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IT TAKES MORE THAN JUST LOOKING TO MAKE A DIFFERENCE

The challenge for planning research

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Introduction

I registered as a doctoral student in planning in 1987. At this time very few of the academic staff in the Department at the University of Sheffield where I undertook my studies had a PhD, and none were female. The central role of a professionally accredited planning school was to teach, and the main quality necessary to be an effective teacher was to have been a planning practitioner. The assumption, as with many professional disciplines, was that you had to have been there and done it, and, perhaps on a part-time basis, remained professionally active. The tension between the so-called academic and professional runs deep and has long excited lively debate within planning and beyond. This is manifested in a perception that scholarly excellence and professional relevance are inherently incompatible, and, moreover, that the intellectual and the practical are worlds apart, each diminished by cross-contamination, rather than mutually invigorated. However, as I started my doctoral studies, pressures beyond the planning community in the UK were in the process of changing the landscape. Margaret Thatcher’s administrations of the 1980s brought with them not just a retrenchment in higher education but more particularly the introduction in 1986 of the first Research Assessment Exercise. The quality of research produced by academics was to be measured and ranked, with those departments performing the best being financially rewarded. For good reasons or more sinister, research excellence, as an intellectual aspiration or more narrowly a performance measure, had arrived as an issue for planning schools.

It is the theme of the relationship between the intellectual and practical that I have decided to take as the focus for my reflections, for in many respects my career has run alongside the maturing of research capacity within the planning academy. Hence, by the time I was appointed to a lectureship in 1991, a PhD had become a key requirement for an academic position, at least within research-intensive universities. There is no doubt that for some within the planning community the appointment of the likes of me, damned by the term ‘career academic’, and even worse ‘intellectual’, epitomises all that is wrong with planning education. What can I know about the real world? What do academics actually do that makes a difference to practice?
Research often seems to be perceived as some arcane pursuit undertaken by dusty folks in ivory towers, speaking a barely decipherable language, and of virtually complete irrelevance to the nitty-gritty lives of practitioners, or for that matter real people and real places. I would certainly not contend that all evidence runs counter to such perceptions. And from inside the academy, there remains a disdain for the so-called applied or professional disciplines as somehow not really intellectually respectable. Only second-rate academics would find themselves cast into such disciplines; as A.J.P. Taylor observed in 1967 about post-war developments in higher education, ‘Universities nowadays have Professors of almost everything – Brewing at one, Race Relations at another, Town Planning at a third.’ With the sensibilities of forty years these appear a somewhat contentious combination, but the broad thrust of such commentary echoes debates which have reverberated for centuries over the capacity for the achievement of scholarship in fields concerned with professional education. How far does the intellectual become compromised, even debased, by its engagement with the practical and professional? As a planning researcher it is easy to feel caught in an uncomfortable trap between the worlds of the profession and the academy. But the relationship between scholarly excellence and relevance asks fundamental questions about not just the planning discipline but also the more general role of intellectuals and universities in society.

The danger with reflections is that they can become nostalgic and backward-looking. My purpose is to try to draw on my past experiences to look forwards. If once I thought that what planning needed was to embrace the rigour of the traditional social sciences, I am now reminded that the relationship is not one-way, and that planning’s appreciation of the interplay between theory and practice (research and action) is a necessary prerequisite to drive forward intellectual scholarship, both within planning and crucially beyond. As research competence matures within the planning community, we should have confidence in being planning researchers (dare I suggest, planning intellectuals), rather than mimics of the established, traditional disciplines. It may be challenging, but I see no incompatibility between scholarly excellence and the capacity of knowledge to make a difference.

In the remainder, I first draw directly on my own experiences as a researcher, and sometimes uneasy traveller within the planning discipline, and then secondly, focus more specifically on the activity of social science research – more especially, what it is we see, or perhaps more particularly do not see, as researchers. In the conclusion I return explicitly to the myths and challenges concerning the relationship between scholarship and relevance.

