The Early Days of Architectural Psychology in The United Kingdom

An Intellectual Autobiography

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Abstract

The personal history is described, stating in the late 1960's, that gave rise to an involvement in architectural, and later environmental psychology. This includes both the research activity and the emergence of a theoretical perspective of people's active involvement in their surroundings that has been the basis of subsequent research and professional activity. The impact of particular areas of application, notably the study of human activities when caught in a building on fire, and other emergencies, facilitated the emergence of the concept of environmental roles and rules, which enriched the developing Psychology of Place theory. This autobiographical account leads to the unexpected involvement in the early 1990's in contributing to police investigations. As in all autobiographies it is a work in progress, looking back in order to gain some understanding of the future.

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BRIEF SUMMARY OF EARLY BACKGROUND

When there were riots in Liverpool's Toxteth district in the early 1980s, they led to the closure of the bank I and my father before me had used for over 30 years. This brought to an end the last remaining contact I had with a small area of Liverpool in which I had been born on January 5, 1944, where I had gone to the Hebrew Primary School and the Collegiate Grammar school. The same square mile also housed Liverpool University, where I had obtained my undergraduate degree in psychology in 1964 and my doctorate in 1968.

Like many Jews in Liverpool, my father had arrived there around the time of World War I, escaping from the pogroms in Lithuania, with every intention of going on to the United States. His painful life, including the murder of all his close relatives by the Nazis, culminated in his marrying my mother who had been born in Liverpool, but whose father had come from Russia. My Liverpudlian roots being so shallow, it is not surprising that in 1966, I took the opportunity to join the Psychology department at Strathclyde University, in Glasgow, and the year after to move to the School of Architecture in the same university, as a member of the Building Performance Research Unit.

In Glasgow, I was joined by Sandra, who had been doing psychology with me at Liverpool (she did rather more than I did because I spent so much time in the university dramatic society). Sandra had completed an outstanding MSc in clinical psychology at Queens University in Ontario before coming to Glasgow. She went on to do a PhD at Glasgow University on schizophrenic thought disorder, while working as a clinical psychologist. Over a 10-year period, she developed her career as a clinical psychologist as well as giving birth to our three children. Our first child, Hana, was just 6 months old when we went to Japan in 1970 on a Leverhulme Fellowship. On our return we spent a year in Glasgow, where Daniel was born. Then we moved to Guildford, where I took up a lectureship in the psychology department at Surrey University in 1972. We managed a sabbatical at the University of California-Berkeley in 1980 a few months after Lily was born, which makes her virtually the same age as the Journal of Environmental Psychology, which Ken Craik and I got moving while I was on sabbatical.

The lack of movement in the British university system kept me in Surrey for as long as I lived in Liverpool, being appointed as a Professor of Psychology and then Head of the Department in 1988. Being less than one-hour's journey from the centre of London undoubtedly contributed to the very many research opportunities I had whilst living in Surrey, from government departments, industry, commerce, charitable bodies, and research councils.

I enjoyed horse riding, and in the 1980's started to learn the clarinet. I even had an exhibition of my collages in the University of Surrey gallery in 1988. Exposing myself to that kind of public scrutiny was a good preparation for writing the present chapter.

EARLY ACADEMIC DEVELOPMENTS

Any account of a natural process must be a simplification. The written word can only sketch the variety that is integral to growth and change. This is true whether it is a garden that is being described or a human career. But for a career there is a further distortion. The sequence of activities that intertwine to make a period in a person's life when written as a history has far more shape and direction to it than it ever had at the time, when it is being experienced. Certainly, for me, setting out to produce an intellectual history of myself, I am aware that the history I am about to describe, as confused as it may be presented, will appear far less haphazard than it felt at the time.

The arbitrariness of the emerging story line may be gauged by considering the research contracts for which I was responsible in those early day in Surrey. These ranged from studies of the experience of homoeopathy to examination of the behaviour of serial murderers and rapists. They included studies of safety in the steel industry and the design of psychogeriatric facilities. All these projects have roots in my earlier work in architectural psychology, even though those roots may be confusingly entangled in a disordered undergrowth.

OFFICE SIZE

The profligate diversity of my research started from an unambitious PhD on the effects of office size on worker performance. What the PhD had in common with nearly all my later work was a determination to use field-based methodologies to develop psychological theories about environmental actions and experience. In this sense the 'architectural'

aspect of this is the need for the research to be embedded in some recognisable physical context.

A predilection for using multivariate statistics as an aid to the development of these theories was also present from my earliest studies. Curiously, though, I had found my way into the study of office size from an undergraduate degree in psychology at Liverpool University. The Psychology department at Liverpool was steeped in the experimental tradition of British psychology, but through the guidance of its head, L. S. Hearnshaw (made clear in his *History of Psychology* 1987) and other members of staff, notably D. B. Bromley (as revealed clearly in his book on *Case Study Methodology*, 1986), there was a productively eclectic debate about the nature of psychology and appropriate directions for its growth. I had wished to follow my personal interests in art to study empirical aesthetics for a doctorate, but the only opportunity available to me was to join the Pilkington Research Unit in Liverpool University's Department of Building Science. This multidisciplinary team was led by an architect, Peter Manning, who had written on architectural education and systematic design procedures. His objective was to develop appraisals of all aspects of a building's environment. He brought a geographer and a physicist on to the team as well as a psychologist, Brian Wells, who was studying the psychological implications of open plan offices (Manning, 1965).

In effect, Brian Wells supervised my PhD, which was nonetheless registered in the Department of Psychology. Thus, my existence with feet in more than one university discipline was presaged from my earliest days as a researcher. The Pilkington Research Unit encouraged me to move away from a focus on aesthetics and look directly at the implications of office size for worker performance. At times I feel that my subsequent research has been a struggle to return to my original interest in how the physical phenomena that are artistic productions can have such a significant emotional impact[1]. The office research convinced me that field research explores a different class of phenomena to those, so popular amongst psychologists, that are studied within the confines of the experimental psychology laboratory. Although there can be fruitful interactions between laboratory and field studies, they should not be misconstrued as studying the same thing.

