HealthWatch

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Contents

£1.3m for CAM research but Lords' advice "ignored"

THE GOVERNMENT is to fund a group of research projects into complementary and alternative medicine chosen with apparent disregard of the recommendations of the House of Lords investigation which had asked for the funds to be made available in the first place. The projects chosen for funding may do little to advance our knowledge of the safety and efficacy of complementary and alternative medicines, according to leading scientists writing in Focus on Alternative and Complementary Therapies (FACT), the journal published by Exeter University's Complementary Medicine Research Group.

The 6th Report of the Select Committee on Science and Technology, published in November 2000 and entitled Complementary and Alternative Medicine, set out as priorities a number of questions that research should aim to answer, namely: "Does the treatment offer therapeutic benefits greater than placebo?", "Is the treatment safe?" and, "How does it compare, in medical outcome and cost-effectiveness, with other forms of treatment?"

The projects that the Department of Health have selected to receive shares of the £1.3m total fund comprise three qualitative studies into the use of CAM in cancer, including a study on male cancer patients' views on and use of CAM; and a set of studies aimed at research capacity building within CAM and which includes a study of homoeopathic doctors' clinical decision-making processes.

Of the experts invited by FACT to comment on the news—including Jos Kleijnen and Paul Wilson of York University's Centre for Reviews and Dissemination, Sheila Glenn (a Director of Research at Liverpool John Moores University), David Colquhoun, Professor of Pharmacology at University College London, and HealthWatch's chairman Professor John Garrow—all expressed surprise at the choice of projects which did not appear to address the Lords' report's brief. John Garrow, who had himself offered evidence to the House of Lords Select Committee, writes that the report, "took evidence from a very wide range of special interest groups, including 55 oral hearings…[it] ran to 141 pages with nine chapters and nine appendices." He comments, "Evidently the government felt free to ignore this advice…it seems that the issues of efficacy/safety/value are not central to any of the five products in the second group. I am concerned that it now seems this money will not be used to answer the questions so clearly given priority by the HSLC."

Focus on Alternative and Complementary Therapies
Dec 2003; 8 (4)

Top of page

Congratulations and controversy at AGM 2003

Three influential new personalities welcomed onto the committee, prizes to some of the coming generation of outstanding medical students, and possibly the most controversial awardwinner's address that HealthWatch members have enjoyed to date, helped to make the fifteenth HealthWatch Annual General Meeting a memorable one.

On 28th October 2003, some 45 members and guests gathered at the Medical Society of London. Treasurer John Hanford began with the news that current membership is around 200 and subscription income alone cannot maintain our expenditure, the shortfall being met by donations from members. However, members were cheered to learn from Dr David Bender that the HealthWatch website receives some 1,000 hits/month, indicating it is of significant interest and influence. The review of the year, by chairman John Garrow, can be found below.

In the annual elections, the committee had pleasure in welcoming three eminent new members from the fields of science, media and medicine:

Heinz Wolff , best known for television series such as Young Scientist of the Year and the Great Egg Race, is also a distinguished scientist and a pioneer in the field of bioengineering. The Emeritus Professor of Bioengineering at Brunel University has recently been appointed Director of the Huntleigh Research Institute, created to develop, amongst other things, the technology which will enable elderly people to live independently in their own homes for longer.

Medical journalist and award-winning author **John Illman** is a former editor of General Practioner who spent five years as medical correspondent on the Daily Mail, eight years on The Guardian as health editor, then three years as The Observer's medical correspondent.

Dr Gillian Robinson is an Associate Specialist in Sexual and Reproductive Health, currently at St Giles Hospital in London where she works in women's health in the community.

Presentation of the HealthWatch awards for the year began with the AJAHMA Student Prize. This is given to healthcare students for their skill in spotting the hidden defects in clinical trial protocols; it is a principle of HealthWatch that students must develop these skills if they are later to critically analyse advice and promotional pressure regarding medicine use. This year's winners are all medical students. Joint first prize winner Katerina Denediou (UCL) was present and will share the £1000 first prize with Shirley Moore (Aberdeen). Runners up, receiving £100 each, are Simon Harrison (Bristol), Kathryn Musgrave (Cambridge Pembroke), Danny Lim (London Imperial) and Mareeni Raymond (UCL).

