Postscript, 2007

"great truths may be very near and yet not be discerned"
Sir James Paget, 1879

Opinion about *Bad Medicine* is polarised. Broadly speaking, doctors (who take progress seriously) love it, and historians of medicine and of science (who don't) hate it. Since the book is about the way in which a profession can become wedded to irrational and unjustifiable assumptions, and since it calls in question mainstream medical history, it would be disconcerting if the historians had greeted it with enthusiasm. In fact the reception of *Bad Medicine* has been a striking example of just the kind of resistance to innovation that the book itself analyses.

One reason *Bad Medicine* has polarised opinion is that its own argument is conducted in terms of polarities: good medicine and bad medicine, heroes and villains. The question of whether it is permissible to write "accusatory" or "celebratory" history is the most interesting one raised by the critics, and it is evident that I did not address it fully in the book's opening pages. There are in fact a number of interlocking issues that need to be prized apart. First, my opponents advocate what anthropologists call "charitable interpretation": we should understand the past in its own terms, and make our best efforts to bring out the rationality of viewpoints which at first seem alien to us. The problem with charitable interpretation as a methodological principle is that it is designed for societies in which everyone thinks more or less alike. But if you want to understand a dispute (and there have been disputes about medicine since the days of Hippocrates) understanding one side's point of view "in its own terms" involves criticizing the other side; a "charitable interpretation" of one side's arguments is of necessity a "stranger's account" of the other side's. There is no choice: as I say, "You have to take sides." For a striking example of this see Steven Shapin's and Simon Schaffer's *Leviathan and the Air Pump* (1986), in which they take Thomas Hobbes's side against Robert Boyle on the question of the possibility of a vacuum.

A crucial difference between *Leviathan and the Air Pump* and *Bad Medicine* is that Shapin and Schaffer defend what looks to us like the losing side, while I give my support to those scientists and doctors who turned out, as we see it, to be in the right. It is not taking sides that my critics really object to, I think; it is taking *this* side. History is written for the living, not the dead, so every history book, whether avowedly or only implicitly, is an intervention in our own culture and involves some sort of taking of sides: historians of science who refuse to write about progress are, explicitly or implicitly, questioning the role of science in our own society. What is shocking about *Bad Medicine* is that it quite openly employs hindsight to decide which side to take. Critics who say that it relies on "twenty-twenty hindsight" or on the use of what doctors call a "retrospectoscope" are making a serious point. There's no doubt, for example, that the book looks closely at early germ theories of disease because the germ theory of disease turned out to be broadly correct, and this is a judgment made with the benefit of hindsight. There are plenty of theories – those of Paracelsus, or van Helmont, for example – that *Bad Medicine* passes over quickly, because with hindsight we know they were incorrect.

Argument from hindsight is not always bad history, and I want first to present two reasons for thinking such arguments are permissible, and then later I will go even further and claim that in history of science hindsight is indispensable. Let us think, for a moment, not about curing diseases, but about propulsion. For people in the seventeenth century there were only three reliable forces of propulsion: human and animal muscle power (horses, oars, etc.); the wind (sails on boats and on windmills); and water (watermills). Within these three broad categories of power-source there were all sorts of improvements that were possible – the introduction of the fan-tail on windmills for example (1745), or the invention of the bicycle. It makes sense to ask what the preconditions for those improvements were: Why was the fan-tail never adopted in France? Why was the bicycle not invented until 1865? And there was of course a power source that everybody had some experience of, but whose significance had simply not been recognized: steam. Thomas Savery patented the first steam engine in 1698, although Hero of Alexander had already used steam power in the first century AD, as had Giambattista della Porta in 1606.