My own interests have always been in what people do in their daily lives rather than in what they can do if a psychologist asks them. I think that this perhaps also has some roots in my experiences as an actor and producer in student drama when I was an undergraduate. It became very clear to me that people have a huge flexibility for generating actions under training and instruction. The laboratory experiment really examines the range and limits of this flexibility. The study of offices taught me this. I had a simple stimulus-response idea of how buildings have their influence. This led me to set up the study to examine directly the impact of office size on the performance of clerical workers. The results showed that people in their own small offices were performing better than people in their own large offices but that this effect disappeared when people were tested in other people's large or small offices. This finding was difficult to understand as a direct effect of office size on performance.

When I stopped considering the results as revealing the effects of the office size on the workforce and started looking at them as an indication of the type of person who would accept, or stay in, a job in an office of a particular size, they made much more sense. Looking on the subjects of the research as actively part of their context, selecting where they would work (or at least being selected), rather than passively being influenced by the room made the results quite comprehensible (Canter, 1968).

Better, more committed clerical workers were more likely to be found in the preferable smaller offices. Yet this active, context-specific interpretation could never have been gleaned by asking people to rate pictures of offices presented to them or other laboratory techniques, unless they were asked to say if they would be prepared to work in such a room. That question, though, touches on the wider significance of the design. Its meaning to the respondent as part of their lives, rather than as a 'stimulus'.

ROOM MEANING

My experimental, mechanical, origins in psychology did not fade away too rapidly. After the office research, I thought (as many researchers still do) that I could study the meanings, implied by the differences between the people found in different rooms, in a systematic, controlled way. So that when Roger Wools, an architect, joined me to do a PhD under my supervision, together we continued with simple laboratory studies. We wanted to look

1 My current study of music composition is an integrated exploration of that original fascination!

at which aspects of buildings held particular meanings for people and used a classical, factorial experimental design in which types of furniture, ceiling angles, and window sizes were modified in drawings and photographs of models (Canter & Wools, 1970). These studies showed very clearly that people did associate sloping ceilings and easy chairs with room friendliness. But although a few doctoral students attempted to follow this idea directly, they found that it was not really possible to establish a vocabulary of forms, whereby certain physical constituents could be linked to particular responses. One reason was a methodological one. The experimental design quickly becomes very complicated and unmanageable if a large number of aspects of form are explored. Yet the need to explore interactions between aspects of form mean that a series of simple experiments are likely to prove inconclusive.

Another reason for the difficulty of developing a vocabulary of meaning of building forms was more closely tied to the psychological processes revealed by later studies. The meaning of the forms is specific to context and culture as well as relating closely to respondents' reasons for judging meaning. In other words, just as office workers' responses are a function of their position in the organization, so the ratings of pictures relate to the particular type of experimental/subject role that the respondent is taking. This continues to be a challenging area of architectural psychology research. But it is noteworthy that most of the people who have started to explore this avenue have moved on to quite other research questions, usually more distinctly field based. Even those who set up major laboratories to create simulations of environments to study have changed the way these simulations have been used and distanced themselves from the mechanical stimulus/response examination inherent in looking at which architectural variables "cause" which semantic differential responses.

It was about 15 years after I supervised Roger Wools's thesis that I was able to work with Linda Groat, who, having a design training initially, asked very similar questions to Roger but who was able to benefit from the work that had been going on in the interim. In supervising her MSc (published in part in Groat, 1982) and PhD thesis (Groat, 1985), it was possible to work on nonexperimental approaches to architectural meaning. That work helped to establish an approach very different from the semantic differential and the factorial design models that Roger Wools had worked with (Canter et al., 1985). It gave rise to work that was published in Progressive Architecture (Groat & Canter, 1979), a rare acceptance by the architectural profession of findings from an uncompromising piece of architectural psychology.

The study of environmental "meaning" as it has been called has continued to be a recurrent theme in my research. As an undergraduate, I had been very interested in empirical aesthetics. At that time, in the mid-1960s there was virtually no literature on the topic, and what there was appeared to be mainly the discursive writing of retired professors. It not being a fit subject around which to build a career. But I had carried out my own undergraduate projects on Christmas card selection and the judgments of paintings. These studies, cast in a quite strong experimental tradition had been unsatisfying, but I had wanted to take these studies further. The opportunity to join the Pilkington Research Unit had therefore been seen as a way of approaching aesthetic issues through the architectural context.

But I have also thought that the roots might have been even deeper than an interest in the arts. It dawns on me that the search for significance beyond mere signs, attempting to reach for more symbolic aspects of the environment, may well have been laid in my study from the age of 10 or so of Talmudic interpretations of the Bible. To be introduced to the possibility, at such an early age, that words can have layers of meaning that can be peeled back, or like a Swiss Army knife, have hidden within them an unfolding range of tools and applications, did, I think, prepare a way of thinking that has remained with me ever since. Indeed, the facet approach that I came to much later can be seen as a scientific procedure for generating hermeneutic frameworks.

THE ARCHITECTURAL CONTEXT

We are all conduits for the ideas and actions of others. So that one of the illusions my personal intellectual history could create is that my actions in some way can be clearly distinguished from the actions of others. This, of course, is far from the truth. Peter Manning and Brian Wells both set the agenda for my PhD work, and although I was supervising Roger Wools, he taught me much of what an architect strives for in psychological research. The research that was my main activity at the time that I was working with Roger was also shaped by the perspectives of others. This was the development of building evaluation procedures and their use in the evaluation of the newly emerging British 'comprehensive'

schools. My work on offices was conducted as part of the "total environment" evaluations of the Pilkington Research Unit at the University of Liverpool. That unit had pioneered the use of building appraisals as a contribution to design. Following on directly from it, Tom Markus established at Strathclyde University, in Glasgow, the Building Performance Research Unit. It was as a member of that unit that I found myself supervising Roger Wools. In 1967, it did not seem as strange as it might today for a psychologist to join a research team in a school of architecture. The quest for interdisciplinarity was still strong then. Tom Markus brought together a team with very varied backgrounds. Tom Markus brought a rare combination of expertise to lead the Building Performance Research Unit (which published a book, BPRU, 1972). Trained as an architect, he had completed higher degrees in both architectural history and building science. We were joined throughout the 5 years of the team by Tom Maver, who had a degree in mechanical engineering with postgraduate research in service engineering, and Peter Whyman, an architect with a particular interest in modular design.

