

Comments and Critique

Consensus Meetings

Who Needs Consensus?

"The agreement of experts has been a traditional source of all the errors throughout medical history" (Alvan Feinstein) [1].

SINCE the establishment of the multimillion project of consensus conferences by the National Institutes of Health in 1977, the fashion of issuing *ex cathedra* statements on matters of medical faith by *ad hoc* committees of selected, and often self-appointed, experts has spread throughout Europe. Hardly a week passes without the announcement of some new consensus by an anonymous group of experts, whether it is global warming, the spread of satanism or the upper limit of "normal" cholesterol.

The need for consensus in medicine, as in religion, stems from the fact that medicine, not being science, must close ranks in order to cover up its doubts, uncertainties and ignorance. The fear is that by being frank about the arbitrariness of dogma, the discipline may forfeit its superhuman status.

Traditionally, doctors were expected to treat patients *lege artis*, that is to say, as art and authority prescribe, even if such treatment was useless or even harmful. Many readers will still remember the orthodox consensus on the correct management of myocardial infarction—6 weeks of absolute bed-rest. What is new about the current consensus craze is the bureaucratic process by which it is achieved, providing employment to whole institutions, and the involvement of the media, which ensures that the received truths are spread to the farthest corners of the land.

The absence of consensus may be embarrassing. In his *Preface on Doctors*, Bernard Shaw recalled how during an influenza epidemic in London towards the end of the 19th century, an evening paper sent a journalist, who posed as a patient, to leading consultants in London and then published an account of the advice and prescriptions: "The case was the same: but the prescriptions were different, and so was the advice". This despicable act outraged the medical profession who roundly denounced it "as a breach of confidence of those eminent physicians". More recently, in 1986, at a London consensus conference on breast cancer, a researcher presented the results of a survey of Scottish and Australian doctors who were asked how they would manage 2 hypothetical patients with breast cancer, whose clinical histories were supplied. For the patient with a worse prognosis, 62 doctors advised 36 different plans of treatment; for the patient with a better prognosis, 42 different approaches were offered [2]. As might be expected a neat consensus was then reached: the participants of the consensus conference agreed to agree that the incurability of breast cancer (evidenced by the plethora of "treatments" available and by the

passion with which they had been defended or denigrated) should not be made known to the public.

Medical consensus is, for obvious reasons, avidly sought in all areas of medical practice: diagnosis, screening, treatment. When the American Psychiatric Association decided in 1973 to declassify homosexuality and to get a consensus (which they did) that their previous consensus on homosexuality as a psychiatric disorder was incorrect, one psychiatrist observed: "It is indeed saddening to see the American Psychiatric Association functioning more as a church council deciding matters of dogma and philosophical speculation than as a professional scientific organisation".

I had the opportunity, as an invited token critic, to observe the proceedings by which the Forrest committee reached their consensus on breast cancer screening. Shielded by secrecy, the committee rushed to a foregone conclusion, without even waiting for the results of their own randomised controlled trial. Their consensus recommendation for a national screening programme was based on selective evidence and all areas of uncertainty were papered over with a transparent tissue of wishful thinking [3]. Dismantling this enterprise will now take years.

When the results of the Milan trial of breast cancer treatment with combined cytotoxic therapy were first announced in the *New England Journal of Medicine* in 1976, the accompanying editorial hailed this "major advance" and the "dramatic effects" as "nothing short of spectacular", and calculated "how many hundreds of thousands of lives can be improved, or indeed saved, by application of the present information in the coming decade" [4]. (Yet the Milan group presented their modest results with appropriate caution and warned against overinterpretation.) This enthusiasm quickly infected the profession and one correspondent who attended two large symposia on breast cancer in 1976 spoke of the jubilant atmosphere of immediate triumph. A few years later, reviewers wrote about the same treatment as "frankly disappointing", and "giving rise to a degree of pessimism". Obviously, the dissenters had to be silenced and the time for a consensus was ripe. In 1985, a consensus conference declared that "for premenopausal women with positive nodes, regardless of hormonal status, treatment with established combination chemotherapy should become standard care" [5]. Nobody seemed to notice the contradiction between the professed aim of cytotoxic therapy, mentioned in the preamble of the consensus document, "the rationale is to eradicate occult metastatic disease that otherwise would be fatal", and the lack of effect of such therapy in premenopausal and postmenopausal women with negative nodes, which had been freely acknowledged in the same statement. Subsequently, the West Midland Oncology Association published results of their trial which set out to confirm the Milan claim. Their conclusion was that "neither overall survival nor survival in [the subgroups of premenopausal or postmenopausal patients] was

significantly influenced by treatment" [6]. Dr E.J. Maher from the Mount Vernon Hospital, Northwood, commented: "This is a depressing paper. Just when the last sceptics were beginning to come round to the necessity of adjuvant chemotherapy for node-positive premenopausal women with breast cancer, we read this paper" [7]. End of consensus? Unlikely. There would seem to be a need for another consensus conference!

