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## Commentary: Facts, Opinions and Affairs Du Coeur

**Simon Carter**

It is now over thirty years since Marmot began his comment article(1) with the provocative quotation "... these are the opinions on which I base my facts...". Here Marmot identifies a central tension found in many branches of the sciences: namely, how is it possible to evaluate and choose between the competing theories or hypotheses that often seek to explain the same phenomena? A concern that is more germane than ever. He asks if, despite all their scientific trappings, the decisions taken by scientists are not simply based on the irrational and the intuitive? And, as Marmot points out, this is not merely an academic argument, it has significance for understanding the causes of disease that are of deep importance to a wider public.

He uses a case study of coronary heart disease (CHD) to suggest an answer to the thorny question of how to choose between competing theories. Four accounts, taken from the philosophy of science, were chosen to shed light on this issue: the inductive view; the Popperian view of science; the theories of Lakatos; and Kuhn's account of revolutionary science. The inductive view, that once sufficient 'facts' are gathered a 'correct' theory will emerge, was dismissed as essentially hollow – theories do not emerge fully formed from the dry dust of facts and the 'history of science commonly gets reconstructed from an inductive viewpoint'(1). The Popperian view that theories are tested by attempts to falsify them, while more attractive is equally doomed because it is always possible to save a theory by the presentation of an 'auxiliary hypothesis'.

Marmot finds the remaining two accounts of science to be more productive. The Kuhnian(2) view is that in periods of 'normal science' the work of most scientists consists of tinkering and problem solving – in other words adding footnotes to an established 'paradigm'. Periods of revolutionary science in contrast are marked by the accumulation of anomalies and the breakdown of an existing paradigm and the emergence of a new one. This new scientific knowledge would, according to Kuhn, be 'incommensurate' with the old paradigm – like all revolutions those on the losing side would not even be able to comprehend what had overtaken them. While this view may throw light on some developments in scientific knowledge it is doubtful if it has much to offer in terms of understanding debates concerning CHD – as these, from a Kuhnian point of view, largely represent problem solving within the 'normal science' of epidemiology. Indeed, periods of revolutionary science probably occur rather infrequently (one such example might be the increasing influence of modern epidemiology on medicine and public health policy in the 20 century). Marmot finds that the theories of Lakatos provide the most compelling aid to judging between competing research findings. In this view an account can be retained as long as it remains progressive – a theory that encounters anomalies can be thought of as progressive as long as these point towards new empirical observations. Thus, using

Marmot's example, if one believes a causal factor in CHD is fat intake and one discovers a group with a high fat intake but low levels of CHD it might indicate that not all types of fat cause disease. In other words, this anomaly points towards a new empirical observation that might allow the theory to remain progressive. This Marmot argues is a valuable insight from the philosophy of science because it provides some practical help to the scientist in choosing between competing theories.

One of the central arguments running through the Marmot article is that scientists in general and epidemiologists in particular, can learn valuable lessons from studying the history and philosophy of science. In the period since his article appeared many anthropologists and social scientists have increasingly turned their attentions towards the natural sciences and scientific practices and given birth to a new sub-discipline – science and technology studies. What insights might be now be added to complement Marmot's call to take 'philosophic speculations' seriously?

One of the first is that the nature of knowledge production and consumption has increased quantitatively and altered qualitatively and since the mid 1970s – for example the World Wide Web did not exist in any meaningful way before the 1990s. It is now no longer just epidemiologists that have to decide between competing epidemiological theories about the causation of disease but also the wider lay public. Public health education campaigns are frequently based on epidemiological insights, albeit often loosely and in a somewhat simplified way. Additionally, there are frequent media reports about the findings of research conducted by epidemiologists. Thus scientific evidence, generated within the discipline of epidemiology, is increasingly being translated for non-epidemiological settings. Both health professionals and lay groups are involved in appraising and judging these translated forms of scientific knowledge and in making decisions based on them. However, this process is rarely unproblematic. Differing pieces of epidemiological evidence, when removed from the context of their own discipline, may often appear to conflict with or contradict one another. The public understandings of these translated forms of knowledge have been explored by anthropologists in the now classic studies of 'lay epidemiology' and CHD(3, 4) to explain how people critically judge simplified versions of epidemiological risk assessment against a more complex background of social and cultural knowledge about everyday health practice and outcomes.

In addition, technical knowledge can be incorporated into new contexts in selective or unplanned ways. To give one example, in 1995 the Committee on Safety of Medicines (CSM) issued a warning, based on epidemiological evidence, which indicated that there was a very slight increased risk of venous thromboembolism associated with some forms of the oral contraceptive pill(5). The warning from the CSM was widely covered in print and other types of media generating headlines such as: 'If men had to take the pill my sister would not have died' (Daily Mirror, 1995); and 'Danger Pill: 1 ½ million Woman Warned' (Daily Mail, 1995). There followed much debate about this crisis, particularly on the role of the media and the

way the Department of Health handled the release of information. What is clear however is that significant numbers of women immediately, and understandably, made the decision to stop taking oral contraceptives(6, 7). The more recent and continuing controversy surrounding public debates over the risks and benefits of MMR has raised issues of scientific accountability(8) and the public understanding of epidemiology.

Although we might argue that knowledge production and consumption has expanded, the questions posed by Marmot have not abated. Indeed the problem of choosing between competing accounts has amplified considerably because now a whole range of professionals and lay publics have to pass judgment on diverse epidemiological explanations of disease with, arguably, few tools to help them. This is exacerbated because, as noted earlier, whereas the findings of epidemiological research frequently find their way into public domains, the actual craft of the epidemiologist is largely hidden from view. Why might this be so? In many ways the science of epidemiology, more than any other science connects the natural world of the biological, embedded organism with the social world of populations and society. But as the philosopher of science, Latour(9) points out, this is not an easy relationship. Latour contends that science has to be understood as part of the 'modern constitution' that emerged in the seventeenth century from Thomas Hobbes and Robert Boyle's constructions of science and politics. At a time of tumult in English society Boyle and the newly founded Royal Society devised a way of examining 'nature' that was independent of the observer and which gave rise to modern experimental science. At the same time Hobbes discovered a way of speaking about politics and social order that was separate from material circumstances and appealed to an abstract 'culture' or 'society'. In effect, according to Latour, Boyle and Hobbes established a separation in our understanding of 'nature' and 'culture' – each was purified with the traces of the other being hidden from view. But this was always a fiction because there have existed an increasing myriad of representational paradoxes, or 'hybrids', that mix politics, science, technology, nature and society. The ozone debate and global warming provide us with useful examples of such hybrids. In conclusion Latour suggests we should stop this attempt at the purification of the modern world by bringing these hybrids into full open view.

Epidemiology has, perhaps more than many other disciplines, always stood at the confluence of politics, science, nature and society. According to a Latourian view this helps explain why the craft and practices of the epidemiologist have remained hidden. Now that the discoveries of epidemiology are increasingly becoming a focus of public debate perhaps it is also time that the inner workings and skills of the epidemiologist were also exposed to a wider public view. Perhaps then we would all be in a better position to judge competing accounts and honestly say 'these are the opinions on which I base my facts...'

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