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RESEARCH IMPACT
Should the sky be the limit?

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Introduction

There is a scene in Hugh Whitemore’s play *Breaking the code* (1986) which all academics ought to read and recite every time they are asked to anticipate the practical worth of their research. It is about a job interview where a civil servant asks a young academic about his research and receives the following enthusiastic, yet rather confused, answer:

Hilbert thought there should be a single clearly defined method for deciding whether or not mathematical assertions were provable . . . I wanted to show that there can be no one method that will work for all questions . . . Eventually I conceived the idea of a machine.

(33–34)

The baffled civil servant asks, “You actually built a machine?” to which the young academic replies, “No, no. It was a machine of the imagination”. The interviewer’s next question is emblematic of the dominant, albeit stereotypical, view of academics as people living in their ivory towers and using public money to do ‘blue sky’ research of no use to anyone. He asks,

What is the point of devising a machine that cannot be built in order to prove that there are certain mathematical statements that cannot be proved? . . . Is there any practical value in all this?

(34)

By now, you have probably guessed that the play is based on a true story; that the young academic was Alan Turing; that he was interviewed for the post of the leading cryptanalyst in the team at Bletchley Park; and, that he went on to break the Germans’ Enigma code, which influenced the date of the Normandy landings, shortened World War Two and saved countless lives. And, if that was not enough for ‘research impact’, he also built, almost by accident, the first electronic computer.

Stories like this abound, showing the non-linear, unpredictable and contingent nature of research impact processes and outcomes. The moral is that ‘researchers’ flights of fancy can pay off’ (Reisz, 2008: 37) even if there are no obvious or immediate economic benefits. It also affirms that we can never really know in advance about the impact of research and its beneficiaries, but
we continue to insist that we can, as is reflected in the growing interest in *ex ante* assessment of the impact of funded research. Increasingly, both national and international research funding institutions are assessing research proposals on the basis of not only their scientific quality but also their potential impact. Research impact is now an integral part of the assessment processes of the research councils in the United Kingdom (see, e.g., AHRC, 2007; BBSRC, 2005; Davies *et al.*, 2005) and elsewhere (see, e.g., SSCUC, 2005; Spaapen *et al.*, 2007), as well as major research programmes of the European Union (see, e.g., EC, 2005; Georgiou, 1995) and other international funding organizations (see, e.g., Adamo, 2003; Cunningham *et al.*, 2001; World Bank, 2004).

There is a close link between the growing interest in assessing research impacts and the upsurge in evidence-based policy and planning (see Davoudi 2006 for a critique of the latter). On the one hand, policymakers and practitioners are urged to make better use of evidence and research in policymaking. On the other hand, research funding bodies are asked to justify their research priorities in terms of their contributions to societal demands and governments’ policies. Inevitably, these pressures have been cascaded down to individual researchers, who are required to demonstrate the relevance and impact of their research. While research impact assessment is on the rise, there is little agreement on what is meant by it, how it is defined and measured and what factors are important for the research to have an impact. This chapter aims to address these questions by drawing on a review of the literature, prior work on related areas of evidence-based planning and personal experiences of acting as a ‘knowledge broker’ and assessor of research proposals both in the UK and internationally. The main argument is that the way ‘impact’ is understood and articulated depends largely on how the interface between research and policy and between research and practice is conceptualized. Three models of conceptualizing research impact are presented and illustrated by planning examples from the United Kingdom.

**Research impact assessment**

We prided ourselves that the science we were doing could not, in any conceivable circumstances, have any practical use. The more firmly one could make that claim the more superior one felt.