In a pre-electrical world there is a finite, and specifiable, set of possible sources of power. One can call it *hindsight* that enables us to identify them as muscle power, wind power, water power, and steam power, but one can also call it *insight*. Some lines of enquiry were promising, others (perpetual motion machines, for example) were doomed to failure. We can say this because there are objective limits on which technologies will work and which ones will not, and it is therefore interesting and productive to ask what the obstacles were to inventing the steam engine before 1698, the fan-tail before 1745, or the bicycle before 1865. A history of technology which treated perpetual motion machines in the same way as it treated steam engines would be strange indeed. However some people insist that this is exactly how history of science should proceed. They advocate what is called the "symmetry principle" – that one should give the same kind of account of false beliefs as of true beliefs, of irrational beliefs as of rational beliefs. *Bad Medicine* does not respect the symmetry principle, and it would be irrational to do so.

Others, who do not go quite as far as to adopt the symmetry principle, still do not think that science is constrained by objective limits. If you think science is socially constructed, then the number of possible sciences is infinite, and the sciences you actually get are a purely contingent and unpredictable outcome. If you think like this, then hindsight always misses the point, which is that the future could not possibly have been predicted. This seems to me plain wrong about science, and particularly about technology. Technologies develop in directions that are constrained by the laws of nature, and by and large (with the interesting exceptions of astrology, alchemy, and Hippocratic medicine) fantasy technologies are easy to identify and are quickly abandoned. There were no *alternatives* to the steam engine in the sense that there might be alternatives to constitutional monarchy, or utilitarianism, or tragic drama, or the morse code. Once a serious search for a new power source began, it was only a matter of time before the steam engine was invented. So too, if medicine was to become effective, there was no *alternative* to the germ theory of disease, or to its application in the form of vaccines, antiseptics, and antibiotics. One can imagine a different timing and pace of progress (indeed I argue we need to take seriously the idea that the timing and pace of progress could have been very different); one cannot conceive of progress taking place in a quite different direction. In situations like this it is perfectly permissible to use hindsight in order to concentrate one's attention on valid lines of enquiry, to ask what the obstacles to them were and how they were overcome. This does not mean that one should ignore failed lines of enquiry (the history of perpetual motion machines is of real interest): but there is no need to treat them as if they could have succeeded.

There is a second version of the argument about hindsight that also looks, at first, as if it deserves to be taken seriously. In this book I frankly admit that if we are going to talk about "good" medicine (medicine which works) and "bad" medicine (medicine which performs worse than a placebo, and may do harm), then we are also going to have to praise and blame individuals who were able to tell one from the other. My critics take exception to this way of thinking, and yet I am grateful to one of them, Chris McManus, for an example that shows that it comes naturally to people caught up in the midst of an intellectual revolution. He reports that Sir James Paget, the Victorian surgeon and pathologist, looking back in 1879, was dismayed and puzzled when he realized how unnecessary had been the delay in inventing anaesthetic surgery, reflecting how "great truths may be very near and yet not be discerned." "In explanation," McManus says,

> Paget emphasizes "the misery (of painful operations) was so frequent, so nearly customary, deemed so inevitable that, though it excited horror... it did not excite to strenuous action"... Paget did not expect a kindly verdict from history: "Our successors... will look back with horror, and on us with wonder and contempt for what they will call our idleness or blindness or indifference to suffering."

What could be wrong with writing precisely the sort of history that Paget foresaw, a history that looks back with horror and wonder at idleness, or blindness, or indifference to suffering?

In 1930, Cecil Paine, working in Sheffield, was the first doctor (or at least the first since Lister) to make clinical use of penicillin. Paine, who had read Fleming's 1929 article on penicillin, used “crude mould juice” to treat eye infections that were resistant to all available therapies – gonorrheal infections in the eyes of newborn babies, and a pneumococcus infection in the eye of a man injured in an industrial accident. He had remarkable success, but abandoned his work when he was posted to a new job. Wainwright and Swan, who have written the history of Paine’s work with penicillin, defend his failure to recognize the potential of mould juice. We must, they say, avoid hindsight, and recognize that in the 1930s doctors were looking for antiseptics not antibiotics. But Paine himself took a different view. “At the end of our interview Dr Paine was asked where he placed himself in the penicillin story. He replied: ‘Nowhere – a poor fool who didn’t see the obvious when it was stuck in front of him’.” Both Wainwright and Swan on the one hand, and Paine on the other are right: it would have been remarkable if Paine had grasped the full implications of the cures he was performing; but it is also true to say that the obvious was stuck in front of him – and he, after all, was alone at the time in having grasped the significance of Fleming’s 1929 article, alone in having started to use penicillin to kill off infections, so that the obvious was stuck in front of someone who really *was* more than half way to grasping its significance. Contempt seems entirely the wrong response here, but a certain amount of wonder and dismay is surely in order. Since participants in the events see no problem in employing hindsight, historians too should be allowed to make use of it.

