LEARNING THE CRAFT OF RESEARCH
A continuing process

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Embarking on a doctorate

I have often told the story of how I came into the planning field. After a few years working in the planning department of a London borough in the 1960s, during which I got my professional planning qualifications by studying part-time, I still felt I knew little about the planning activity I was involved in. I did, however, come from an academic family, so I thought that by doing a PhD I might get a better idea of the nature of the planning endeavour. This led me to get accepted for doctoral studies at the London School of Economics (LSE) in 1969. I was lucky at the time because a new master’s and doctoral programme in urban and regional planning had just been established, and my interest and professional background seemed to fit the profile of the students they were looking for.

As with so many doctoral students, I came to the LSE with a vague idea that I wanted to investigate how far planning – as an idea and activity – could contribute to transforming societal conditions. ‘Planning and change’ became the overarching theme of my inquiries, and the eventual title of my dissertation. My supervisor suggested I should look at the experience of new and expanded towns in the UK, but at the time I had the opportunity of going to Latin America, where urbanisation was proceeding apace. This seemed much more interesting, and after a few months, the UK experience was quietly dropped. But that still left me with an enormously broad canvas, and I spent months in the LSE Library exploring what the social sciences had to say about the trajectories of countries experiencing development and about the relation between societal development and urban development. These journeys into different disciplinary fields were adventures into the thought-worlds of diverse epistemic communities.1 Grasping the complex relation between concepts, research questions, research methods and interpretive analyses was a challenge. Each field, with its own multiple strands and debates, was so different. ‘How do they come to think like this?’ I often wondered. Debates on urbanisation and development in sociology, anthropology, geography and political science had different foci of attention and different ways of framing issues, investigating them and justifying findings. But the explorations proved excellent training in how to enter into and draw ideas from different disciplinary fields, as well as developing my sensitivity to interpretive approaches.2
However, back then, there was little mention of interpretive approaches, as the social sciences which I encountered were dominated by a mixture of positivist science and empiricist method. And in those days in the UK, there was little tradition of training doctoral students in research methods. I drew, in an unreflective and unstructured way, on what I had learned in undergraduate days in geography (descriptive ways of identifying and mapping phenomena) and social anthropology (ethnographic ways of immersing the researcher in the life / thought-worlds of others). Supervisors left us mostly to our own devices, and to our discussions with each other and in seminars. I used to prepare papers for my supervisor, and only occasionally got feedback. However, I discovered that an imagined supervisor could be valuable. I used to think about how my actual supervisor might respond to what I had written, and in such reflections began to construct all the criticisms that I thought he might make. In this way, I lodged the critic inside my head. Through supervising others, I have since come to think that a key role of a PhD experience in an academic career is to develop this auto-critical capacity. Good scholarship demands a difficult combination of confidence to drive forward with new explorations and insights, and humility in the face of the difficulty of grasping, even in a small focused study, how the world goes on.

So my doctoral experience was a messy, ‘do-it-yourself’ kind of training experience, through which I learned a tremendous amount, including about the tradition of ideas in the planning field and about how such ideas came to life in practices – in my case, in Venezuela and Colombia. In retrospect, I wish I had had more exposure to discussions of research methods. It would also have been helpful to have had some opportunity to discuss the different epistemological traditions through which systematic knowledge could be produced. Such philosophical reflection was lacking in the intellectual climate of British geography and planning at that time, until David Harvey came along. But on completing the thesis, I found enlightenment in phenomenology, to which I was introduced by Joe Bailey, who was then teaching sociology to planners at Kingston Polytechnic, where I had a lecturing post for a few years. Joe introduced Marxist analysis and phenomenological ideas (Bailey 1975), a combination which was later brilliantly articulated by sociologist Anthony Giddens (1984). Through David Silverman’s work in particular, a sociologist then interested in organizational dynamics, I came to know of all kinds of ways of doing qualitative research investigation.

My first funded research project

By this time, although I was by then at the Oxford Polytechnic (now Oxford Brookes University) and involved in the design of planning programmes, it had become clear to me (and others) that I was fuelled by curiosity about how planning ideas came to interact with the practical world. This was in effect a more modest reformulation of my earlier interest in planning and change. In my Latin American work, I had found different ideas about planning coexisting and sometimes clashing in the development of urban governance practices. Remembering my planning experiences in London a few years before, I wondered what ideas were influencing planning practices in the thirty-three London boroughs. This led to the design of a research project, Planners’ Use of Theory in Practice, funded by the Centre for Environmental Studies (CES). Brian McLoughlin was a senior researcher there at the time, and proved very supportive to an unknown and inexperienced researcher. Such people are very helpful to early-stage researchers, and I was lucky to find him.

This new study was much more methodologically aware than my PhD. It combined participant-observation with interview survey to investigate planners’ use of theory in practice. Jacky Underwood, who had also worked as a planner in a London borough, became the research
associate who spent six months in one of the boroughs. We both did the interviews with planners at different levels in each borough planning department, using semi-structured questionnaires and the careful writing up of each interview. We also followed Silverman’s advice, and discussed our draft research reports with those who had been the subjects of our research. This proved both challenging and enriching, as we had to make sense of the reactions and comments they made. Where possible, I have used this procedure in subsequent research. However, such a practice takes time and demands careful attention to ethical concerns.