(Snow, 1959: 16)

This is the kind of statement that can give academics a bad name. It is from a 1959 Rede Lecture presented by Charles Percy Snow, a British scientist and novelist whose book, *The two cultures* (1959), ignited a heated debate about what he called a gulf between scientists and literary intellectuals. It is an extreme example of what was then considered by some as ‘good’ or ‘pure’ science. ‘Today, such statements are rarely heard from scientists, at least not in public. By contrast, scientists go – or are pushed to go – to great lengths to show how their science is of relevance to society and what its impacts are because, in addition to *ex ante* evaluation of the potential impact of research, attentions have also turned to the retrospective assessment of research impact. The emphasis is on non-academic or extra-academic effects of research. It is not about how many times an academic paper is cited in other academic papers, as measured in bibliometric counts, but what difference it has made to the economy, society, culture and environment. In short, it is about the societal impact of research.

Assessment of the societal impact of research has been introduced into UK higher education by the most recent reform of the mechanisms for assessing the quality of research in universities. In many ways it is an attempt to correct the pernicious wedges of the former Research
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Assessment Exercise (RAE). The RAE, which took place every six years or so, assessed the quality of research in individual units of assessment (which did not necessarily overlap with university departments) on the basis of a number of key factors. These factors included, notably, the quality of staff’s submitted publications, level of research incomes and completed number of doctoral studies. The outcome of the RAE had significant direct and indirect impact on universities in terms of the level of central government research funding allocated to them. The outcome of the Research Assessment Exercise was also used by producers of the league tables for the ranking of the universities, as well as by universities themselves as a “rhetorical device” in their publicity materials. Once released, the RAE outcomes “develop(ed) an autonomous ‘public life’” with an ever growing “performative power” (Burrows, 2012: 12). Performance in the RAE, which was largely based on the quality of publications in peer-reviewed journals, became the single influential factor in academic career progression and promotion.

This meant a shift of emphasis among planning academics from engagement in planning practice to publication in academic journals. The shift was even supported by the Royal Town Planning Institute – the professional body that accredits many planning schools in the United Kingdom – which in 1991 demanded for the first time that the assessment of the “qualities of an effective planning school” should include their “publications; research income; research rankings [in the RAE]; research council recognition for courses and research students” (RTPI 1991: 5).

As a result, there has been a fall in the appointment of academic staff from planning practice and a rise in the number of what are sometimes termed ‘career academics’. Having a doctoral degree became more important for securing an academic post than having experience in planning practice. While in the past planning academics in the UK were active practitioners (such as Thomas Sharp and Patrick Abercrombie), today they are hardly involved in practice other than in advisory capacities. Similar trends are taking place in other European countries, such as Sweden, the Netherlands and, more recently, Italy. In these countries research assessments, based on academic publications, are increasingly becoming a common feature of academic performance. Thus, although the RAE has been a powerful drive for improving the quality and profile of planning research in the United Kingdom and internationally (Davoudi and Pendlebury, 2010), it has been largely responsible for a sense of divergence between the planning academy and planning practice. As will be discussed ahead, the extent to which this can be translated into an ontological divide between research and practice is, however, questionable.

To rectify what is considered as the perverse impact of the RAE, the current version of it – now titled the Research Excellence Framework (REF) – has an additional dimension in its assessment matrix – namely, ‘research impact’. Therefore, in addition to assessing the quality of research outputs (focusing principally on academic staff publications), the societal impacts of research will also be assessed. The idea is to retrospectively evaluate the extent to which publicly funded research has been used outside academia by non-academics and what impact it has had. The purpose of this ex post assessment is to make judgement on the ‘reach and significance’ of impacts achieved. This is different from ex ante assessment of a research proposal whose purpose is to learn how to enhance future potential impact of research. This difference in the purpose means that, while the former (REF) focuses on the impacts emanating from the research outputs and outcomes, the latter (i.e. the research funders) concentrate on research processes and pathways to impact.