We’ll come back to the question of hindsight shortly, but I want first to acknowledge another respect in which the argument of the book is incomplete, and I can now develop it a little further. I am not the first to claim that until fairly recently medicine did more harm than good: Shapin tells us that “The Harvard biochemist L.J. Henderson [1878-1942] was supposed to have remarked ‘that it was only sometime between 1910 and 1912… that a random patient, with a random disease, consulting a doctor chosen at random, had, for the first time in the history of mankind, a better than 50-50 chance of profiting from the encounter.’” Most historians of medicine have encountered this argument in some form, although for the most part they choose to ignore it: this is the price they must pay if they are to avoid writing about progress. The result is that there has been no serious debate about how much good traditional medicine did (if any), though Sheila Ryan Johansson has argued that medicine extended elite life expectancy in the period 1500 to 1800. I think she is mistaken: the improvements in life expectancy she attributes to medicine were, I would argue, attributable to improvements in diet and hygiene.

I am also not the first to claim that when pre-twentieth-century medicine worked it was by mobilizing the placebo effect. The claim was made by Arthur K. Shapiro and Elaine Shapiro in *The Powerful Placebo* (1997), and before them by W. R. Houston, writing in the *Annals of Internal Medicine* (1938). The Shapiro book is widely cited by doctors; it was reviewed in three medical history journals, but it has only once been cited in an article published in such a journal. Because they have avoided the question of how far traditional medicine worked, historians of medicine have also avoided the Shapiro thesis.

When I wrote *Bad Medicine* I did not for a moment imagine that the fact that Hippocratic medicine did more harm than good was *my* discovery. But I failed to say whose discovery it was. This was for the simple reason that I did not know. I was clear that Pierre-Charles-Alexandre Louis had not grasped that bloodletting necessarily did more harm than good. But who did first understand this simple fact? (It is, indeed, a fairly simple fact: it was apparent to Robert Boyle in the 1660s, although he was too frightened of the doctors to say so in print.) Standard histories of medicine do not address the question. The only clue I had when I wrote *Bad Medicine* was a passing reference by Carlo Cipolla to a Dr. Dietl in Vienna and a Dr. Bennett in Edinburgh. Now, belatedly, I can give them their place in my argument.

The classic English-language attack on bloodletting was written by John Hughes Bennett (1812-75), a professor at Edinburgh who is now remembered mainly for having given the first account of leukaemia as a blood disorder. “Observations on the Results of an Advanced Diagnosis and Pathology applied to the Management of Internal Inflammations” appeared in the *Edinburgh Medical Journal* in 1857. Hughes Bennett was drawing on the work of Jospeh Dietl, *Der Aderlass in den Lungenentzundungen* or *Bloodletting in Pneumonia* (1849). Dietl and Hughes Bennett were the first to produce statistical evidence comparing treatment with bloodletting with no treatment, or with what amounted to placebo treatment, and to show that no treatment was markedly preferable to traditional treatment. (In 1851, in a work unavailable to Hughes Bennett, Dietl showed that bloodletting tripled the death rate in pneumonia).

