

Correspondence

Power and Dependence

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P. S. J. Spencer's review of my book (*J. Pharm. Pharmacol.*, July 1992, 625–626) reaches conclusions which set it far apart from every other opinion and review I know of, with the predictable exception of a statement from the Association of the British Pharmaceutical Industry. I wondered if he has any personal or professional interests to declare.

Three examples were given to support Spencer's view that *Power & Dependence* is a work of "low scholarship and journalistic excesses". First, he complained that I had written that "by the end of the 1980s, the best estimates suggested that perhaps half a million people were more or less addicted to benzodiazepines". But Spencer failed to say in his review that the estimate I gave was cited to two sources; he also quoted it completely out of context. Immediately preceding this passage, I noted the estimate by the Committee on Review of Medicines (1980) that 28 persons had become dependent on benzodiazepines between 1963 and 1979—and its endorsement of the estimate from Marks, *The Benzodiazepines. Use, Overuse, Misuse, Abuse*. MTP Press 1978, p. 45 of only one case of dependence per 5–50 million patient-months of use.

Presumably, Spencer would accept that the higher estimates I cited are better than those lower ones, but he specifically criticized my suggesting they were the "best". They are certainly not the highest (*Br. Med. J.*, 26 January 1991, 200), but if Spencer has better evidence he should produce it. He also rejected my using the word "perhaps" to qualify the figure of 500 000; I did so to indicate that this might be regarded as a ceiling figure, and only an approximation. Similarly, he objected to my saying people became "more or less" addicted to benzodiazepines—which was to indicate that different users may not experience symptoms of addiction/dependence of similar intensity and type.

In the second example, Spencer took me to task for writing: "One cannot be precise, but perhaps a third of all drugs are now

prescribed for their placebo effect." He asked: "on what evidence is that conclusion made... equally of course what evidence is there to say that the Author is wrong!". The figure I mentioned came from a report in *World Medicine* (13 December 1980, 34–35), also cited in a *Br. Med. J.* leader (23 May 1970, 437). But why make such heavy weather of the general point that doctors (knowingly or otherwise) often prescribe drugs to elicit a placebo effect?

The third example appears confused in that it equates the work of the Dunlop Committee with self-regulation by the industry (and also confuses the words "imply" and "infer"). I think it would have been clear from the reported conclusions of the Sainsbury Committee that, however good it might have been, self-regulation was not nearly good enough. There seemed to me no particular reason to discuss the work of Dunlop's Committee on the Safety of Drugs. Whatever it achieved through the cooperation of industry and otherwise, it was essentially a stop-gap measure. At the time, Dunlop himself noted that the Medicines Act "seeks to encompass far more than the present limited objectives of the Committee" (*Ann. Intern. Med.*, August 1969, 237–244).

Given his lofty views on the conduct of pharmaceutical medicine and journalism, and his resolute rejection of exaggeration and the like, I would have expected very much better evidence than any of these examples provide—e.g. for the allegation that my book contains "many exaggerations or distortions which litter almost every page".

In what I thought was a breathtaking claim to objectivity, Spencer also asserted that his colleagues would share his views: "If you are a pharmaceutical scientist or member of a health profession . . . you will find on most pages statements or assertions which you will believe to be untrue, distorted or simple exaggerations". In the light of the examples Spencer gave, I suspect many would conclude that the intellectual hygiene problem might be his rather than mine—but I am proposing to test this hypothesis by offering a full refund on *Power and Dependence* to any reader of the *Journal* who feels that it merits Spencer's description of it. I would not expect to be troubled by more than the odd vexatious claim.

Power and Dependence—a reply

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Charles Medawar has done what he accused me of doing, namely focused his remarks on particular comments in my Review rather than consider it as a whole. I totally accept that my Review was more critical than complimentary, not so much for what he had to say but more for the manner of his presentation. Furthermore, he should not criticize my taking his statements or conclusions at face value, when he now does exactly that with mine!

I invite readers to consult my review again (*J. Pharm. Pharmacol.*, July 1992, 625–626) and, as Medawar suggests, decide whose intellectual hygiene (his phrase) is in doubt. Of course, I urge readers to take up Medawar's very generous offer of a free copy of his book which, for all its faults, I still believe is worth reading.

As to whether I have any personal interests to declare: I worked in the industry at the time of the thalidomide disaster, and Medawar will never understand the frank horror, disbelief and responsibility felt by many thousands of experimental scientists at that time. I was thus immediately struck by the false picture painted by this book.