Tom Markus brought the team together to develop architectural evaluation, or performance, procedures that could be widely used. Once the team was together, we all soon agreed that we had to know what was being evaluated before evaluation procedures could be developed. Thus began the continuing debate on how to conceptualize buildings and where to find the appropriate criteria for their assessment. I had published a couple of papers while still a student with the Pilkington Research Unit. One pointed out that building appraisal procedures could learn a lot from psychometric concerns about reliability and validity (Canter, 1966). The other was a first attempt to outline a theory of what the function of a building was (Canter, 1970). It should be remembered that in the late 1960s when these papers were written, the architectural slogans of "form follows function" in praise of the International Style was still the dominant fashion. Postmodernism and the associated discussions of architectural meaning were unheard of. Consequently, to suggest, as I did, that one of the functions of a building was to provide meaning was treated as fairly radical.

The burden of my earlier arguments had been that the central function of buildings was to provide appropriate contexts for people, an idea that had certainly not been accepted within architecture and one that was challenged in the late 1960s. Architecture was heralded as an 'art' form, or just some functional means of housing activities. The idea that buildings carried meaning and significance beyond their aesthetic contribution (whatever that was?) was not understood.

The work of the BPRU gave me a chance to take that idea a step further by asking what it meant to evaluate a building when its function was seen in human terms. The answer to this question required some view on the nature of people, and this was where my perspective on the active, context-specific use of the environment had its influence. Drawing on my office research, I took a broadly organizational view on building use and proposed that evaluation was an indication of the extent to which a building enabled people to achieve their objectives. It was a number of years later that this idea was developed into the model of purposive evaluation (Canter, 1983), partly because at this stage I was still reliant on statistical models that constrained solutions as distinct, orthogonal dimensions; yet what I was studying was a system of interrelated components.

The list of variables produced from factor analysis has really been absorbed into more complex later models. But one particular aspect of the BPRU work did encourage me to take the more active models of human experience of place even more seriously. Peter Whyman and I had noted how many of the new school buildings had undergone changes to their fabric and use in the few years since they had been first occupied. He had called these modifications improvisations and had noted for a number of school buildings that the changes varied from major alterations, such as the addition of new classrooms, to minor changes, such as the redesignation of room allocation, with sealing up doors or moving walls as more intermediary levels of change. We wondered what the consequence of all this improvisation was. A simple environmental effect hypothesis would suggest people were reacting to poor conditions. A more active hypothesis would suggest that they were positively making sense of their buildings. It was possible to test these opposing hypotheses because we had building evaluations of the schools and we were able to derive scores for the amount of improvisation that had been carried out. The result was very clear: a significant positive correlation between degree of satisfaction and degree of improvisation. I took this to support the active hypothesis. Unfortunately, no one has been able to replicate this study. It takes a dedicated architect and a large-scale survey to make it possible, but if the result could be reproduced, it would have enormous implications both for architectural psychology and for approaches to design.

Towards the end of my time at the School of Architecture at Strathclyde University, my research activity had provided me with some basic principles that my subsequent research struggled to make sense of. These may be summarized as follows:

1. Architectural psychology had to be carried out in existing environments. Too much is left unsaid and unstudied if it is moved into the abstractions of the laboratory.

2. The environment is not just a useful base for research with complex variables. It provides a *context* for examination that has to be studied in its own terms.

3. The environmental context cannot be approached devoid of any world view or meta-theory. A perspective that searches for the role of human agency is most likely to be fruitful.

4. But human agency itself implies that people have some understanding of their environment and its significance. Examination of people's experience of environments must therefore include exploration of what is signified by them as well as how people evaluate their contribution to their own actions.

THE ARCHITECTURAL CONTEXT

By 1970, I had become convinced that psychology had much to offer architecture, especially architectural education. As part of my job in the School of Architecture I had set up a variety of courses, so that students studied various aspects of psychology in every one of their 5 years. Increasingly, I had found that as the Architectural Psychology literature had been developing, architecture students needed some background in psychology in order to understand the advancing field of research. But none of the existing psychology texts answered their needs. I therefore set about writing Psychology for Architects (Canter, 1974). I mention this because, although I now regard it as being very dated in its account of psychology, it continued to sell a few copies each year for the almost 20 years it had been in print. It therefore continued to answer some sort of need, serving to show that psychologists can be too ambitious in what they aspire to give to designers. This book contains virtually no "architectural psychology," just an account of psychological ideas with architecturally relevant examples.

THE ARCHITECTURAL CONTEXT

The seeds of my subsequent projects can be seen in the principles and emphases of Building Performance (BPRU, 1972) and other publications from the late 1960s and very early 1970s. Certainly, if in those days, I'd been asked if 20 years later I'd be working with the Salvation Army on hostel design, I'd have said I hope so. But behaviour in fires and emergencies would have been more difficult to foresee, and the eventual involvement with the police on offender profiling would have seemed beyond the scope of our theories and methods. Two nascent themes already present in the late 1960s, but the significance of which I had not recognized then, can now be seen as directly pertinent to later directions that my work took. One of these themes was the drift from an individualistic to a social psychological context for considering architectural experience and meaning. The other was the need for methods for constructing theories and the associated analysis systems that would help in finding patterns in data harvested from 'the field'. So that when the opportunity arose of spending a year in Japan, I was already primed to be sensitive to a number of possibilities that later dominated my research. The undemanding fellowship to Japan was of particular significance in that it virtually shocked me into seeing the power of culture on all aspects of behaviour, especially the way people deal with each other and make use of their surroundings.

Living in such a different culture it became clear to me that the significance of a place was not some reflection of the external physical parameters that characterize that place. That significance derives from the cultural framework within which a person experiences a place. These are reflections of the way they see the world and think about it.

ETHNOSCAPES

It was a number of years before the Japanese experience really surfaced openly in my publications. It was certainly one of the reasons why I was so keen to include regional reviews in the *Journal of Environmental Psychology*, a development that was clearly seen to be of value because the distinguished editors of the *Handbook of Environmental Psychology* later copied the practice. Even more directly, the series of books I established

with David Stea, Ethnoscapes: Current Challenges in the Environmental Social Sciences (Canter et al., 1988), make explicit the need for environmental research to embrace cultural diversity. This is not just a matter of including cross-cultural comparisons on the research agenda but of integrating studies in different national and subcultural contexts within the framework of research activities.