A journey in planning research – learning to look like a researcher

Let me start with an admission. I became a PhD student somewhat by accident in a discipline that I was deeply unimpressed by, at least for the most part. I have no tale of being propelled into a PhD as the result of the unlocking of an intellectual spark. I had come to planning as a master’s student and fully intended to go and work in practice, (something I still held as the most likely option while I was undertaking my PhD). By the end of the first term of the masters I came to the conclusion that planning seemed somewhat shallow. I wonder now at the arrogance of my judgement, although not the substance! Much of the content of planning writing was descriptive, largely procedural in nature, focused on matters of detail and seemingly unquestioning and uncritical, while also absorbed by a fractious turf war with its big, bad, older sibling, architecture. In fairness, while the discipline could hardly be described as at the forefront of intellectual thought, there were individuals determined that research and scholarship were necessary to drive forward both the intellectual and professional aspects of the discipline.
There was little structure to my PhD studies and only a handful of other students, mainly from overseas. I attended a couple of somewhat hurriedly organized classes for doctoral students across the social sciences and had to give a departmental seminar about six months into my studies outlining my research proposal. But generally the development of my research was a matter for me and my supervisor. I met regularly with my supervisor, but by current standards, I was left to go my own way, to make my own discoveries and my own mistakes. It was inevitable that my research questions would point me towards ethnographic research, and mine was the first thesis in the department to embrace such an approach. In retrospect, perhaps strangely, my research approach, which involved being based in two local authority planning departments for several months, did not prove a matter of particular contention. However, it mattered a great deal to me to be employing what I regarded as radical qualitative methods. Hypotheses, statistical surveys, modelling and spatial analysis smacked of outdated, outmoded, even conservative or reactionary ways of thought. I have since come to realize that the qualities of research cannot be assessed according to method alone. Research can be good, bad or, more frequently, mediocre, but such qualities are not confined to any particular methodological approach. Sadly, divisions over methods continue to deflect attention away from much more profound questions about the real merits of our individual and collective research output. I was asked recently, “Are you qualitative or quantitative?” The nature of my journey is indicated by my response: “Neither!”

I learnt much through trial and error about the nitty-gritty of doing research. For example, the art of effective in-depth interviewing was central to my research and continues to fascinate me: how to word a question or frame an intervention, how to use an eyebrow or shrug of the shoulder, so as to unlock insight rather than to close off discussion; how to encourage the interviewee to tell you more than intended when you first met; how to listen; and how to work out what matters. There is a tendency in preparing to undertake fieldwork to concentrate on the researcher’s relationship to the interviewee. But I wasn’t just listening and watching them; they were watching me. I also became conscious that the world tends to assume that if you are young and female, you necessarily know very little. While frustrating and at times exasperating, this does leave scope for the deployment of those most penetrating of questions – the (seemingly) silly or naive. As an aside, my experience suggests that youthfulness and intellectual gravitas are seldom regarded as coexisting. Despite a world sensitive to issues of identity and exclusion, it seems youthfulness (or the appearance of youthfulness) tends not to be associated with academic credibility and standing.

I was also aware that my research participants were not just watching me but also prepared to probe the possibilities of what use I might be. For some, having a researcher about the planning office was a source of suspicion – was I the bosses’ nark? – while others wondered whether I might act as a useful conduit to pass on information. Related to this, while gatekeepers are invaluable to researchers, it is crucially important to be seen as demonstrably independent. More significantly, what were my responsibilities to those who so readily, for the most part, allowed me to occupy their space and time? The giving of time and knowledge by my research participants was (hopefully) enable me to gain a PhD and subsequently provide the building blocks for publications, but what would they gain individually or collectively? I remain to this day amazed how frequently doors, which in other circumstances would remain firmly closed, are opened by the statement, “I’m undertaking a research project and wonder if you’d mind if I asked you a few questions?”

A PhD, however, is not just about acquiring competence as a researcher. It also provides an initiation into the academic community. A crucial, although oft-times overlooked, aspect of
most doctoral programmes is how the behaviours of supervisors and other academics provide role models for the future. Would we personally wish to be treated in the way some doctoral students have to endure? Beyond the basics of actually turning up to supervisions, I recall seminars in which fellow students were destroyed by criticism from staff, which was actually directed less at them than at their supervisor. The seminar was used as an opportunity for the airing of academic prejudices and jealousies, not for the student to receive critical but constructive feedback and advice. I was relatively fortunate and was generally treated with respect. However, while it is inevitable that we all have our grumpy moments, the way academics behave towards their doctoral students, colleagues and research participants matters in itself, as well as for the wider messages which are conveyed about what it means to be an academic.

At the completion of my doctorate, perhaps the most powerful message I took away was a sense of the messiness of research. The journal papers I read and the seminar presentations I heard, regardless of epistemological starting point, all seemed to suggest a clarity to the process of doing research: research problems precisely identified, methods determined, empirical evidence unproblematically collected and analysed, conclusions derived and, bingo, an original contribution made. Yet my experience suggested otherwise – of a much more messy process, riven with uncertainty, of never being quite sure what I was doing or what I had found. Intriguingly, while those around me suggested I always appeared to have a clear focus, that was far from how it felt to me. The key lesson I derived from the satisfactory completion of my PhD was that uncertainty is inherent to doing research and as my thesis had come out okay, I might hope the same would be the case for future research projects. But this did not detract from my wider sense that so much about the actual research process was swept discreetly under the nearest carpet, while a mythology was perpetuated of control, order and clarity. The relatively isolated and lonely nature of doctoral study at that time further encouraged such feelings. I therefore finished my doctoral studies invigorated by the experience, but also clear that doctoral planning programmes could and should be better.