Measuring impact

The rising interest in measuring the societal impacts of research is closely related to the concern about a perceived gap between what researchers produce and what societies demand, which, as mentioned earlier, has been aggravated partly by the RAE, and particularly so in disciplines.
such as planning. Thus, practitioners often complain that academics are interested only in ‘blue sky’ research with no immediate relevance to policy or practice. Academics, on the other hand, whine about the problem of ‘little effects’ (Weiss, 1975), referring to the large amount of research which sits on the shelf and gathers dust despite being of direct relevance to policy and practice. Despite this, there is no ontological divide between research and practice. It is difficult to distinguish with any certainty “where is the science? Where is the society?” because, as Latour suggests, “They are now entangled to the point where they cannot be taken apart any longer” (Latour, 1998: 209). Indeed, knowledge is generated in the interaction between science and society in ways that make it almost impossible to know who is producing and who is using knowledge. Latour (1998) interprets this intertwining of science and society as a shift from ‘science’ to ‘research’, by which he means a shift from what used to be an exclusive academic enterprise towards a complex web of interactions between science, industry, society and polity. The shift is also manifested in the coupling of ‘matters of facts’ with ‘matters of concern’, with the latter referring to conflation of facts and values (Latour, 1993, 2005; see also Davoudi, 2012, for its implication for planning). One example of the entanglement of science and society is the intricate systems of research prioritization, funding, dissemination, evaluation and utilization. Despite all this, simplistic notions such as knowledge transfer continue to perpetuate the perceived divide between producers and users. After being subjected to heavy criticism, knowledge transfer is now replaced in official discourses by the notion of knowledge exchange as a way of stressing the multi-directional process of interactions between researchers, policymakers and practitioners. As will be discussed ahead, the differences between the two notions are reflected in different models of conceptualizing research impact.

**Models of conceptualizing research impact**

Since the introduction of research impact assessment there has been a growing body of literature attempting to find ways of measuring it (see, e.g., Lavis et al., 2002; Hanney et al., 2003; Molas-Gallart et al., 2000; Elliot and Popay, 2000; Nutley et al., 2003; Molas-Gallart and Tang, 2007; Wooding et al., 2007; Davies et al., 2005). Drawing on my earlier work on evidence-based planning (Davoudi, 2006) and the work of Carol Weiss (1979), it is possible to group the plethora of approaches to research impacts into three broad models: the instrumental model, the conceptual model and the symbolic model. The following account provides a brief outline of each and their interpretation of research impact.

**Instrumental model**

In order to unpack the instrumental model I would like to start from my personal experience of acting as a ‘knowledge broker’ for the United Kingdom government’s department responsible for planning. Between 2003 and 2007, I led the Planning Research Network for the UK planning ministry which at the time was called the Office of the Deputy Prime Minister (ODPM) and is now called the Department of Communities and Local Government (DCLG). The network consisted of some thirty senior planning researchers from universities, consultancies and research funding bodies. The aim of the Network was to advise the government on its planning research priorities and facilitate knowledge exchange between researchers and policymakers. It was one of the four networks that were established in the ODPM as a way of responding to the then Labour government’s promotion of evidence-based policy. I perceived the Network as being located between two neatly defined policy and research communities and charged with
the seamless transfer of policy concerns into research programmes and research findings into policy agendas. In short, my image of the Network is an illustrative example of the instrumental model of research impact.

According to this model, the impact of research on policy and practice happens through a linear and unproblematic process in which either research leads to policy and experts are on top, or research follows policy and experts are expected to be on tap (Davoudi, 2006). Weiss (1979) calls the former the ‘knowledge-driven model’ of research utilizations, and the latter the ‘policy-driven (or problem-solving) model’ (see also Cave and Hanney, 1996). Given the rapid cycle of policy change and the slow pace of research, in practice the latter often becomes the dominant expectation. The emphasis is, therefore, on speed and on the need for evidence to be timely, digestible and available on demand. The instrumental model has had a powerful influence on several approaches that have been developed for the assessment of research impact. For example, the much-cited approach advocated by Knott and Wildavsky (1980) and elaborated by Landry et al. (2001) is based on a ‘ladder’ of research utilizations, which has seven rungs:

- reception: research findings are transmitted to and received by users;
- cognition: research findings are read and understood by users;
- reference: research findings are referred to by users;
- efforts: users make efforts to make use of the research findings;
- adoption: research findings influence users’ decisions;
- implementation: research findings are implemented in the users’ policy;
- impact: users’ policies lead to a change in practice/behaviour.