One important example of this context aware approach is allowing research questions to be defined by local, cultural imperatives, rather than by some reference to the current intellectual fashion in North America. This series had truly transnational roots, evolving out of meetings I had with David Stea in Indonesia and Venezuela and Martin Krampen in Germany. All three of us were aware that there was a changing mood in environment and behaviour studies being reflected in conferences around the world. Yet the old vocabulary of environment, behaviour, architecture, psychology, and so on was masking these changes. We therefore deliberately set out to coin a new term that would reflect the new sensitivities of researchers in many countries and to launch a series of books that could act as a vehicle for publishing this research. We defined Ethnoscapes as:

"*Scholarly and/or scientific explorations of the relations between people, their activities and the places they create and/or inhabit; historical, psychological or sociological studies of the experience of places, attitudes toward them, or the processes of shaping, managing or designing them*" (Canter et al., 1988, p. xi).

To some extent, the growth of our field beyond the North Atlantic Basin has naturally led to a greater cultural diversity in the studies being carried out, with, I think, enormous long-term benefits to the field. But I was also made aware, in Japan, that the cultural divide could be bridged in some ways by the written word. I was really surprised to find copies of my early papers already known and translated in Japan, being quoted and drawn on, even if inappropriately. An intellectual imperialism can be rife without really intending it. The experience of living in an unfamiliar large city also alerted me to environmental psychology issues at a planning scale, which I had never really explored before. In particular I was aware that Tokyo was such a complex city to find my way around that I became interested in how that was possible. Route finding appeared an inappropriately simple-minded, and practically extremely difficult way of exploring the basis of urban navigation. I therefore started asking people to estimate "crow flight" distances (although in one study that I supervised in Japanese this got lost in the translation and the respondents ended up giving me shortest walking route distances!). I had begun some similar, tentative explorations in Glasgow before going to Japan, but I was surprised by how accurate people could be in a city as complex as Tokyo.

On my return to Glasgow, I worked with Stephen Tagg and to explore this further (Canter & Tagg, 1975) and became aware of the power of dominant features such as the 'circle line' of Tokyo's underground system and the Thames and underground train network in London. Clearly, people form some sort of composite conceptualization of a city that they use to act on. This is more pragmatic and individualistic than Lynch's 'image/' although it clearly relates to it. But it was not until my return to Britain and my move to Surrey University in Guildford, near London, that I was able to develop these ideas much further. I also needed the opportunity to get to know a strange building in depth in the way I had got to know Tokyo.

EMERGING CONCEPTUALIZATIONS OF PLACE

On my return from Japan, I had a unique opportunity to study the Royal Hospital for Sick Children at Yorkhill in Glasgow. I was able to spend a great deal of time over 6 months, with assistance from students and colleagues, examining the new building at the request of the *Architects' Journal*. The editor had requested the study because he felt that a children's hospital should not look like a multistorey office block and he wanted, I think, a psychologist to confirm this. The intensive study I was able to conduct (Canter, 1972) was close to an ethnographic account of the building and quite unconstrained by any limitations as to how it should be done. I interviewed whomever I could, carried out behavioural mapping studies, and got people to complete repertory grids and questionnaires.

Probably the most valuable aspect of the work for me was the training it gave me in what a building is and how it is shaped by many forces. I certainly learned more about the real world or architecture in that study than I had in the previous five years in a School of Architecture. The study helped me to develop a number of ideas for which I had been reaching. Three in particular are worth noting at this stage.

- First, how a building is created, the socio-political and economic processes, as well as the design intentions, is very important in influencing what results. This will seem obvious to any practicing architect, but it is a point that is too often ignored in the architectural psychology literature.

- Second, by being able to explore in detail, with a number of people, their views and experience of the building, it became very clear what large differences there were between them in what they saw the building as being and, as a consequence, how they evaluated it. The major difference appeared to be a function of what they wanted to do in the building, what they were in the building for. This I summarized as 'role differences' in conceptualizations.

The third idea to emerge more strongly from the Yorkhill study had been presaged a few years earlier in a paper entitled "Should We Treat Building Users as Subjects or Objects?" (Canter, 1969) in which I argued that, to get a full picture of the psychological implications of a building, we needed to combine observation of buildings in use with explorations of the significance of those uses to the users. The intensive Yorkhill study, using a mixture of very different methods of data collection, also forced me to accept that the experience of the building was reflected in the combination of actions and conceptualisations. By carrying out behaviourally oriented studies following Barker's ecological perspective, in combination with personal construct studies following Kelly, it was clear that both had something to offer and any future development must find ways of combining these two very different perspectives.

Barker had ignored the interpretations of the people being studied, and Kelly's intense clinical perspective seemed inappropriate for the essentially public and social qualities of a building. Taken together they could leaven each other's weaknesses.

A STUDENT QUEST

Soon after the Yorkhill study I moved from Glasgow to Surrey. (The final draft of the special issue of the *Architects Journal* was written in the greenhouse of my new Surrey residence because there was still no furniture in the house.) At Surrey I joined a new, rapidly developing psychology department. There was something of a culture change as I rediscovered my psychological roots and also came to terms with the difference between the south of England and Scotland. Yet, with the foolhardiness of youth, being in my mid-20s, I quickly (possibly too quickly) established the graduate program in environmental psychology, the first entry of which was in 1972, which is still attracting students from around the world half a century later.

In those days, there were almost no books in the field, and most teaching was done from photocopies of articles. Therefore, one of the first major tasks was to write a text book for the course. This text I put together with Peter Stringer. It was called *Environmental Interaction* (Canter & Stringer, 1975) in order to emphasize the significance of what people brought to their surroundings as well as what consequences the environment had for people.

In order to organize the course and the book, a simple framework was needed that would capture the range of material that we wanted to cover. It seemed reasonable to choose environmental scale as the structuring component because this also provided increasingly complex phenomena to deal with, starting with heating and noise through to building use and on to the urban scale and landscape. In retrospect, this appears far more of a theoretical statement about the psychological processes involved than it did at the time. By eschewing the psychologist's approach of dealing with supposedly "fundamental" issues such as perception and learning, before moving on to matters like social processes, we made a stand on the integrated nature of environmental experience, showing that differences of the scale of variable dealt with may change the complexity of the interactions under study but do not necessarily change fundamentally the psychological processes involved.