Good wishes were expressed towards recently resigned committee member Caroline Richmond, who is currently gravely ill in hospital. One of HealthWatch's founder members, Ms Richmond is a medical journalist and we hope we shall read more of her incisive views in this newsletter in due course.

Finally, Nick Ross presented the 2003 HealthWatch Award to Dr Peter Wilmshurst whose controversial address, "Obstacles to Honesty in Medical Research", appears below.

Top of page

News in brief

The Observer's alternative healing columnist, the Barefoot Doctor (real name Stephen Russell, see HealthWatch Newsletter issue 45 for background), came under fire from web site users when he went online on 14th October to answer questions on "wellbeing, alternative therapies and medicines and ways to cope with modern life". Hostile fire began with the very first enquiry and continued, leaving genuine questions to the "doctor" outnumbered more than seven to one by criticism and sarcasm. Comments on "superstitious nonsense" and complaints about the appropriateness of Russell's use of the term "Doctor" appeared frequently. One writer responded to Russell's comment, "…implicit in my writing that all my advice is intended to complement conventional treatment" with, "Wouldn't it be a lot safer for your readers if you made it explicit?"

http://talk.guardian.co.uk/WebX?128@84.TQezbsvV1ms.0@.685e9480

AN ALTERNATIVE healer who claimed that he could cure cancer may face jail after being convicted of two offences under the Trades Descriptions Act, according to The Times. Reginald Gill, a "wellness practitioner" of Poole in Dorset sold terminally ill cancer patient Stephen Hall an electronic device that he said would reverse the illness. Hall, apparently on Gill's instructions, stopped taking his morphine and spent the last weeks of his life trying to stick to a bizarre eating plan. Bournemouth Crown Court was told that Gill charged Hall £2,500 for an IFAS High Frequency Therapy device, intended for treatment of relatively mild conditions such as insomnia and hair loss and bought for £200, claiming that it could kill off cancer cells. The case resulted from a complaint to local Trading Standards' Officers by Hall's mother. Sentencing is due to take place in January 2004.

The Times, December 13, 2003

Top of page

HealthWatch Award winner: Obstacles to honesty in medical research

Dr Peter Wilmshurst, a consultant cardiologist, has spent the last two decades trying to expose research misconduct and has reported more than twenty doctors to the General Medical Council. In recognition of his dogged and selfless pursuit of the truth, Dr Wilmshurst was presented with the HealthWatch Award 2003.

I feel greatly honoured to receive the Health Watch Annual Award and I am grateful for the opportunity to speak to you about obstacles to honesty in medical research.

I have been interested in this subject for 20 years, since I first experienced research misconduct when I was a research registrar. I hope that a personal account of my experiences may explain why I believe this is a serious problem.

In 1986 I went to the Guardian Newspaper with the story after the medical and pharmaceutical regulators

refused to take any action.[1] I supplied the Guardian's lawyers with over 200 pages of documents and statements, which convinced them that they could successfully defend any legal action if sued. We were not.

My research was on heart failure. This is a common condition and it has a worse 5-year survival than many forms of cancer. Twenty years ago there were few treatment options to improve symptoms and none was proven to improve survival. I was offered the opportunity to do research on a promising new drug, named amrinone. It was patented by Sterling-Winthrop. Preliminary research looked promising. Research, mainly from the company, showed that the drug increased the strength of contraction of the heart in animals. But the most influential article and the one that persuaded me that the drug was worthy of research was on patients and was published in the New England Journal of Medicine in 1978.[2]

The New England Journal is the most influential medical journal in the world. The article came from the Cardiology Department at Harvard and one of its five authors was the most well known cardiologist in the world and head of medicine at Harvard, Professor Eugene Braunwald. The paper was given extra prominence by being the first article in that issue of the Journal and it was accompanied by an editorial.

In a large series of experiments we showed that, although amrinone increased the strength of contraction of normal heart muscle, it did not affect contractility in patients with heart failure. We also found that amrinone frequently caused life threatening side effects.

With hindsight there were two things that should have raised my concerns when we started our research. The first were anomalies in the study from Braunwald's group. It was a small study, which made claims that were not substantiated by the observations reported.