Staged models such as the preceding are overly linear and based on instrumental assumptions that research is utilized in a sequential process, with all the steps having equal weight and none ever being skipped. As Davies et al. (2005) argue, it implicitly assumes that climbing from one rung to the other requires similar degrees of effort and that research impact can be realized only at the top of the ladder.

**Conceptual model**

Soon after the establishment of the Planning Research Network, mentioned earlier, it became obvious that in the real world the interface between policy and research does not map on to the instrumental model. The experience confirmed the observation that “neither . . . the academic nor the political has a particularly well-articulated sense of the other’s agendas, practices and discourses” (Jasanoff, 1996: 394). The experience confirmed the observation made by many scholars that research is only one input in the messy world of policymaking, and that research is always competing with other contenders – such as experience, political insights, ideological judgment, tacit knowledge and institutional memory – in its attempts to influence policy and practice. In short, the instrumental model upon which the Network was designed proved very limited in explaining the exchange of knowledge between researchers and policymakers. This, however, does not mean an absence of research impact. Indeed, the Network may be seen as a success if it is judged according to an alternative, conceptual model of research impact which considers research impact as indirect and non-linear and taking longer to realize. According to the conceptual model, research works by illuminating the landscape within which decisions are made (Davoudi, 2006), and by providing “a background of empirical generalizations and ideas that creep into policy deliberation” (Weiss, 1980: 381). Such impacts are hard to measure because
research is not neatly codified into tools, instruments, protocols or computer models which can be traced back to its originator. For example, research impact may include the absorption and internalization of research into professional tacit knowledge “as it emulsifies with many other sources of knowledge (experiences, anecdote, received wisdom, lay knowledge, etc.)” (Davies et al., 2005: 13).

Furthermore, a conceptual understanding of research impact suggests that the ‘users’ of research are not just identifiable policymakers or business entrepreneurs but also society as a whole, with numerous yet anonymous beneficiaries. Therefore, according to the conceptual model a meaningful and fair assessment of research impact should focus not just on whether a particular piece of policy is based on a particular piece of research, but also on whether society as a whole has been better informed because of the cumulated effects of research and scholarship. The impact may be less targeted and more diffused.

**Symbolic model**

This third model of research impact refers to the symbolic, rather than substantive, use of research. It is about the ‘political’ and ‘tactical’ utilization of research in policy and practice (Weiss, 1979; Lavis et al., 2002). In the former, research is employed to support and justify a predetermined position and enhance the credibility and acceptance of particular political decisions. In the latter, research is a resource to be drawn upon as a substitute for action on complex public issues. It is used, for example, to buy time and dampen down pressure for action. In both, research is a means to maintain a political stance either by bolstering or halting decision making (Davies et al., 2005). Research is not conducted for the purpose of solving specific problems or shedding light on the context for decision making. It is carried out and used as ammunition in adversarial decision processes. The planning system in the United Kingdom has been a particularly fertile ground for the political and tactical use of research because it is riddled with adversarial appeal cases, where the proponents and the opponents of a particular planning application commission their own research and expect that its outcomes justify their particular position. The result is sometimes a set of contradictory evidence for and against the case, which makes it more difficult for adjudicators (including planning inspectors) to reach an informed decision.

Such uses or, indeed, abuses of research raise important questions about impact assessment, such as: is all impact good impact? What is the purpose of assessing impact? Is it about learning how to enhance impact? Is it about making judgements on the level and quality of impact? Or, is it indeed about enacting market processes in the academy and performing “quantified control” on academic scholarship (Burrows, 2012: 2)?