This idea was to see light in a much stronger, more theoretically articulate form in my book published a couple of years later, *The Psychology of Place* (Canter, 1977). The postgraduate program, especially the dominant 12-months' master's course, had a direct impact on the development of my thinking. The challenge from students to put ideas into a more coherent framework, as well as the rapid evolution of ideas brought about by postgraduate dissertations being produced by a cohort of 10 or so students every 12 months meant that I was now exploring in publications ideas that had been superseded by subsequent student activities. Some scale of this problem can be gauged by the fact that there are now hundreds of Environmental Psychology MSc dissertations in

Surry University and more than hundred PhD. At least a quarter of these contains material well worthy of publication, but even today very few have seen the light of day in formal publication. This very poor rate of publication against a background of a full library of theses has the strange consequence that master's and doctoral students at the University of Surrey have access to a rapidly evolving body of knowledge. They can learn a great deal from work completed only a few months earlier, but unfortunately this creates a sort of hidden school of environmental psychology that the outside world catches curious glimpses of. It is like a medieval monastery with its illustrated scrolls available to its residents. I have been told that other graduate schools in our field suffer in similar ways. It is partly a function of the employability of our graduates. They are so quickly taken off into practical jobs that they have no time or inclination to write up their dissertations for a journal. It also serves to show why productive new developments in our field can take so long to spread. They really have to wait their turn in the queue before time can be found to give a public account of them.

THE THEORY OF PLACE

By the mid-1970s, students on the MSc course were pressing for some coherent, theoretical account of where I stood in relation to environmental psychology. It was probably clearer to them than to me that the type of research I had done and the context within which I had done it made it rather different from the essentially U.S.-based texts they were reading. These differences were not characterized by a total repudiation of U.S.- based empirical research, but it was possibly confusing to students that I found such a mixture of good and bad in apparently different traditions in U.S. research. For example, with hindsight, I wonder what they made of my strong criticisms of the behavioural tradition in U.S. research and its associated S-R models of environmental impact, yet my obvious interest in the general value of Barker's ecological approach (Barker, 1965) that so self-consciously focuses on behaviour.

They were possibly confused further by my arguments that Kelly's personal construct theory (Kelly, 1955), with the importance given to individual interpretations of experience, was not only of great potential significance to environmental psychology but actually complemented Barker's approach. The pressure from students for me to organize my ideas in a way they could grasp, together with the Japanese experience, the Yorkhill study, and the distance estimation studies, became the basis for an attempt at an outline of an environmental psychology theory, which became my book *The Psychology of Place*[2] (Canter, 1977).

The book explored how the complex process that shapes our surroundings could be influenced by a psychological perspective. Further, the need to deal with different environmental scales, made clear to me in producing Environmental Interaction, was a further specification for designing the book. The need to take human objectives into account was implicit in much of the book, stemming from my office studies and the BPRU work, but because the book was written very much with students, rather than researchers in mind (remembering the apparent value of Psychology for Architects), it became more of a descriptive text than an articulated theory. Nonetheless, *The Psychology of Place* does articulate a model of environmental experience with which I am still reasonably comfortable.

The writing of that book was the most personally valuable course of study I have ever undertaken. Indeed, in working on a totally revised second edition, I realise how much the personal development that I experienced when writing the book is reflected in the unfolding story line of the chapters. It is the final chapter that reveals the nub of the book because it was only really at that point in writing it that I began to become clear in my own mind what the book was aiming at, although this is not really the best place to put the most significant part of any book.

Two fundamental challenges of architectural/environmental psychology emerged when developing the book. One is the empirical fact that the physical environment can only be shown to have any strong impact at the margins of physiological tolerance. Any other significance of variations in the environment can be readily swamped by social processes and human determination. Yet a great deal of effort and resource goes into shaping our surroundings. One task for architectural psychology is to resolve this paradox of why resources are spent on something that does not seem to produce direct measurable effects on behaviour or performance. The second difficulty stems from the first. How can psychological involvement contribute to the improvement of our

2 The opening chapter that I have put online is still, to my amazement, the most highly read and cited of any of my publications. That is why I am currently working on a total revision of it 45 years later.

surroundings? If social processes and personal expectations are so much more important than any direct impact of the surroundings, how can we make recommendations about the form, shape, or characteristics that those surroundings can take? Talking in general terms about design flexibility, individual variation and social constraints do not really give an architect anything very specific, or concrete, to go on. In considering these issues, they seemed to me to be so fundamentally difficult to resolve that the questions themselves must have some basic illogicality in them. It was out of these reflections that I began to think that taking the environment as an entity distinct from behaviour was the flaw.

A unit of focus for research was needed that adjusted the emphasis. The idea of a *place* as that unit seemed worth exploring. This 'place' became a system that integrated physical and psychological aspects of experience. Research therefore needed to discover the structure of places. Contribution to design became participation in the shaping of these structured systems. This introduced the idea of 'place making', which has now become a commonly accepted approach to design.

When *The Psychology of Place* was published, there were very many loose ends to the model outlined there and some fairly fundamental ambiguities in what was being proposed, but it took a variety of further studies to identify these clearly and begin to tidy them up. In doing so, certain aspects of the model that were not emphasized in the book turned out to be very fruitful and have taken on much more significance in later writing. This includes *role differences* and associated *rules* of environmental use. These became specified as 'environmental roles' and related 'place rules'.

THE JOURNAL OF ENVIRONMENTAL PSYCHOLOGY

Writing *The Psychology of Place* and the associated reading and discussion with students had alerted me to the fact that there was a strange hiatus in publications in our field. The only major journal, *Environment and Behavior*, deliberately had the important objectives of communicating across disciplines and making direct contact with policy issues. Furthermore, because so many researchers carrying out applied studies, in effect, published mostly for the non-specialist who might act on their results, there were very few opportunities for researchers to present to other researchers intensive, academic accounts of their work.

It is essential that there is a debate between experts at the most demanding intellectual levels, the theories, methods, and results out of which our discipline is evolving. After all, it is such internal debate that gives science its strength. But by the late 1970s, although there was a reasonably sized, scholarly community in environmental psychology, the pressures to communicate with those who fund our activities tended to mask the equally important communication among ourselves. I therefore proposed to Academic Press that we launch *The Journal of Environmental Psychology*. A sabbatical in 1980, at UC-Berkeley with Ken Craik enabled us to launch the journal by 1981.

In launching the Journal, though, we were determined that it should not ossify the field but contribute to its evolution. From the beginning the journal was eclectic in what it takes 'environmental psychology' to be. It deliberately cherished many forms of communication besides the report of empirical studies. Forty years on it has grown in significance, being of ever more relevance. Although it has appropriately moved its emphasis to 'green' environmental psychology issues, it still retains its original roots in the broad discipline that Ken Craik and I envisaged.

