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The new management of scientific knowledge in medicine: a change of direction with profound implications

Bruce G Charlton

Introduction

In 1993 I published a *Lancet* Viewpoint entitled ‘Management of science’ in which I pointed out that purchasing managers in the National Health Service had begun implementing very precise contracts which controlled detailed aspects of clinical practice (Charlton 1993). For instance, it became mandatory to specify protocols for beta-blocker and aspirin treatment following myocardial infarction, and for prescription of benzodiazepines, antidepressants and other psychotropic drugs, and for aspects of skin cancer management. The justification for these blanket recommendations was that these interventions were ‘scientifically’ proven to be effective across the board in almost every clinical circumstance – at least according to the NHS Management Executive’s interpretation of the results of large randomised trials (although expert clinicians may frequently have disagreed; see, for example, Julian (1995)).

But the fundamental problem was structural: the Government was creating a managerial structure that separated the power to influence treatment from clinical responsibility for the consequences of that treatment. For example, managers were claiming the right to determine the nature of a drug prescription, while doctors remained both morally and legally responsible for the outcome. This seemed self-evidently unethical, and placed doctors in the impossible position of being vulnerable to sacking if they were disobedient, and to malpractice suits if they obeyed. The system was also open to corruption, since political influence could be brought to bear on managers, tending to generate protocols using criteria dictated by expediency rather than by effectiveness. For example, the call for specified protocols on psychotropic drugs was unsupported by any rational consensus on what such protocols should contain, and the instructions concerning ‘skin’ cancer (and the failure to differentiate between basal cell, squamous cell and melanomatous malignancies) seemed more justified by the immediate demands of public relations than by any body of solid scientific evidence.

As things turn out, I had underestimated the seriousness of this kind of threat to clinical practice, and my article unfortunately proved to be prophetic of a trend which has culminated in the creation of the National Institute for Clinical Excellence (NICE) and the Commission for Health Improvement (CHI). The management of science in medicine is now established by statute, and – even worse – the criteria

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Key

Upper case = objective occurrences

Lower case = uninformed opinion

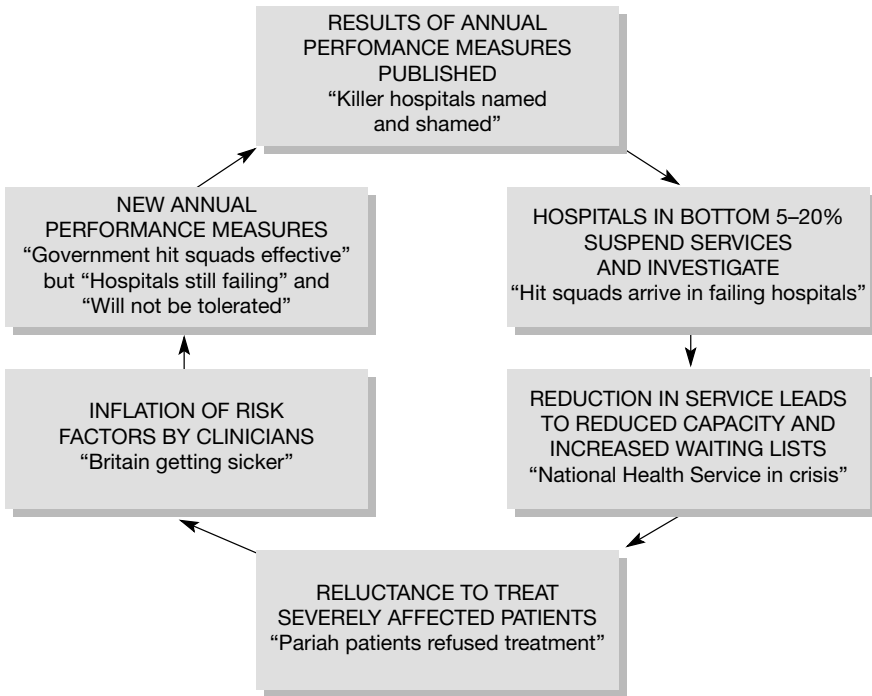


Figure 10.5 Problems with performance indicator league tables

might be helpful; however, I now believe that the intention to use guidelines reveals reductionism to be running wild. It is inappropriate and unbalanced, and it ignores Type II and III complexity. If it is allowed to succeed, those of us who deal with Type II and III complexity will be spending most of our time explaining why there were special circumstances that applied to particular patients even if our performance indicators were good. It could be argued that we have to accept that ‘we have to be accountable’, but I suspect more of those with generalist leanings will in future opt for a quieter life by entering reductionist specialties. I have no argument against reductionism in general. Indeed reductionism, by focusing on particular questions, has been a major method of scientific discovery but I am against the idea that the quality of care of complex patients merely comprises the serial ascertainment of fixed, non-interactive diagnoses that will yield to serial application of Type I complexity guidelines. The medical profession recognises this when things go wrong and guidelines do not work, as they do from time to time, when we say that the patient has ‘developed complications.’ Patients do not develop complications, but rather reveal previously unsuspected complexity.