Hughes Bennett ought to be an important figure in any history of medicine, for not only was his attack on bloodletting decisive (or at least it should have been – as we have seen, Osler was once again recommending bloodletting in pneumonia in 1892), but he was one of the first in Britain to place the microscope at the centre of a medical education. The date of his work on bloodletting is important: traditional therapy still retained an intellectual credibility until the middle of the nineteenth century, right up to the revolution represented by germ theory. Once the old fantasy technology had finally been abandoned, it took only a decade to produce a medicine that really worked. But Hughes Bennett, statistician and microscopist, had no part in that revolution. Dietl, having recognized the deleterious effects of traditional therapies, turned to hydrotherapy – at least he stopped doing harm. Hughes Bennett took a different path. He put his faith in the idea of a new medical science, but unfortunately he had an uncanny ability to make the wrong choices. In 1857 he was proselytizing for the chemistry of Justus von Liebig: this was two years after Snow had shown, in the case of cholera, that one had to think of infectious diseases as caused by organisms (or something similar), not, as the followers of von Liebig claimed, by poisons (or something similar). Hughes Bennett then turned to work on the Pasteur-Pouchet debate. He produced a learned and persuasive article ("The Atmospheric Germ Theory", *Edinburgh Medical Journal*, 13 [1868], 810-34), based on his own elaborate and painstaking experiments, an article proving that Pasteur was wrong and Pouchet was right. He published this ambitious article the year after Lister first published on antiseptic surgery, putting Pasteur's germ theory of putrefaction to work, and it seems improbable that Hughes Bennett had not already heard of the work Lister was doing in Glasgow, just a few miles away (though he cites only Lister's early article on the pigmentation of the skin of frogs).

Hughes Bennett is a striking example of how easy it is to back the wrong side during a scientific revolution. I recommend him to historians of medicine who want to write history without hindsight.   Advocates of the symmetry principle may find it interesting to give an account of Hughes Bennett's contribution to the spontaneous generation debate written on the assumption that *he may have been right, and Lister may have been wrong*. It is perfectly possible to write such an account, providing we leave out a fact that we can only know through hindsight, now that the debate about spontaneous generation has finally been settled, a fact that was invisible to Hughes Bennett and is therefore missing from the historical record: Hughes Bennett's techniques for sterilizing his experimental equipment were inadequate. (Since he believed no living creature could survive a temperature of 100^{o} C it is likely that he skimped on the procedures advocated by Pasteur.)

Hindsight is sometimes not just permissible but indispensable. You cannot write the *history* of a scientific dispute until you know the outcome, because until then the basic facts are in dispute. If you intervene in a scientific dispute before it is over, you are writing science, not history. If you were to write, after the outcome is known, pretending the facts might be other than they are – that the sun might go round the earth, or germs generate spontaneously – then you would be writing science fiction, not history. There *are* varieties of history that you can write without employing hindsight. History of science is not one of them.

If I were writing *Bad Medicine* now, Hughes Bennett would have a central place in my story. So would an obscure eighteenth-century doctor, William Taplin (1740?-1807). By 1789 Taplin was well on his way to making his fortune by marketing pills for horses: in that year his *Gentleman's Stable Directory* appeared in its ninth edition. But before he became a farrier, Taplin had evidently tried to make a living in medicine, and in 1789 he published under a pseudonym ("Gregroy Glyster, an old practitioner") a humorous work, *The Æsculapian labyrinth explored; or, medical mystery illustrated* (retitled in its third edition *A Dose for the Doctors*). This wonderful little book should be read by every historian of medicine.

One of the central questions raised in *Bad Medicine* is how traditional medicine survived when it did no good. Half of the answer is provided by the mobilization of the placebo effect; but the other half of the answer is that doctors learnt to mislead their patients into thinking they were doing good when they were in fact doing harm, just as astrologers learnt to adapt their horoscopes to the hopes and fears of the individuals they had in front of them. I was clear when I wrote *Bad Medicine* that traditional medicine was an elaborate confidence trick, one which deceived doctors as well as patients. But where could one find an account of how the trick was performed? The answer is in Taplin's *Æsculapian Labyrinth*. The purpose of the book is to instruct every sort of medical practitioner (doctors, surgeons, men midwives, apothecaries) on how to maximize their income. Taplin writes on the assumption that actually curing patients is completely irrelevant to success: what matters is creating the right image. So a doctor should seem always to be in a hurry, and should keep a carriage standing at his door, so that prospective patients will be convinced that he is in constant demand. He should never return in his carriage by the same route as he drove out, for he needs to be seen on his rounds by as many people as possible. When visiting a patient you must