FIRE RESEARCH

My directly applicable research activities were also given a fillip in Japan when I came across a small study carried out by Masao Inui and his colleagues, which as far as I know was never published. They had interviewed people who had been in buildings on fire. I was struck by the possibility that these Japanese Building Science researchers had discovered of getting people to answer questions about a threatening and traumatic situation. As an undergraduate, I had been introduced to the work of Quarantelli (1957) on disasters and learned from his studies that patterns could be found to seemingly bizarre and random behaviour. But I had not appreciated the potential significance of these studies for building design.

In the context of the Japanese Building Research Institute, I began to see that the fire regulations governing the design of buildings were based upon assumptions of what people would do in a fire and other emergencies. Yet these assumptions were all derived from major enquiries of very unusual incidents. Very little systematic research had been

done pm day to day events. On my return from Japan, I approached the British Fire Research Station and discovered that they, themselves, were developing an interest in human behaviour in fires and so started to support our own endeavours. This research on fires provided me with one of the strongest themes to my work for over 10 years. It was unlike my other research activities in very many ways. It is field research in the most extreme form, in that the only really effective way to carry it out is to follow up incidents that have already happened. What emerged as quite remarkable from studies of 20 or so incidents, including some very large-scale ones that my colleagues John Breaux and Jonathan Sime and I examined, was the consistency in the overall pattern of actions that occur in fatal building fires (Canter et al., 1980).

In order to explain these consistencies, it was necessary to ask what are the mechanisms that maintain human actions in these very unusual circumstances? The answer that I propose draws heavily on the idea of place rules and environmental roles (Canter, 1986). The work also revealed that the early stages in any emergency are potentially very confused. The time it takes to make sense of the rapidly changing events can be what turns an emergency into a disaster. The importance of these findings was recognised by the Fire Research Station, especially because they acknowledged the widely experienced problem that alarm bells are not, usually, taken seriously.

A series of studies were therefore commissioned on what we called Informative Fire Warning systems (Canter et al., 1987). Out of this work, prototype computer-based warning systems have been developed and installed, which could have a large impact on approaches to fire safety in buildings. It is interesting that this work, with its roots in a fixed engineering view of provision for escape, should have matured into yet another context in which the interpretations that people make of their surroundings and the opportunities or threats they pose are paramount. This approach to design for active understanding and control doubtless has applications to many other aspects of architecture.

BUILDING EVALUATIONS

The studies of human behaviour in fires were one strand of the contract research that I was carrying out during the mid-1970s to the mid-1980s. In parallel, my earlier involvement in building evaluations was continuing through a series of studies of housing satisfaction and evaluations of acute wards in hospitals and prison buildings (Canter, 1986; Canter & Rees, 1982; Kenny & Canter, 1981). These were all studies that were defined in terms of the methodology most appropriate for them. I found this increasingly unsatisfying for three reasons. One, it was difficult to see any accumulation of approach or knowledge. Each study seemed to exist on its own, in a sort of theoretical limbo. Secondly, the questionnaire methodology sometimes seemed to so structure people's responses that many of the insights apparent in the pilot work were lost by the time that the main study was completed. Third, the implications for action from the evaluation studies were not always apparent. These three problems led me to use the evaluation studies, increasingly, as a vehicle for developing new methodologies and a general theory of evaluation. The multiple sorting task (Canter et al., 1985) and the purposive evaluation model (Canter, 1983) were the result.

Curiously, these rather academic developments opened the way to a much more direct, yet rather distinct, mode of involvement in the design process. These developments required a much more flexible methodology, more subtle in how it could be used to uncover interacting systems. Facet theory increasingly provided the vehicle for this.

FACET THEORY

One of the other coincidences about my stay in Tokyo was that during my time there Louis Guttman visited for a month. I had been interested in the unusualness of the approach to attitude scaling that is named after Guttman and wished to explore possible developments of it with him. To my amazement, I discovered that the principles inherent in Guttman scaling had evolved into a major new approach to doing scientific research. When I met Louis Guttman in Tokyo, he had probably not met anyone for a few weeks who spoke fluent English and was prepared to listen at length to his thoughts. I was therefore given the privilege of a lengthy disquisition on his theory about how science should be carried out, which he called Facet Theory.

It took me a number of years to digest and understand the implications of what I was told that morning (Canter, 1985). Indeed, looking through my diary and notes for my year in Japan, I can find no reference to that meeting, although I remember it clearly, and Louis Guttman also mentioned it when I met him again a few years later. What attracted me

to his approach was that it did away with arbitrary levels of acceptability for 'findings' and put the creation of a lucid account of the system being studied at the forefront of scientific activity.

My methodological interests, and the search for some sort of theoretical perspective that would capture the essence of an ongoing system, had pushed me further and further away from the experimental models in which I had been schooled. But I did not feel comfortable with a retreat into a type of journalistic, purely qualitative account rendering. As I worked within the facet framework, it became clearer to me that it would provide a sound methodological framework for the type of theoretical accounts I was trying to give.

Facet Theory enables me to generate models that describe initially complex phenomena in quite simple, clearly structured ways. Probably the two most fruitful uses this has been put to so far are first in the development of the purposive model of evaluation (Canter, 1983) and second in the analysis of multiple sorting procedures (Canter et al., 1985). In both these cases, a system of interrelationships is revealed upon which future elaboration is possible without having to start from scratch.

PURPOSIVE EVALUATION

One particular contribution of the facet approach was to start building a model of environmental evaluation that would evolve from one study to the next. The first large data set we had to work with was drawn from an evaluation of hospital wards (Kenny & Canter, 1981). Initial factor analysis provided a very patchy picture of the reactions to these wards. However, nonmetric multidimensional scaling, with a faceted framework for interpretation revealed that the provision of care at the bedside was the metaphorical as well as the literal focus of ward evaluation. Furthermore, a clear level of interaction facet, showing the different scales of the place, from the bedside to the whole ward, was also found in the results.

This provided a testable system of relationships that was consistent with studies of attitudes in other very diverse fields. We were therefore encouraged to look for evidence for this framework in other data. The housing satisfaction data we had collected yielded a similar structure (Canter & Rees, 1982), and Donald (1985) found evidence for the same model in office evaluation. Because each of these studies used different questionnaires, they were able to identify quite different foci for the places being studied. Such foci were the central purposes of those places as conceptualized by the respondents.