Later I discovered that though the article stated that the 5 authors were employed in the Cardiology Department at Harvard Medical School, 2 were full-time employees of Sterling-Winthrop and had never worked at Harvard.[3] Two of the three that worked at Harvard were paid consultants to the company.[3] These conflicts of interest were not declared.[3] In fact the New England Journal of Medicine had no policy on declaration of conflicts of interest at the time. The first statement on conflicts of interest was published in the New England Journal one month after I wrote to the Massachusetts Medical Society, which owns the Journal, complaining about the undeclared conflicts of interest in this case.[3,4]

The second thing that should have alerted me was a letter published in the New England Journal of Medicine from cardiologists in Los Angeles.[5] The letter reported fatal side effects from amrinone. The first author, Dr Stanley Rubin, had a patient with severe heart failure. The patient's wife was a stock-broker. She saw the dramatic increase in the price of Sterling-Winthrop shares after the paper from Braunwald's group was published. She reasoned that this proved that amrinone was an important advance. She asked Dr Rubin to get amrinone for her husband. Rubin was able to persuade the company to let him have amrinone on a named-patient basis and the amrinone swiftly killed his patient. Rubin and colleagues sent the New England Journal the first report of side effects with amrinone. They did not tell Sterling-Winthrop that they had submitted the report. Within 48 hours Rubin was under pressure by the company to retract the report. The Journal admitted that it had sent Sterling-Winthrop a copy of Rubin's report. The Journal initially refused to publish the report but was forced to do so when Rubin said that if they did not he would go to the press.[3,6,7]

However the conflicts of interest involving the New England Journal, the Cardiologists at Harvard and Sterling-Winthrop did not end there. The company later produced a congener of amrinone, named milrinone. The initial human research on milrinone was also performed in Braunwald's department.[8] Unusually it was agreed before the research had been completed that it would be published in the New England Journal. When the first 2 referees chosen by the journal to review the paper recommended rejection, the editor, Dr Arnold Relman sent the article to 2 more referees. They also recommended rejection, but the Journal published the paper on milrinone as previously agreed.[3,6,7] This says much about peer review in the World's most prestigious medical journal.

I discovered this much later. In the early days of our research my colleagues and I were more concerned that we could not confirm in our large number of experiments claims made in the small study from Braunwald's department.

We reported to Sterling-Winthrop that we were unable to find evidence that amrinone injections increased contractility in patients with heart failure and we reported our experience of serious adverse effects with the oral preparation of the drug. Company employees asked us to exclude some patients from the analysis. These were ones where there was a downward trend in contractility. The effect of excluding them would have been to produce an apparent but spurious increase in contractility in the remainder. We refused. My supervisor and I were then threatened with litigation.[1] We published.

Our on-going research studies on amrinone ended when company employees removed the drug stocks from the pharmacy in the hospital and research institute.[1] As a result, 2 of our publications contain statements pointing out that the studies were smaller than planned because Sterling-Winthrop had prematurely discontinued our trials without our agreement.[9,10]

A number of tactics were used to try to prevent my colleagues and I presenting our findings at meetings and to discredit us when we did present.[1] One strange incident involved one of my colleagues, Alex Crowther, who

was due to present some of our work on amrinone on the second day of a meeting in Luxembourg. He just managed to get on the last flight of the day that would permit him to attend the first session of the meeting. When he arrived he discovered that his talk had been rescheduled for the previous day. The organisers had received a forged letter that appeared to be from him asking for his talk to be brought forward a day. Those responsible were never identified.

When I presented our findings on side effects a company employee stood up and said that I had made up the findings.[1] I had to point out that I was an independent investigator, but that my accuser was a company employee. I had nothing to gain by claiming that the drug was unsafe. I asked the chairman to appoint people to review our data. A few days after the meeting I received an apology from the company, but the hundreds who heard the allegations at the meeting would not be aware of the company's retraction.[1]

At a number of other meetings at which I presented our findings, three eminent professors of cardiology, each of who was a paid consultant to Sterling-Winthrop, made public statements that they had tried to replicate our findings and failed. None of them acknowledged their affiliation to the company. Twenty years later none of those failures to replicate has been published. This tactic came to an end at a European Congress of Cardiology, in front of several hundred doctors. I pointed out that a professor who made these claims was a paid consultant to the company and that he had been making the claims for two years. I suggested that if he continued to make the claims without publishing his data people might think that he was lying. My findings were not challenged again.