As meta-analysis is increasingly used at consensus conferences, and invited meta-analysts conjure metaphysical "statistical significance" where no clinical significance exists, like the alchemists of old converting dross into false gold, the time will soon come for metaconsensus of consensus. This could be applied with profit to differing consensus on breast self-examination and mammography, issued by the National Institutes of Health, the American College of Radiology, the American Cancer Society, the American College of Obstetricians and Gynaecologists, the Council on Scientific Affairs of the American Medical Association and their counterparts in other countries.

Cancer treatment breakthroughs are now appearing with such a rapidity that time could be wasted by organising a consensus conference, and the National Cancer Institute circulates instructions in the form of "an alert" to 13 000 oncologists, long before such information reaches the pages of peer-reviewed medical journals.

The intensity of consensus activity is directly proportional to the paucity of data and to anticipated profits. For this reason the largest number of consensus conferences held so far on any single issue has been concerned with cholesterol. I am aware of 22 different cholesterol consensus recommendations, and I do not specialise in collecting them. Many names appear repeatedly on these panels, and their arbitrary recommendations are as predictable as they are meaningless. Those who can see through the mists of obfuscation, wishful thinking and power games of these committees are not as a rule the experts but investigative journalists (see for example well-researched cover stories on cholesterol mythology in the *Atlantic Monthly* of September 1989 or *Der Spiegel* of November 5, 1990).

Consensus bandwagons have certain common features, which include technological optimism, single-cause myopia, suppression of dissent and the urge to replace uncertainty with officially approved activism. Token dissenters are occasionally invited in order to create an appearance of objectivity. Yet if a consensus was as good as the proportion of consenting members on the panel indicates, there would be no need for the conference in the first place. If a genuine disagreement exists, experts should be encouraged to meet and identify clearly the areas where evidence is weak or non-existent, so that useless procedures can be abandoned and experiments planned for resolving the confusion. Such meetings, however, would not be called "consensus" conferences, but ordinary professional meetings and debates. This is what happens in science. The last thing which entered any physicist's mind when rumours spread that the impossible dream of limitless energy generated by cold fusion had become a reality was to call a consensus conference on cold fusion. Instead a consensus was reached quietly that the notion was irreproducible nonsense, based on attempts of experimental replication.

It was encouraging to see a report of a meeting on the value of remission-inducing drugs in rheumatoid arthritis [8]. The

meeting was called because the use of these drugs is of uncertain benefit, despite loud claims to the contrary, and the conclusion of the debate was that "no definite decision was reached as to whether the beneficial effects of the drugs outweigh their costs in terms of toxicity and patient and NHS resources". Perhaps because the discipline of rheumatology has never used the military rhetoric of cancer fighters and surgeons-general, and because rheumatologists are not in the business of "saving hundreds of thousands of lives" but modestly try to alleviate suffering of their patients, their critical faculties are more developed than in their colleagues who stand in the frontline against the big C.

In science, controversy is cherished as it spells progress, points out to new laws and leads to new hypotheses and their testing. In medicine, controversy is deplored ("at least not in front of the children", that is, the public) and if possible stifled by consensus. An unfortunate consequence of this ostrich strategy is postponement of clear formulation of the problem and progress towards its solution.

Alvan Feinstein, writing about fraud, distortion and delusion in medicine, described two forms of the consensus syndrome. In the milder form, "Disputes are resolved not by the scientific process of identifying and repairing methodological problems, but by a form of political process: the majority vote of a panel of selected authorities. In the more severe form of the syndrome, the process extends from politics to religion" [1].

To end on a positive note, I would propose a moratorium on consensus conferences and a return to the old custom of proper scientific meetings. Those in the public health service, tired of investigating the association between coffee-drinking and snoring, might direct their abilities to the assessment of the cost-effectiveness of past consensus conferences, their impact on medical practice and on the general wellbeing of the population. When facts become convincing enough, consensus emerges spontaneously as a byproduct.

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