It was on the basis of this research that I created my first map of the planning theories in circulation in practice. But we concluded also that the dominant ideas and practices (we might now call these ‘communities of practice’, after Wenger 1998) in each London borough were influenced by the configuration of local politics and the development challenges faced in each place. This led on to research which focused directly on the way that development plan-making work was shaped by its context. In these investigations, I got used to working in teams, and also with clients. Funding from national government for studies on how the planning system was working had the benefit of both providing significant financial support for research activity and facilitating good access to the working practices of local authorities. But there were also significant tensions. I have described elsewhere the pressures a client can put on a research team to change their findings. We also had to keep in mind a double audience for our work. On the one hand, we had to produce research reports which conveyed our findings in a format that would reach our client’s audiences. On the other, we needed to report our investigations to a more scientifically minded academic community. This double reporting was a time-consuming and demanding process, but very productive of new insights.

Changing locations

In the late 1980s, I moved to the University of Newcastle (United Kingdom). There I was challenged to understand a very different context to that in affluent southern England. With colleagues, we explored a variety of policy initiatives aimed at the physical and social regeneration of the Newcastle area, sometimes as participants, sometimes shadowing key actors as they went about their work, sometimes working with community groups to give voice to their perceptions of an initiative as it evolved. In terms of research strategy and design, perhaps our most sophisticated study was that on a city centre revitalisation project. This project was funded by national government, but was always in tension with local government. In this case, and building on our experience, we had articulated a concept about what might lead to the transformation of established governance processes.

The core hypothesis was that the energy for such transformation of established governance processes (we used the term ‘mobilisation’ to capture this idea) would be the result of qualities of the knowledge resources and relational resources available to, and generated through, the governance initiative as it interacted with its wider context. We used this concept – of ‘institutional capacity-building’ – to structure our research inquiry into the evolving governance processes of the partnership agency which was set up to pursue the project. We were a team (Ali Madanipour, Claudio de Magalhaes, John Pendlebury and myself) with different capacities, with Claudio pushing us towards coherence in our methodology. We shadowed the agency’s chief executive officer (CEO), attended partnership meetings, conducted semi-structured interviews with people selected through a systematic stakeholder analysis and reviewed a good number of documents (reports, minutes of meetings, etc.). We analysed all this material through detailed textual work, making use of the programme NUDIST, a software package for the computer-aided analysis of textual data. We then drew the analysis together around four themes arising...
from our initial concept. We wrote the work up in a detailed research report, but also in papers which emerged in two books. What we found was that, although the agency pioneered new ways of working, and that this effort was underpinned by the mobilisation of considerable knowledge resources and social networks, these were not sufficient to have wider effects on the governance landscape. This led us to conclude with a broader conception, that transformation episodes had the chance to penetrate into the institutional landscape of ongoing governance processes and the wider governance culture, but whether, how and when this happened depended on wider contextual forces as these interacted with local histories and geographies.

Perhaps finally I had come to learn how to do what most PhD students are now trained to do! And my most enjoyable research experience was the study of spatial strategy-making practices in different European countries, where, having formally retired from teaching and academic administration, I could become my own research assistant, and work a bit like a PhD student once again! But, as we older people say, we are not quite the same as when we were starting out all those years ago, because we (hopefully) have a richer experience and more confidence in all the many judgements, small and large, which go into the creation of a piece of research inquiry. The value of this experience and these judgements is, of course, for later generations to assess.

Some general reflections

As my research activity has developed, I have come to understand that I like to work in a particular way, more akin to the methodological traditions of sociology and anthropology than those of economics or psychology. I am fascinated by engaging with the fine grain of what goes on, and in the small details I find inspiration and clues about the wider processes which are fuelling events. We all have our particular sensibilities. But I have also found myself continually reviewing and refining my craft. One aspect of this craft which I have always found challenging is how best to write up research findings. As John Forester discusses in Chapter 1.7, this is a significant skill for any researcher. Some studies I was involved in never moved beyond a report for a client, though the learning we got from it lodged in our collective heads, to feed into other projects and writing. But I have also found myself writing for several different audiences. Although the core message (the ‘findings’) may be the same, the relevance of the findings will be different for different audiences, and each will respond to a different language style. I have had to learn how to hone my own writing to be disciplined as well as expressive, and to avoid the long complex sentences which sometimes have poured out of me. (I could write lots and fast when at school, and did not learn how to discipline the flow until much later!) It is a great help in learning to write well, as Forester underlines, to have some critical friends who will read a text not just for meaning but also for style. But that still does not meet the need to relate to audiences who read and absorb material in different kinds of ways. For that, the best approach I have found is to think hard about each audience and how they might relate to what I am trying to say. Of these audiences, perhaps our academic peers as they review our papers are the most complex to predict, for the many reasons that Forester discusses. But there are other audiences as well, many of which are not interested in the issues which concern academics. It is a hard task to synthesise complex concepts and findings – which must always be accompanied by caveats about the limitations about what can be known, with the time and resources available – into clear policy messages and practical advice to particular audiences. The previous sentence, for example, is far too long for such a purpose!