At one point, my supervisor and I were asked to meet with the company and a different American professor of cardiology who is an opinion leader in the treatment of heart failure and who was a consultant to the company. The American professor told us that we were mistaken about the drug. He said that he was aware of finding by other investigators and that these entirely refuted ours. He advised us that we should not publish any more of our findings. He said that we would be found to be wrong and our reputations would be adversely affected. We went on to present 14 abstracts, and 15 publications.

One of the presentations was at the American Heart Association meeting in November 1982. I presented data, which showed that amrinone did not have the cardiac effects claimed. After my presentation, 3 professors of cardiology at separate American university hospitals told me that they had also obtained results similar to ours. They were unaware of each other's research or of our research. They informed Sterling-Winthrop. The company arranged meetings between each of them individually and the same professor of cardiology, who had told us that our findings were aberrant. He also told each of them the same thing. He persuaded two of them not to publish. The third did publish, after much soul searching because he was afraid that he would lose research contracts with Sterling-Winthrop and other pharmaceutical companies. After he published he received threats, including a threatening phone call at 2am.

The Netherlands Committee for the Evaluation of Medicines spotted our paper on the side effects of amrinone.[11] There were major discrepancies when compared with the clinical record cards submitted by the company on our patients. We showed that the company had sent the Netherlands Committee forged clinical records for our patients with the information on adverse events deleted.

Because of this I contacted the UK Committee on Safety of Medicines and discovered that Sterling-Winthrop had also failed to notify the CSM of side effects in our patients.[1] During discussions I discovered that contrary to statements made to us at the outset of our research, Sterling-Winthrop had not obtained a Clinical Trials Certificate for oral amrinone, though they had got a CTC for amrinone injection.[1] This meant that the research with oral amrinone conducted by us as well as by doctors in the National Heart Hospital in London, in Newcastle-upon-Tyne and in Birmingham had been illegal.

When I raised this with the company, the senior vice president bragged that they were telling the government that if the company was prosecuted it would close down its large manufacturing plant near Newcastle upon Tyne. The company was not prosecuted for breaches of the Medicines Act.[1]

I tried unsuccessfully to get sanctions against the company or its employees, but the Association of the British Pharmaceutical Industry, the Faculty of Pharmaceutical Medicine of the Royal College of Physicians and the General Medical Council were not interested.[1] I spoke to editors of medical journals, including BMJ, Lancet and Nature. None disputed the facts but all were afraid to take on a multinational pharmaceutical company with unlimited financial and legal resources. One editor mentioned the loss of advertising revenue from the company.

The process of being rejected by all the official bodies that I believe should have dealt with the issues took nearly 5 years. While this was going on, in 1984, the company told a hearing of the Food and Drugs Administration in the USA that there had been over 1400 serious adverse events in 1200 patients given amrinone in trials and the company announced that they would cease trials and applications for product licences worldwide. Officially the drug was unsafe to take even on a doctor's prescription. Two years later, in 1986, I discovered that the company was still marketing amrinone in parts of Africa and Asia.[1] In those countries it was being sold as an over the counter treatment for heart failure. I approached Oxfam, which had workers in the developing countries where this was happening.[1] They collected evidence, which was presented at a meeting of the World Health Association in Geneva. Sterling-Winthrop was finally embarrassed into withdrawing the drug world wide in 1986.[1]

It was my contact at Oxfam who put me in touch with James Erlichman, a Guardian reporter. He and the deputy editor, Peter Preston, were convinced by the evidence I had and so were the Guardian's lawyers. The paper covered the story on the front, back and the whole of an inside page of one issue and in follow-up stories in other issues.[1]

I had seen how corporate greed and personal ambition had tended to distort scientific evidence. Sterling-Winthrop believed that my supervisor and I could be bribed or threatened into suppressing our data. Others, such a Drummond Rennie, Deputy Editor of the Journal of the American Medical Association, have documented this occurrence.[12] Some professors preferred to suppress their findings rather than run the risk of losing prestige by appearing mistaken or losing lucrative contracts for future pharmaceutical research. Financial conflicts of interest caused some opinion leaders to behave dishonestly. Conflicts of interest, affected publication decisions at the New England Journal of Medicine. The institutions including government, which one might expect to help preserve research integrity, were not prepared to take on a multinational pharmaceutical company.