At interview for my first lectureship in 1991, I expressed a keenness to develop the department’s approach to doctoral education and on taking up the appointment was told, “Do it . . . on you go”. I was given no brief, no objectives, no work allocation hours, no terms of reference and no one to report to; I was just told to “do it”. With a contemporary eye, it may seem grossly irresponsible that the most junior member of staff was given free rein with the department’s approach to doctoral education. Sure I made mistakes, lots, but making mistakes is not such a bad thing; it is inherent to learning, and I suspect if my elders and betters had thought I was going completely off the rails they would have intervened. This may seem part of a bygone age of irresponsibility, both for me personally and for the department, but I relished the freedoms I was given in this role, and in many ways too with respect to my PhD studies. The quest in recent years to provide greater support and structure was probably necessary, but there are dangers and a need for wariness that the academic endeavour is not becoming standardised, reduced to tick box templates. Structures should facilitate, not constrain or, worse, diminish.

My starting point for what I wanted to achieve with the Sheffield doctoral programme was relatively simple – opportunities for PhD students to come together, find they were not alone and learn from each other. That was to share their real experiences of doing research: of the challenges of negotiating access to a case study; of translating between different languages and working with research assistants; of struggling to secure secondary data sets; of cultural variations in the etiquette of conducting interviews; of always sensing there was more to be read and that someone else had already done the research; of how to make sense of voluminous empirical material; of transcription nightmares; of undertaking interviews immediately following an
earthquake; of getting stuck, literally, in a monsoon; of writing blocks; of children being born; of computing failures and lost data sets; of vivas successfully negotiated and examiners tamed; and, yes, also, of how to manage your supervisor. It was about being a community of researchers not just with interests in our own narrow specialism, but also with an appreciation and respect for a wider world of knowledge. It was in our weekly seminars and discussion groups that I started to learn that no methodological or epistemological position has a hold on truth (including those that acknowledge there are no truths). Often the less I knew about a topic or approach, the more I was challenged to rethink the assumptions and understandings about my own area of study. Moreover, what could be more exciting than working with the next generation of planning scholars? Any academic discipline is only as strong as its doctoral programmes, but for planning schools traditionally founded on the demonstration of professional excellence alone, such a premise represents a profound change of orientation.

As I was beginning to explore how to develop doctoral education in my own planning school, the Economic and Social Research Council (ESRC) began to formalise its own practices and related processes for the recognition of departments deemed worthy of holding scholarships for ESRC-funded students. I was invited to contribute to the writing of the research training guidelines for planning and eventually chaired the subject panel in the late 1990s. Involvement in these processes raised some challenging questions for me about the foundational knowledge for planning scholarship. For the established social science disciplines such knowledge had long been the subject of well-rehearsed, not to say contentious, debate. But internal tensions were matched in equal measure by an ability to articulate to the wider academic community clear intellectual boundaries, and hence assert disciplinary distinctiveness.

Similar debates within planning were much less well developed. After all, planning schools had little intellectual tradition and had come into existence to train (I prefer educate) planners. At a personal level I relished the absence of disciplinary protocols and boundaries by comparison to my undergraduate experience as a geographer. I could dip in and out of different domains, without anyone bothering too much. But the research training guidelines for planning could not just say “do what you like”, and nor was that really what I thought. As I sat around tables with the representatives of other disciplines, I became ever more convinced that we were selling short the planning researchers of the future if we did not provide them with a capacity to relate their research to the broader world of intellectual ideas, both as such ideas were understood within planning and crucially beyond. This also implied the need to have an understanding of the full range of research methods. Admittance to the scholarly community required such knowledge, accompanied by a rigorous approach to undertaking research. This was a question not simply of being able to talk the language of social science research but also, more importantly, of having the intellectual grounding to be taken seriously in questioning and challenging taken-for-granted ideas. Furthermore the planning profession would not be well served by a research community which lacked the insights and capabilities necessary to generate research of the highest quality.

I do not claim that there was, or is, consensus within planning about this approach to doctoral education. There remain tensions between what are portrayed as the different needs of practice and research, and the implications this has for the priorities of planning schools. However, in the late 1990s I had little sense that, while the intellectual understandings and methodological rigour of the social sciences were (and are) necessary prerequisites for the development of planning as an academic discipline, the development of such capabilities was not the complete answer, and that further, perhaps more profoundly important, challenges lay ahead.
Is looking like a researcher enough?