**Factors affecting research impact**

There is a plethora of good practice guides on how to increase the impact of research. The vast majority of these tend to reduce the problem of ‘little effect’ to the problem of miscommunication. Therefore, it is not surprising that they are often solely focused on presentational issues, ranging from the use of ‘simple’ language to the shortening of the length of the reports and to the optimal position of the report’s logo and the attractiveness of the cover page. For example, the EU Directorate General on Research has issued a guideline on how to communicate ‘research for evidence-based policy’. This stresses that “generally, the length of . . . policy brief should not exceed ten pages. Experience suggests that most briefs can be accommodated in eight pages, though some may be as short as six” (EC, 2010: 16), because decision makers are
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reluctant or do not have time to read anything longer than that. It also states that “the project logo should appear directly opposite the blurb, in the left-hand column” (ibid). Elsewhere under the heading of “Power of page one”, the guideline suggests that, “Vulnerable to the power of first impressions, people routinely judge policy briefs by their covers” (ibid.: 17).

Guidelines such as these, useful as they might be, tend to treat research as a commodity which can attract potential ‘users’ in what is becoming a crowded market only by appealing to their sensory perceptions. They tend to overlook other critical factors which can affect research impact – notably, the content of research, the processes through which research is conducted (and communicated) and the context within which these processes take place (Pettigrew, 1990). The content is about the scientific quality and credibility of research, which can be influenced by research inputs such as resources, existing knowledge, past experience and expertise. It is then manifested in research outputs, including innovative theories, concepts, methods and tools. It can also be manifested in capacity building such as developing new skills, research training, career development and network formation. The process is about the nature, level and effectiveness of dissemination and modes of interaction and communication that are used. Such interactions can be both targeted (such as advisory work and formally organized networks) and diffused (e.g., through research mobility, publications and media appearances). The context is about the demand environment (such as policy need for research and the timing of the research results) and the beneficiaries’ level of receptiveness. Context plays an important part in the understanding of the dynamics of research impact, particularly if the purpose of assessment is learning. As Walter et al. (2004) argue, research impact is contingent on the context in which it is used. The extent to which those who are the targeted beneficiaries of research (or indeed the society as a whole) are receptive to new knowledge and demand well-informed deliberations plays a significant part in the generation, quality and impact of research. An important element of context is the research funding bodies, including government organizations, whose priorities shape the type and form of research that is funded.

Conclusion

Knowing the impact of publicly funded research is a legitimate concern of the funding bodies. However, developing a deeper and more realistic understanding of research impact requires moving away from the dominant instrumental model of impact assessments, which privileges quantifiable and easy-to-monitor measurements. These are incapable of capturing the diversity, breadth, complexity and contingency of non-academic impacts of research. Such approaches are particularly limited when applied to social science research, whose impact on practice and policy is often less tangible, more indirect and less immediate. A great deal of planning research falls into this category. Social scientists’ ideas and concepts often percolate into decision-making processes in such a way that makes it more difficult to identify and distinguish between the originators and the users. Although the difficulties of tracing intangible and convoluted impacts are particularly pronounced in social sciences and humanities, they are not exclusive to them. Indeed, many scientific discoveries are also the outcome of cumulative research, by several unknown researchers, undertaken over decades and in multiple sites. The lack of a clear lineage does not mean that research is of no relevance to society or makes no difference to people’s lives; it may simply mean that we need to revisit our conceptualization of ‘impact’ and the purpose of impact assessment.

If impact assessment is about making the connections between science and society more visible and more fruitful, then it has to go beyond the instrumental model and embrace a broader
perspective on how research creeps into practice, and vice versa, in ways that are sometimes impossible to disentangle, let alone to measure. Otherwise, in the rush to formalize, quantify and institutionalize impact assessment we may lose sight of or devalue that connection. If we get fixated on measuring things, we are in danger of changing the very things we measure, which is ultimately independence of thought and intellectual rigour drawn upon to engage with the concerns of society at large. We are in danger of undermining the role of public intellectuals.

Notes

1 This was later rejected by Snow himself as snobbery.
2 The criterion of ‘reach’ for impacts does not refer specifically to a geographic scale but is interpreted along with significance by the assessment panels.

References


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