However these are not the only obstacles to honesty in medical research I have come across.[13] In one case an eminent clinician, who was the president of his specialist society, and who had a large private practice doing a particular interventional procedure wished to publish a series of 400 cases. It was then the largest series in the United Kingdom. When the data was analysed it was found that his mortality rate for the procedure was unacceptably high compared with rates in other countries. If this became known it would have a disastrous impact on his private practice. So the mortality rate was falsified. However, they had already published an abstract at an obscure meeting at which amongst other things they reported the deaths in the first 254 patients. The number of deaths reported in the abstract was greater than in the 400 reported in the paper. This discrepancy became common knowledge in the specialty. I was present during a meal at which a junior doctor that was a co-author of the paper admitted that the falsification had occurred. He implied that he and other junior doctors had little option but to go along with their boss. Five other junior doctors heard the admission. I contacted the editor of the journal. It was part owned by the specialist society of which the senior author of the paper was the president. The editor knew of the rumours. He said that if I could get one of those who heard the incriminating admission to confirm it, he would act. I went back to those who had heard the admission. Now, years after those events, some have provided me with written statements confirming that they heard the admission, but at the time all said that they would not support my efforts to get the paper retracted. Some said that it would be bad for their careers. Some said that it would be bad for medicine or the specialty. One said that he thought that it was the sort of thing that any of us would do. Those 5 junior doctors went on to get consultant posts and one went on to be a president of the society himself.

My efforts to get the paper retracted were common knowledge in the specialty. I was asked to see the post-graduate dean who advised me to stop upsetting influential people. Until that point things had gone well in my career. As an undergraduate, I had obtained honours or distinction in 10 out of 11 subjects. I had been awarded an Honours degree overall, plus six undergraduate prizes and an Intercalated B.Sc. My house jobs were in my teaching hospital, and included the professorial medical job. Then I was senior house officer at the Hammersmith and in Oxford, medical registrar at Northwick Park, and cardiac registrar and senior registrar at St Thomas'. After these events, for the first time in my career, I had difficulty getting a job. I stopped counting the rejections after the 42nd. In many cases individuals with much less clinical and research experience were appointed. It was clear to me that loyalty, no matter how misplaced, was valued more highly in medicine than honesty.

I believe that obstacles to honesty in medical research generally fall into a few categories. One is personal ambition for promotion, advancement, money, kudos and power.

A second obstacle is that those who achieve success by becoming heads of departments or institutions can only maintain their position if their institution continues to succeed. Success is judged in many ways, but the most common measure of success is the balance sheet. Department heads are expected to pull in research grants. So money is another obstacle to honesty in research. This does not apply purely to pharmaceutical companies. I do not imagine that executives of Elsevier, which owns the Lancet, asks the editors much about the research published. I imagine that Elsevier asks how much was earned from drug advertising, how much was earned from sales to pharmaceutical companies of reprints of trials showing their drugs in a positive light and how the current citation rating will affect circulation profits. Of course academic institutions are the most mercenary of all.

However the greatest obstacle to honesty in medical research is the code of silence that pervades the medical profession and the research establishment. There is still considerable reluctance to shop another doctor, no matter how dishonest he is. In this setting of tolerance is there any wonder that ambitious young doctors, aware that to progress they need lots of publications with exciting findings, will embellish their findings and some will falsify the lot? Should we be surprised that a search for funding for their department and personal gain, from drug company consultancies, result in dishonest behaviour by senior academics and opinion leaders? Who will blow the whistle on them? Institutions seeking high rating in the research assessment exercise will try to suppress knowledge of dishonesty in their establishments, even to the extent of letting the guilty escape punishment. Those institutions demand success from their department heads and do not look too carefully at whether that success was achieved honourably or honestly. In this setting it is almost invariable that whistle blowers are damaged more than the guilty they expose. Academic institutions and journals do not want to be associated with dishonest research and treat harshly anybody that brings it to attention.

