

## The importance of the tacit dimension and the limits of “impact” for research (0012)

There are three quite distinct contrasts which cut across each other in Michael Polanyi's claim “that *we can know more than we can tell*” (Polanyi 1967, p4), where the tacit describes the right-hand side term:

- contrast (1), articulated/codified knowledge vs practical knowledge (or, meaning the same, discursive/linguistic vs non-discursive);
- contrast (2), focal awareness (explicit) vs subsidiary awareness (implicit).
- contrast (3), the explicit (what is understood about the world) vs the hidden (in the sense of a solution to a new problem about the world yet to be uncovered), and it is this sense which is relevant for understanding research as a mode of learning about the world, with the focus on the object of research.

There is a dual connotation to the term "object": one sense concerns what the enquiry is enquiring into, the object being measured or otherwise articulated, what the enquirer is trying to learn or find out more about; the other sense concerns the purpose of the research endeavour, what the enquirer is doing it for. Both senses are important in articulating a response to the growing “impact agenda”, a key aspect of current policy debate in the UK at least and an attempt to construct a framework to measure the worth of research and research proposals by the social, and especially economic, benefits they would bring about.

Policy consultation is ongoing about how much such impact, once demonstrated, should count in the periodic exercises in assessing the value of research output and the concomitant financial payback out of the public purse to higher education institutions linked to the performance of their academic departments (in turn considered as a group of individuals). With the replacing of the "Research Assessment Exercise" with the new "Research Evaluation Framework" the overall performance of departments and their individual researchers is likely to be linked more with demonstrated impact at the expense of for instance the quality of scholarship in scholarly papers.

The impact agenda has also been invading the proposal process: asking applicants for research grant awards to explain what benefits they expect will result from their proposed project, to contribute to the justification for it being funded is provoking angry responses that such a requirement will have the effect of distorting the proposals, and in time the very research themes and direction of the development of knowledge of the discipline (Corbyn 2009).

The problem is less clear, however, in applied science and in social science disciplines. If you as a researcher are interested, as your object matter of your research enquiry, in certain technologies, concerning, say, aeronautics, you can easily state as your object or purpose the advancing of those existing technologies and also consonant economic and social benefits of making aeroplanes faster or more reliable. Much social science research deals with 'nitty-gritty' data, much of it quantitative, with a view to finding out more about an identified phenomenon, and much is commissioned by independent bodies with a social purposes in mind. For instance, you might be interested in tracking the changes in social attitudes over a population over time, which could inform various policy initiatives (government or business).

If the types of research I describe in the last paragraph are typical, the impact agenda should not change much research practice. Critics of this agenda are quite right to complain that they do not constitute the only paradigm of research. Researchers in philosophy and the

pure natural sciences, especially but not exclusively, have the problem which is to articulate adequately what specifically they are trying to do. Ultimately, this articulation cannot be done in advance because this paradigm is research as discovery, which leads us to consider an aspect of Michael Polanyi's "tacit dimension", namely the "hidden". As one of these researchers and critics of research policy, you need in order to maintain your stance, for the sake of consistency at least, to be appreciating the metaphysical importance of Kant's noumenal world for the object of research, and the ground for Polanyi's "hidden". For Kant the phenomenal world, things-as-they-appear, is all that we understand about objects, things that we can perceive and conceive. Kant hypothesises that there is a world or aspects of the world that are beyond our knowledge, things-in-themselves, or the noumenal world.

Graham Priest supports Kant's logic, with the proviso that Kant needs to cease concern about genuine contradiction (Priest, chapter 5). Polanyi (1967, pp21-25), indeed, poses the complementary challenge to the possibility of research as discovery in the form of a paradox from Plato's *Meno*: if you do not know already what you will discover then you will not be able to recognize it but if you do know already then the discovery cannot be original. Put in Kantian terms, the Platonic paradox is that the tacitness of the noumenal, being outside our knowledge, will be the origin of the discovery but we cannot articulate it if it is in the noumenal. What you claim to be a discovery you have to articulate to publicise, which means it is already in the phenomenal realm. But how did you come across the new thing if it was outside the conceptual boundary and not constituted in a fashion such that we could perceive it already with our cognitive faculties? The noumenal is what has been always beyond our powers of cognition, a statement of our finitude, although we may make gradual incursion into it as we find out more about the world and, metaphysically speaking, the "torn boundary" thesis allows us to do this:

"...limits of thought are boundaries which cannot be crossed, but yet which are crossed [...] there is a totality [...] and an appropriate operation that generates an object that is both within and without the totality..." (Priest, p4)

The poignant aspect of research is the finding and articulating of new knowledge. But we have to be silent about what our research may uncover, until uncovered. And that is the ground for the impossibility of articulating the specific impact of such research.

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