PLACE GOALS

Other studies conducted since, as part of graduate dissertations, have found the model fruitful when applied to places as varied as neighbourhoods, city parks, and training centres. This range of applications led to the consideration of whether there are places in which there is a mixture of purposes that may be in conflict. Such an idea had already been presaged in the work Sandra Canter, a clinical psychologist, and I had done on therapeutic environments. This was summarized in the book we edited, *Designing for Therapeutic Environments* (Canter & Canter, 1979).

In the introduction to that book, we outlined the various goals for therapeutic environments, ranging from custodial to personal enhancement. Subsequent student research showed that different groups within a hospital will have different goals and, as a consequence, will differ in the designs that they consider appropriate. Some of these goals may be in conflict. The purposive model of place is consequently of value as a way of establishing the emphases in place goals and how conflicts between them may be resolved by approaches to management and design.

A DEVELOPING THEORY OF ENVIRONMENTAL (SOCIAL) PSYCHOLOGY

The use of the facet approach, to help develop the Model of Place, also served to show that some of the directions in which such unfolding was productive related to social and conceptual processes, rather than perceptual or formal architectural ones. This helped to move beyond some of the weaknesses of earlier frameworks. Indeed, once *The Psychology of Place* had been published, it became clear in discussions with students that there was a productive, but fundamental, ambiguity in the model sketched out in the book.

In striving to develop a research focus that bridged the environment/behaviour divide, I had left it unclear as to where the "places" being studied were. It was argued that they were not simply physical locations, but shaped by the actions and experiences of people. It was also argued that they were not merely mental representations of environments. They clearly have physical components that are integral constituents. If they are not just a part of an individual's psyche and they are not simply a physical location, the question emerges as to what they actually are. To provide any confident answer to this would be to imply that 2,000 years of philosophical debate had been resolved, but some interesting possibilities can be gleaned from taking a social psychological perspective (or even a sociological one, depending where you draw the boundaries between the disciplines) on our experience of our surroundings. Within this framework, especially as elaborated by Moscovici (Farr & Moscovici, 1984), it is recognized that many phenomena experienced as having an independent existence, whether they are for example, 'health', 'psychoanalysis', or 'unemployment', all are socially constructed. Their existence is more than the agglomeration of attitudes or perceptions held by a number of separate individuals. My development of this view was been spurred on by the shift in the audiences that have asked me to write for them or make presentations to them.

In the 1970s and early 1980s I would guess that the majority of invitations came from architectural sources, but this gave way to far more invitations from psychologists, especially social psychologists. Of course, this shift could be entirely due to what I might be able to comment on with any skill, but I think it is more a reflection of changes in the disciplines themselves.

As architects moved away from a concern with their users to a concern with form and image social psychology has become more environmental. This was drawn to my attention when Michael Argyle asked me to talk at a seminar on 'situations', which eventually emerged as a book edited by Furnham (1986). Here, at last (I thought) were social psychologists examining the context in which behaviour occurred.

Unfortunately, I soon found that their experimental traditions soon destroyed this interesting exploration, treating 'situations' as independent variables to be manipulated, thereby losing the significance of the context to which Barker had drawn attention 30 years earlier. From this experience, I began to look at how the notion of place could be linked to the situational debate in psychology. My paper, "Putting Situations in Their Place" (Canter, 1986) was a result of this exploration. The conclusion I came to was that the search for situations and the associated attempt to classify them and systematize their impact was really at too fine a level of detail to reveal any general structures. The concept of place, which could house a number of characteristic situations, was more likely to prove fruitful. Part of the reason for this view was that a variety of studies of place use had produced consistent, eminently interpretable multivariate structures. In studies of domestic contexts at least, the activities in Glasgow, Tokyo, and Lagos appeared to have a similar form to them, although cultural differences were also apparent, especially among tribal groups in Nigeria (Omotayo, 1988). As I presented these results at a number of conferences, where their self-evident nature was challenged by the difficulty of explaining them to an audience that had not been through the history of my thought processes, I was increasingly concerned to try and understand what it was that these consistencies were consistencies of.

We had found that certain clusters of activities were found in certain rooms. Bedrooms, dining rooms, kitchens, and so on can be characterized by what goes on within them, even though the words used to describe these rooms in different languages do not necessarily encapsulate their function as it does in English. That people should sleep in bedrooms, eat in the room with a dining table in it, should not be too surprising. But that there are a whole range of other activities and expectations that also coalesce around these actions is a clear example of the existence of 'place' systems. The questions that reveal these most strongly, though, deal with who is responsible for the furniture or activities in a room and what is allowed or not allowed in a room. In other words, the rules that structure that place.

This awareness that the interpretable structures we were finding were reflections of 'place rules' took much longer to emerge than might be apparent from a reading of *The Psychology of Place*, written 10 years before Putting Situations in their Place. What might be called an anthropological shift took some accepting. From writing of Putting Situations in their Place, my attention had been drawn to the actions that are central to the definition of places. Nonetheless in that paper I was uncomfortable with the apparently static qualities that this model had.

Places appeared as givens, yet there are many reasons why they should not be expected to be static. Perhaps the most fundamental is the dynamic conflict between the active nature of human agency in making sense of the environment and the implied coercive qualities of places that structure human experience. Furthermore, our daily experience shows change and modification as characteristic of place experience, just as improvisation was so prevalent in the Scottish comprehensive schools. I was therefore puzzled by the need to find a balance between the consistency of place use and experience, necessary for a social sharing, and the dynamic qualities that are part of life as it is lived.

The opportunity to pursue these ideas further came from being asked to give a keynote address at the Berlin IAPS conference (Canter, 1985). For that presentation, I explored the possibility that it is the interplay between the static quality of places and the dynamic, purposive nature of human action that provides the process out of which both places and actions evolve and change. I suppose this is a model of person/environment interaction shifted to a higher level of complexity. In moving to this level there is much more of real possibility of the application of architectural/environmental psychology ideas without diluting their subtlety.

THE FEASIBILITY OF APPLICATION

The fire research was the first set of studies in which I have been involved that led clearly and directly into some aspect of policy formulation. It had the consequence of my being invited to join two government-established enquiries into major fires, one for the Bradford City Football Ground fire, the other set up to examine the Kings Cross Station Underground fire. These experiences have caused me to examine closely what it is that Architectural Psychologists have to contribute. Increasingly, I am coming to the conclusion that it is not some specific facts or findings, but ways of thinking about a problem that is the essence of the contribution.

