lunch; I do not have a big lunch as you do. Then, I often spend an hour reading some newspapers. I go back and do most of the routine things in the afternoons. I have tended to do routine things in the afternoons: collect some data I need from the libraries, maybe read some academic papers, or just revise material I have read in the mornings. I do not do many new things in the afternoons. Sometimes when I am under pressure I do. I find I need a very clear, systematic way of working. You have to find the way that works for you. I have a colleague who starts working at 10 o'clock at night and works until 3:00-4:00 am. I cannot do that at all. I finish my working day about 6:30 p.m. and get home by 7:00. Nowadays, I rarely work in the evenings. I used to work in the evenings. I still do some work on Sunday mornings, but I do not work on Saturdays, ever. I like to have a day with nothing at all. I do not do as much on the weekends as I used to do. Now I begin to feel "If I work a pretty long day from Monday to Friday, if I am working efficiently, I should have enough done by that time." If you do this kind of work seriously, you will find a system that works for you. And that works for me.

Do you have a specific place and methodology to get your ideas?

There are all kinds of different places. While I am shaving; very rarely during the night. I do not often wake up. I know people that, in the middle of the night, get a pad and write down ideas. I do not have wake ups, so I do not do that. When I am driving from work. It is kind of random. What I do find is that with the usual problem of dealing with a blank sheet of paper (which I still find very hard), I pretend that I am not really starting. I just scribble a few things down. Sometimes you already write a page and you think "Well, I have stuff that is working." It is awful if I have to sit down and start a paper or chapter *now*. I cannot easily do that. I am a creature of the precomputer age. I only recently got to the stage of writing directly on the screen. Of course, I have some previous drafts. Whatever I do, I spend a lot of time planning. It reduces the blockage. If you produce a very detailed plan, you are a long way along the road to

producing the essence. All you have to do is fill in the details. Maybe I have planning blocks more than I have “writing blocks”. When that happens, I do something else; that usually helps.