I have, with difficulty, persuaded a few journals to publish a small number of articles describing research misconduct.[3,13,14] Each article has been reviewed sentence by sentence by lawyers wanting evidence to support individual statements. This was because the editors of the journals were concerned that they might by sued if individuals or institutions were libelled. In a libel case it is no defence to say I am only the publisher not the author. This is in stark contrast to scientific publications. I have submitted many scientific articles for publication and many had implications for survival of patients, but no journal has ever asked me to prove that I got the results claimed. This might suggest that medical journal editors are more concerned with the reputations of academics and their institutions than the lives of patients. The simple truth is that editors are most concerned with money. Journals are never sued for publishing false results no matter how many patients died. In scientific research they can have the best of both worlds. They are absolved from blame if a study is wrong and gain an improved impact rating if the research is an important advance. A higher impact rating increases revenue from sales and advertising. Editors know that research can bring major reward to individuals and organisations, which may act as a temptation for dishonesty, but journals accept submissions on trust without checking their accuracy. Journals almost never retract work shown to be false. When they do, they make it clear that publication of the false research was entirely the fault of the authors. I would like to see whether the policies at journals changed if some were sued by patients harmed by implementation of treatments based on their publications.

There are few objective medical scientists, because they all know that success in their career is dependent on the results they obtain. Every one has a conflict of interest, everyone is human and some are venal.

Do academic institutions or journals recognise the humanity and venality of their staff? They do in some areas of activity. When paying wages, do any of these organisations leave out a bag of money and trust their staff to take the wages to which they are entitled? Of course they don't, because they realise that for some the temptation for dishonesty would be too great. The gains from dishonesty in research can be greater but institutions and journals trust researchers not to fall prey to these. We need to put in place robust checks on research. I believe that there should be random checks of raw data of work in progress and of submitted work. We know that use of performance enhancing drugs is common in competitive sports because of enforced drug checks without warning at sporting events and between events. If we did not have these checks we might mistakenly conclude that doping was not common in sport. I believe that the checks reduce the dishonesty in sport. We need a similar approach to research. The raw data could be demanded at a routine check during a visit to the research institution or when the research is submitted for publication. Failure to produce the raw data should be considered the equivalent of failing the inspection and should result in a ban on future research for a specified period and a review of previous research published. A finding that a department in an institution had falsified research should be a negative factor when assigning ratings in the research assessment exercise. In this setting justified whistle-blowing would be welcomed by institutions. Publication of dishonest research by a journal should affect its impact rating. The failure of a journal to publish a retraction of dishonest research should have a multiplied negative effect on the journals rating.

However the most important thing is that we must change the culture in medicine in which research success is viewed as the passport to success in ones career. For most clinicians only a limited experience of research is required to enable you to understand what you read in research articles and to participate in multicentre trials, organised by career medical scientists.

However there is a more fundamental problem, which is the issue of honesty. Most medical students start with high ideals. Research, which I hope is honest, has shown that as medical students go through medical school a progressively greater proportion believe that cheating in exams is acceptable. The institutions tolerate it. Three years ago Richard Smith wrote in the BMJ about a medical school that permitted a student caught cheating in the final exams to pass.[15] I know of examples where Universities have refused to withdraw higher research medical degrees that are known to contain falsified research. I know of an academic institution in London in which senior officers know that one of their professors lied about his qualifications when he was appointed to that institution.[16] Specifically he claimed to have a MD that he had not been awarded. The institution does not think he should be sacked and the GMC does not feel that he should appear before it. In that and other institutions there is tolerance of dishonesty at all levels. Only a sea change in opinion will produce the required improvement. I fear that it must be imposed from without because our leaders in medicine and academe lack the appetite to produce the required changes.

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Top of page

Evidence: Science vs emotion: The curious case of water fluoridation

A bid to block the fluoridation of public water supplies was defeated recently in a Commons' vote. Under a new amendment to the water bill, strategic health authorities now have the power to force water companies to fluoridate supplies after local consultation. MPs voted 284 to 181 on 11th November to back the measures, which proponents say will reduce levels of tooth decay. However the evidence for this is still very much in question. Earl Baldwin of Bewdley, vice chair of the All-Party Parliamentary Group Against Fluoridation, argues here that the scientific case for fluoridation is lamentably poor and that the need for hard evidence has been swept aside in the emotion of the debate.

Fluoridation is a curious issue. Books have been written about it, and there are thousands of papers in the literature. Probably more people receive it on a regular basis than any other medical drug [1]. Yet many of the public are apathetic, and are not even aware if their water supply is fluoridated. Like the proposed EU constitution it lacks glamour, yet it impinges on many lives. But at the professional level, in the words of the journal of the American Chemical Society it "has probably brought out as much extremism as any other issue in the modern history of science" [2].