My doctoral studies taught me how to look like a researcher, and I soon also acquired responsibilities for ensuring that others were similarly, hopefully better equipped than I had been. So what does this mean to look like a researcher? I had been taught how to frame a research problem and define researchable questions, how to position a research study within the existing literature and to develop an analytical (or conceptual) framework, how to identify and apply appropriate research methods, how to analyse empirical findings using the framework already established and how to derive conclusions and identify the conceptual and practical implications of research. This is what was required to achieve that most highly of regarded qualities in research design and implementation – namely, rigour. As in many areas, understanding the principles and executing them satisfactorily are not one and the same. However, they are at least a basis on which to build, and I have never doubted the vital importance of developing these capabilities. But is this it – all there is to being a researcher?

As time has gone on I have become increasingly preoccupied by two issues: firstly, what is it we actually see as researchers; and secondly, what is this idea of rigour which so shapes the way we conduct research? Societies trust researchers, even allowing us to alter our title to ‘doctor’ on the award of a PhD, because of the presumed independence and incisiveness with which our research interrogates the world. But what of the nature of the researcher’s eye?

What do researchers see (and hear) when they look (and listen)? I have long mused over why when I undertake fieldwork I do not necessarily see the same things going on as others. Am I not looking hard enough? Do I not understand? Am I misguided or plain wrong? How open am I to seeing all that is going on? Of course we understand that we all carry our own preconceptions and cultural sensibilities, but could there be different qualities of looking?

Good research design, meaning fundable research, requires the identification of a well-defined problem and hence researchable questions. By its nature this means that attention is focused on some things and not others. Moreover, as empirical investigation lies at the heart of the vast majority of research studies in planning (as with the social sciences), this in turn means that we literally look at some things and not others. The use of the term ‘we’ in the previous statement is interesting, as for the most part, after doctoral and postdoctoral work, the ‘we’ doing the looking (the research) is not the same person as the person who wrote the proposal or who will largely write the research papers. Research assistants are employed to collect and often also analyse primary or secondary evidence as part of studies which generally last little more than a couple of years. So looking in practice often resembles a game of Chinese whispers. In an effort to minimise the distortions on route, standardised protocols are developed, but I cannot help but conclude that something must be lost along the way. Is there not a vitally important connection between seeing and thinking?

Intuitively it is easy to be drawn to arguments that suggest a need for more practice-based research, meaning studies grounded in the real world of planning practice and the communities that planners seek to work with and for. But if our research activities are interrogated, how practice-based are any of our empirical studies? To start with, our empirical evidence carries the template of the underlying conceptualisation. That is how a problem is rendered researchable, and hence how we know which evidence to select. It therefore follows that a Foucauldian analysis generally finds a Foucauldian-looking world, a political-economy perspective, a political-economy looking world, and so on. Our looking (or to be precise that of our research assistants) is shaped by our frameworks. What we see is that template. The resulting articles select
the evidence they present. I am not suggesting any misappropriation of evidence, but merely that there is more than just evidence being reported.

Contrary to accepted assumptions within the academic world, my most important research experience so far was being a head of department. The presumption is, of course, that taking on an administrative role is akin to ending one’s research career. One of the most crucial lessons I learnt was that doing and making decisions in real time is very different from observing decisions with the benefit of hindsight. We can all be wise after the event. However, doing is about reasoning and synthesis, while observing is about describing and explaining, the analysis of events which have already occurred. I constantly found myself intellectually surprised, in ways which had not and would not have occurred to me in the course of undertaking a research project. For example, I have long had something of a prickly relationship with the ideas of the American pragmatists, yet in response to an endless meeting in which members of the Faculty of Architectural Studies (including the Departments of Architecture, Landscape and Town and Regional Planning) were agonising over the very essence of their disciplines as the basis for determining which faculty it would be best to join given an inevitable university restructuring, I found myself saying, “This isn’t a question of foundational principles, but which will work best”. I have not as a result become a card-carrying pragmatist, but this moment has given me cause to ponder and think, as did numerous others. More generally, as researchers in planning we encounter every day the world of our concerns, certainly not just when we are engaged in the equivalent of donning the lab coat and undertaking research. It might be suggested that my experiences as a head of department are mere anecdotes, not proper research. The question that must then follow: what is proper research?