> take care to *look* wisdom in every feature; speak but little, and let it be impossible *that little* should be understood; let every hint, every *shrug* be carefully calculated to give the hearers a wonderful opinion of your learning and experience. -- In your *half-heard* and mysterious conversation with your *medical inferior* [the apothecary], do not forget to drop a few observations upon – "the animal oeconomy" – "circulation of the blood" – "acrimony" – "the non naturals" – "stricture upon the parts" – "acute pain" – "inflammatory heat" – "nervous irritability", and all those *technical traps* that fascinate the hearers, and render the patient yours ad libitum.

The doctor must adapt himself to the rank of his patients, "regulating your behaviour… from the *most* pompous *personal ostentation*, to the meanest and *most contemptible servility*." If you are a surgeon you should display in your consulting room a profusion of skeletons and of anatomical specimens, "both wet and dry". "Remember to let the *certificates* of your professional qualifications, from your different *lecturing tutors*, be so placed (in elegant frames) as to meet the eye in a conspicuous direction…"

The Æsculapian Labyrinth is a satire, but Taplin chooses to abandon his pose of deepest cynicism at the end: "A steady observance of the iniquity of medical practice has long since powerfully convinced me of the absolute necessity of professional reformation"; in the meantime his goal is to arm "the public with a weapon of self-defence." Of course his is not an unbiased, objective account of the practice of medicine in the eighteenth century – but it tells us more about the doctor-patient relationship in the centuries before antibiotics than any medical textbook. And it reminds us that the call for professional reformation is as old as the practice of medicine: there never was a time when everyone was taken in by the doctors.

The key obstacle to medical progress, this book has argued, was not economic self-interest, for, as Taplin recognized, new science was every bit as good as old for entrapping patients; nor was it some insuperable intellectual obstacle; it was the cultural identity of the medical profession, an identity transmitted through the texts of Hippocrates and Galen, and symbolized by the leech, the lancet, and the tourniquet. What held doctors captive was an imaginary world of their own creation, and the history of medicine may end as a history of science, but it needs to begin as a history of the medical imagination. The idea of such a history may seem a strange one, but it is an idea as old as the modern idea of science, and any attempt to distinguish between real sciences and fantasy sciences leads straight to it. In *Novum Organum* (1620), Francis Bacon described a number of ways in which the human mind can be led astray. He gives each of these sources of error the name of Idols because, like a believer worshipping a false god, we go astray while convinced we are still on the right road. The last source of error is what he calls the Idols of the Theatre:

> Lastly, there are Idols which have immigrated into men's minds from the various dogmas of philosophies, and also from wrong laws of demonstration. These I call Idols of the Theatre, because in my judgment all the received systems are but so many stage plays, representing worlds of their own creation after an unreal and scenic fashion.

Substantial Revisions

Add to Acknowledgements: The paperback edition has benefited from discussions with Robin Briggs (on scurvy – which has resulted in substantial rewriting on p. 161) and Michael MacKay (on William Taplin).

p. 99, line 16: but the final chapter of the *De Fabrica* (which only appeared in English translation in 2003)

p. 161 replacing para beginning on line 1: One estimate is that two million sailors died of scurvy between Columbus's discovery of America and the replacement of sailing ships by steam ships in the mid-nineteenth century. This estimate is much too high. Typhus killed more sailors than did scurvy, and sailors who disappear from ships' crew lists have often deserted rather than died. So while it has been claimed that of 184,899 sailors who served in the British fleet during the Seven Years War, 133,708 died from disease, mostly scurvy, the real figure may be closer to one tenth that, of which the majority will have died of typhus. It has been argued that almost 90% of the 2,000 men commanded by Anson on a voyage to the Pacific in 1740 died, nearly all of scurvy; but the death rate was 70% (largely from typhus and shipwreck), and most of those with scurvy survived. The normal death rate from scurvy on long voyages was not, as has been claimed, 50%: 5% would be nearer the mark. Still even if only 100,000 died of scurvy between 1500 and 1850, the medical profession were responsible for almost all these deaths (for, when good arguments are beaten from the field by bad ones, those who do the driving must bear the responsibility).