This parallels the often-quoted remark by Kurt Lewin that "there is nothing so applicable as a good theory." But there is nothing so difficult to develop and then communicate as "a good theory." This attempt to communicate a way of thinking about an environmental problem was followed through in my book, written as a result of the work on the Bradford City Football Ground fire, *Football in Its Plac*e (Canter et al., 1989). The book quite deliberately is used as a vehicle to develop a popular account of the relevance of environmental psychology and had as its subtitle, *An Environmental Psychology of Football Grounds*.

As chance would have it, the book was planned to be published in the late spring of 1989, so it was published shortly after the Hillsborough football ground disaster in which 95 people were killed.

EMBRACING THE 'MEDIA'

The Hillsborough tragedy brought home to me that if we really do have anything to contribute, how inevitable is contact with journalism and the mass-media, for an applied field like ours. Although my research activities have increasingly become of interest to television, radio, and the newspapers, it has been easy, from an academic position within a university, to dismiss all this interest as trivial or to see my involvement as merely significant as a form of advertising or self-enhancement. Yet, when our work may contribute toward the saving of lives, we have to consider seriously how our findings can be communicated to those many important audiences who do not read academic journals or attend professional conferences. We should weigh carefully the implications of media coverage. After all, our research activities are unashamedly aimed at changing environmentally relevant actions and decisions.

The applied orientation of person/environment studies has never been in doubt. As Robert Sommer (1988), for instance, has been at pains to point out, the people outside of the academic community whom we wish not only to communicate with but also to influence, do not read articles in the *Journal of Environmental Psychology* or *Environment and Behavior*. They read newspapers and watch television. In Great Britain, they also listen to national radio. The problem this raises is that once we do have something to say that is of general public interest, there is a temptation to shape research in relation to the questions journalists ask. This is wrong. The role of the research community is to formulate ways of thinking about the world that are shaped by empirical scientific processes, not by populist or political ends. I have found the need to constantly examine what the objectives are for my research in the same way that my research has led me to try and

unravel the role of the objectives of others. This search for objectives is the central scientific quest. This is not an easy point to make to journalists who want immediate discoveries to quote for tomorrow's publication deadlines.

BEYOND APPLICABILITY

Although my research since its earliest days in the study of school buildings and offices had applicability as a major objective, the building satisfaction surveys did not, of themselves, appear to have any impact or even clear consequences for design decision making. Yet the ways of thinking about buildings that emerged from those studies could have radical consequences for architecture and the design process. This consequence stems from two related perspectives. One is that the form of any design is evaluated in terms of its potential contribution to what a person is trying to achieve in any given context. The second is that the social/organizational rules that structure place use have to be incorporated into design considerations. The consequence of this approach has been to reconsider design participation. Drawing heavily on the techniques developed by Arie Peled (Peled & Ayalon, 1988), we have found it possible to get people to develop design proposals that incorporate views of how the building is to be used. From this, principles can be drawn out that give direct, clear guidelines to the design team. The attractive quality of this is that it is open to use with groups that are not usually considered amenable to such investigations. Currently, for instance, I worked with the Salvation Army on the design of facilities for the homeless in London using purpose-oriented design participation exercises.

BROADENING HORIZONS

In writing about the early stages of my intellectual history, it becomes apparent to me that the roots of this work can be traced with some confidence, but the long-term directions in which it is leading are far from clear. Looking back, I did not think at the time and could not have guessed that my PhD research on offices would have taken me so far away from examining the effects of the environment on behaviour. At the time of the Yorkhill Hospital study, I did not think that it would have led me to put such store by role differences. Nor was I aware for at least another 10 years that in-depth evaluation of a building in use could provide the basis for a participative design procedure. The studies of behaviour in fires were aimed at the building regulations, so I had not appreciated how they would lead me into considerations of the management of safety in industry (Powell & Canter, 1985). Although that organizational perspective on emergencies and accidents is completely consonant with the social perspective on building design, the emphasis that the safety research has given with regard to place rules was especially unexpected.

The evolution of these early studies all reflected a drift even further away from the experimental, perceptual tradition, in which I was schooled, to a much more transactional, social psychological framework. Of particular delight is the discovery that the problems of environmental research are so difficult that if some handle can be got on them, then this is likely to be of value in other field-based studies as well. As a direct result of the perspectives and methodologies I have mentioned, I became involved in looking at criminal behaviour, with a direct contribution to ongoing police investigations. In some cases, even making a contribution to the apprehension of a person who has murdered a number of strangers (Canter, 1994).

Thinking about how criminals may structure their objectives, in relation to the understanding they she has of the environment in which they operate, turns out to be a fruitful basis for the application of the facet approach. Even less obviously related are the studies I have been conducting on the experience of alternative medicine, most notably homoeopathy (Canter, 1987). Yet here again it is the understanding and direct experience of the user that is the focus, rather than the medical impact of any particular drug. Not unlike an effective environment, it is also emerging that alternative medicine seems to be attractive because of the control over their illness it gives patients. In other words, how it helps them to be more successful in achieving their daily objectives.

It may seem a long way from studies of the effect of office size on worker performance to the experience of homoeopathic medicine, but the strands tying them together are unbroken. The search for active, human agency, interacting with the world of physical experiences is the problem of why art exists that I was curious about as an undergraduate. Seeing these 20 years of research in this light makes me feel that, at last, I am ready to begin.

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Professor David Canter is one the UK's most eminent applied social psychologists, being one of the few to be appointed as an Honorary Fellow of the British Psychological Society and having been elected as a Fellow of the Academy of Social Sciences, the American Psychological Association, and the Royal Society of Medicine. Although he is internationally known for his development of the discipline of Investigative Psychology, bringing scientific precision to 'offender profiling', his earlier work was the development of Architectural/ Environmental Psychology, having established the well-respects *Journal of Environmental Psychology* in 1980. He has worked as a management consultant to major U.K. companies on risk reduction, amalgamations and briefing for new building complexes. He has also given advice to government enquiries into disasters. He has published many books and hundreds of academic articles, having been awarded a UK Golden Dagger and the US Anthony award for his book *Criminal Shadows*, and having written and presented a six-part documentary series, *Mapping Murder*, which is also published as a widely read book. He is Emeritus Professor at the University of Liverpool, U.K.