People outside the sphere of research often wonder what all this endeavour is for. Many look for a clear answer to a practical question. But the more I have engaged in research, the more I
am acutely aware of the limitations of what we can know about any particular issue. Our work as planning academics may provide a really useful input at a particular moment in the evolution of a planning project or the development of a particular policy. This impact may arise through the written word, in a paper or a report. But more often, such impacts arise through interaction between academics and practitioners, in discussions about a problem or an issue. Some impacts occur on much longer timescales, as a new generation of students reads the work of a previous generation. And sometimes a new contribution has to wait until the wider world has caught up with the way of thinking begin presented. This leads me to two conclusions. Firstly, planning researchers should not expect a simple linear relationship between a research study and a practical outcome. Yet, secondly, as planning researchers we should always be conscious of the implications of our research for the wider world. We should always ask, having concluded our study, so what? What is the implication for the policies and practices which are the focus of our attention?

Throughout my research career, I have been conscious of treading gingerly through thickets of ethical dilemmas. In my PhD research, I puzzled over a core ethical concern for all scientists: who will believe me? Or rather, what is the status of my interpretation of what I have found? At that time, I buttressed my accounts with an overload of empirical investigation, much of which never appeared in the thesis. I then learned to interact with subjects to see how far my accounts related to theirs. As time went on, I got much more sophisticated, making more use of concepts and hypotheses to structure investigations. But this is only one layer of my ethical challenges. There were questions about confidentiality. How much about what I have found out from one source should I convey to another? And what guarantees of confidentiality should I give to those interviewed? And am I exploiting my informants? What should I give back, in return for their preparedness to be involved in my study? In the end, my answer to these dilemmas was to treat everyone with respect, although this is not always easy! Sometimes you just have to stay silent, having given guarantees of confidentiality, knowing something which would be very useful to the person you are now talking too. There are also times when wonderfully rich material just cannot be written up publicly because to do so would compromise the ongoing work or position of a key informant. When I started working with others, there were other ethical issues about responsibility – for team management and ‘care’, and also about the attribution of the work we did. In two books in which I was lead researcher, we discussed carefully exactly how the authorship should go, in relation to the amount of work done by each team member. These days, when academic production is so important for people’s careers, it is good practice to discuss these authorship questions early on! Beyond this are wider questions of moral responsibility, especially where there are tensions between researchers and clients. These issues are discussed more fully by Lo Piccolo and Thomas in Chapter 2.3.

So I have learned a lot through experience about the craft of research in our field – the experience of doing research, of working with others, of helping new researchers learn the craft, of working with clients with specific questions and of interacting with all kinds of critical friends and audiences. If research methods books and teaching had been as available when I started as they are now, I might have learned much of what I now know faster. I have also often dipped into familiar research texts when thinking about a particular step in a research project, just to prompt myself of what I need to think about. The research methods texts are there to remind us of the rich possibilities about strategy and technique on which we can draw when engaging in an investigation. But in the end, the key to good research is an arresting preoccupation which animates the inquiry, an imaginative perception of what is going on in the empirical world examined, coupled with the craft through which a preoccupation is turned into a systematic and achievable study. All this requires a capacity for judgement, so that the exercise of the craft does
not get reduced to a technical procedure which so distorts the world in which we live that it cannot ‘speak’. It is the insights from the experienced world which jolt us out of our preconceptions and assumptions and startle our imagination into new interpretations and understandings.

Notes
1. The notion of an ‘epistemic community’ originated in studies in the sociology of science, and refers to a group of scientists who develop and validate their work using similar perspectives, concepts and ways of validating their research (see Haas 1992).
2. Interpretive approaches are now well established in the social sciences. For reviews focused on the policy analysis and planning fields, see Fischer and Forester (1993), Wagenaar (2011), and Fischer and Gottweis (2012).
3. Harvey’s *Social justice and the city* (1973) was inspirational for many planning academics as well as geographers.
4. David Silverman has continued to provide research advice for sociologists, with several regularly updated books on qualitative research methods; see Silverman (2009).
5. There are actually only thirty-two boroughs; the thirty-third is the City of London.
6. The CES was a government-funded research centre, which was closed in the Thatcher era, a period hostile to both planning and research.
7. Brian McLoughlin, who died in 1994, was an academic with planning practice experience. He is best known for an enthusiastic text on a systems view of planning (1969), a view which he later rejected, and for a careful analysis of the extent to which Melbourne’s development matched the planning strategies intended to guide that development (1992).
8. From her detailed study, she produced an account which became well known in the UK in the 1980s, *Town planners in search of a role* (1980). She moved after this work to the School for Advanced Urban Studies in Bristol.
9. This emerged in Chapter 2 of Healey, McDougall and Thomas (1982).
10. This work is the basis for Healey *et al.* (1988).
13. But we concluded that there was no need to elaborate more than one analytical layer in this formal technique.
15. This idea was summarised in Healey (2004).
17. These were Healey *et al.* (1988) and Vigar *et al.* (2000).

References
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