p. 162, line 6: Historians, far from holding doctors responsible for the one hundred thousand or so deaths from scurvy,

p. 163 line 11 from foot: He conducted various trials of therapies at Haslar, not only on methods for treating scurvy, but also on drugs to alleviate fever: he reported in his *Essay on Diseases Incidental to Europeans in Hot Climates* (1771), that he had 'conducted several comparative trials, in similar cases of patients.' But what he gave his readers were conventional case histories, and it seems that none of his later trials, despite all his efforts, produced significant results – presumably because he was always trying one ineffective remedy against another. Moreover his therapeutic practice…

p. 166, line 5: Lind's failure to press home the implications of his single trial, and his failure to repeat it successfully, mean that he actually deserves to be left in obscurity.

p. 167 line 8: This was the discovery of the placebo effect, though the word placebo first appears in English slightly later, in 1811.

p. 170 line 20: The first use of the placebo in clinical trials (a procedure which implies an understanding of the placebo effect) was apparently in Russia in 1832. There trials…

Revisions to further reading:
Add to "Introduction", p. 289: On the Oath, Howard Markel, "'I Swear By Apollo' – On Taking the Hippocratic Oath," *New England Journal of Medicine* 350 (2004), 2026-9.

p. 292, line 6: On the birth of controlled trials and of medical statistics, Kenneth Carpenter, *History of Scurvy* (Cambridge,1986); Stephen R. Bown, *Scurvy* (Chichester, 2003) – but see Nick Rodger, *The Wooden World* (London, 1986) and Glyn Williams, *The Prize of All the Oceans* (London, 1999) on deaths at sea; Andrea A. Rusnock….

p. 293, end: add as separate para: Cedric M. Smith, "Origin and Uses of *Primum Non Nocere*", *Journal of Clinical Pharmacology* 45 (2005), 371-8

POSTSCRIPT

On questions of method, Ian Hacking, *The Social Construction of What?* (Boston, 1999); my review of Paul Boghossian, *Fear of Knowledge* (Oxford, 2006) on www.socialaffairsunit.org; and my reply to Shapin, *London Review of Books*, 14 Dec. 2006. On Hughes Bennett, John Harley Warner, "Therapeutic Explanation and the Edinburgh Bloodletting Controversy: Two Perspectives on the Medical Meaning of Science in the Mid-Nineteenth Century," *Medical History* 24 (1980), 241-58 and L. S. Jacyna, "'A Host of Experienced Micropscopists': the Establishment of Histology in Nineteenth Century Edinburgh," *Bulletin of the History of Medicine* 75 (2001), 225-53. Eugeniusz Kucharz, "The life and achivements of Joseph Dietl," *Clio Medica* 16 (1981),

25-35. Michael Hunter, “Boyle versus the Galenists,” *Medical History* 41 (1997), 322-61. Milton Wainwright and Harold T. Swan, “C. G. Paine and the earliest surviving clinical records of penicillin therapy,” *Medical History* 30 (1986), 42-56. Sheila Ryan Johansson, *Death and the Doctors: Medicine and Elite Mortality in Britain from 1500 to* 1800 (Cambridge, 1999).

Minor Corrections to text:

the em-dash fault:

p. 156, first full para, line 12

p. 146, second full para, line 3

p. 73, seven lines from foot

p. 76, seven lines from foot

p. 189, line 2

p. 178, end of first full para

p. xv, Table 2: “data” not “date”

p. 13, line 3: should read "ophthalmia".

p. 33, 2nd line from bottom: should read "enteric fever".

p. 166, 6 from foot: Elisha Perkins (1741–99)

p. 198, last para: “Moor Monkton near Yorkshire” – this is obviously the result of some half-complete correction. It should be “Moor Monkton near York”

p. 284, line 7: the germ in silk worms is 1835 (the date is correct on p. 129).

p. 303, the index reference to tooth decay is a ghost – the reference disappeared in proof.