This takes me to my increasingly uneasy relationship with that great pillar of academic research – rigour. Rigour is a quality most usually associated with well-conceived methodologies, as described previously. Hence, to obtain research funding, studies need to demonstrate the tightness of their research design, not the relevance or pertinence of the questions being addressed to societal challenges. Moreover, to achieve the necessary level of rigour the research can be only marginally different from that which already exists, for otherwise there would be methodological loose ends and uncertainties. The consequence of this can be seen in the types of studies funded by national research councils. Such studies score very highly on rigour and hence the empirical validity of the research (which of course is not unimportant), but the findings tend to be far from cutting edge, and even mundane and quite dull. It is inherent to the current process for assessing research proposals that they are largely evaluated on the basis of inputs rather than outputs. Such studies have the qualities of the tin man (in *The Wizard of Oz*), exemplifying supreme technical competence but lacking a heart. Perhaps more questions need to be asked of the ethical value of the research which is undertaken. As a consequence, despite an increasing desire by some funders to support riskier forms of research, reflecting recognition that research has got into something of a rut, there will be a lengthy learning process as adjustments are made to understandings of the nature of research excellence. This will challenge the assumptions which underpin much of our research infrastructure, but it is arguably the challenge of greatest importance confronting the research community. How might the nature of research change if ethical value rather than methodological rigour was to become the cornerstone of our intellectual endeavours?

In the end it is not really rigour with which I have a problem, but rather its exclusive methodological association. Rigour of this type undoubtedly restricts the types of research questions that can be asked. Such research currently brings the award of grants and lists of publications...
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(which few will read), but does it bring research that makes a difference? It is entirely plausible that the research industry, of which universities are a part, is currently producing more and more knowledge about less and less. There is a need to develop an equally incisive understanding of rigour, which opens up the possibility for different forms of research questions and places stress on the quality of argumentation and the likely implications, not just the qualities of the methodology. This will not be a lesser form of rigour. If anything it will be a more exacting and demanding form of rigour. Further, and importantly, it implies an understanding of rigour, where rigour and relevance coexist, not as currently, set in opposition to one another.

Conclusion – it takes more than just looking . . . to make a difference

My journey as a planning researcher has seen the research undertaken within planning become better grounded in the traditions of social science research – that is to say, more rigorous. Research studies undertaken within planning are now published in the leading journals of the traditional social science journals. But is this enough? The challenge as I see it now is to remain uncompromising with respect to the importance of intellectual scholarship, but while not losing the essence of the understandings which come from being a planning researcher. We need to have the confidence not merely to be mimics but also to be able to harness the clarity of understanding that results from appreciating that it is not just looking that is important; that intellectual and practical insights lie at the interplay between knowledge and action. The societal problems with which planners are concerned – spatial inequality, climate change, cultural inclusion – do not come readily defined with labels marked ‘sociology’, ‘politics’ or ‘economics’, nor for that matter ‘practical’ or ‘intellectual’. Importantly the knowledge base for professional practice requires a capacity to interweave knowledge, effectively and selectively, so as to frame questions and link the analytical to the normative.

One of my undergraduate lecturers spoke of the importance of “seeing the world in a grain of sand”. I am not sure at the time I really understood what he was implying, but it is a phrase that has stayed with me. Despite the increasing technical competence of our research, I fear we end up knowing more and more about individual grains of sand, without striving for the possibility of seeing the world. We see fragments and parts, not the whole. We analyse but fail to synthesise. Our systems and increasingly standardised research procedures encourage yet further descriptions of yet more individual grains of sand, but not the integration of what we already know or what we do not yet understand, or more crucially the identification of the questions that really matter (see Part 2 for further consideration of such issues). There is an accretion of ever more facts, or interpretations, but not the capabilities to judge what they mean, or whether or how such knowledge might make a difference. We are becoming somewhat more knowledgeable, but not wiser.

A world of individual grains of sand becomes a world of seeming increasing complexity. Yet, as my own PhD supervisor remarked to me, “it is not complexity that is difficult, it is simplicity.” That is not the simplicity that is trite or oversimplified, but the simplicity that offers possibilities for a better future. So to quote William Blake more fully, the real intellectual challenges are

To see a world in a grain of sand,
And a heaven in a wild flower,
Hold infinity in your hand,
And eternity for an hour.

—William Blake, Auguries of Innocence

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Notes

1 Research Assessment Exercises (RAE), now known as the Research Excellence Framework (REF), have since 1986 evaluated and ranked the quality of research across the whole of the UK higher education sector about every five years or so.

2 I was diagnosed in the March of 1987 with type 1 diabetes and, amongst many other pieces of information which were thrust my way, was the evidence that diabetes reduces life expectancy by 10–15 years. Given this, the opportunity to do a PhD looked like something to be grasped, rather than left until another day.


References

Blake, W. (1863) *Auguries of Innocence*.