Most reports and reviews in the English-speaking world have favoured it as an anti-caries measure, especially for children in deprived areas for whom good diets and oral hygiene are foreign territory. Opponents are often portrayed as flat-earthers who for irrational reasons obstruct an effective and, as Michael Baum described it in his recent article on screening [3], "innocent" intervention. What then is the controversy about?

It has a number of aspects, but first let me declare my hand. I oppose water fluoridation, though not necessarily other means of delivering fluoride, mainly on ethical grounds which I will mention later. So perhaps it is odd that I chose to fight it on the science when I started putting down Parliamentary Questions in 1997. This was because it seemed to offer a more clear-cut ground for argument, and because I suspected the work that had been relied on would turn out to be not nearly as good as generally supposed. When I was supported by Iain Chalmers, then of the UK Cochrane Centre, who reckoned I was probably wrong on the science but right to press the Government to display their wares with more transparency, we were granted the systematic scientific review of the evidence, the first in fifty years, which has come to be known as the York review from the NHS Centre for Reviews & Dissemination there which conducted it [4].

York was a Rolls-Royce affair, even among systematic reviews. Not only did it take nine months to examine fifty years of the world literature to the highest international standards against established criteria, focusing on primary sources, human studies and water fluoridation only, but it was conducted openly on the CRD website, and (crucially) overseen by an advisory panel drawn from opponents and proponents of fluoridation, as well as independent scientists and others. I served on this throughout the review, and can testify to the value of being able to challenge each others' biases at all stages of the review process. I have believed for some time that the scientific community needs to take more care with the biases that can creep in outside the areas of trial methodology, especially in such a high-octane area as this; and York was I believe unique in the fluoridation field, where the prevailing medical-scientific culture is strongly not to say passionately in favour, in ensuring that the agenda could not be dominated by either side.

There are times in medicine when received opinion suffers a jolt. Melissa Sweet, an Australian journalist, has written perceptively about professional reactions and damage limitation when some beliefs about hormone replacement therapy were overturned last year [5]. York provided a jolt of similar proportions. The review team said they were surprised to find no good evidence anywhere in the water fluoridation literature, and they catalogued a list of methodological failings among the relatively few studies which met the inclusion criteria, which made it impossible to say anything at all "with clear confidence". Most probable was a beneficial effect in caries reduction, possibly of some 15% (dentists used to claim 40–65%); the authors presented this as a suggestion, rather than a firm conclusion which is how it has generally been reported. Fluoridation probably has an effect over and above toothpaste, and appears to cause dental fluorosis (mottling) on a larger scale than was previously thought. On cancer, bone problems and a number of other possible harms, the evidence was particularly thin in quantity and quality, providing a mixed picture: it is not possible to say it is either safe or

unsafe. Weakest of all was the evidence about reducing inequalities in dental health among social groups, about which little could be said at all: this has been a special plank in the present Government's programme. York pointed up the need for further work in a number of areas, and stressed the importance of high-quality research if conclusions were to be reached.

This was far from the ringing endorsement the Government or professions had expected. York, after all, contradicted in significant ways virtually all previous major reviews and policy statements. The ensuing spin has caused astonishment and dismay among some of the independent scientists involved. Typical was the British Dental Association press release which told the public that fluoridation had been "proven beyond doubt to be safe and to massively reduce tooth decay". The BMA was hardly less gung-ho. In the three years since the York report the picture has not greatly changed. The big organisations (the British Fluoridation Society gets an annual Government grant of around £80,000) have continued to campaign strongly on the basis that fluoridation, being "safe and effective", must be extended. Sometimes they mention the need for more research, without addressing the apparent inconsistency in promoting a measure targeted at millions about which not enough is yet known; more often they do not.

Two years after York the Medical Research Council produced a list of recommendations for research [6], which took forward the York agenda in some helpful ways, notably over the need for "an estimate of the effects of water fluoridation" in key areas, and the need to look at total fluoride exposure from all sources. At the same time it second-guessed much of York's findings by introducing lower-level studies, to produce a picture more favourable to fluoridation. The MRC's was not a systematic review, so it was not possible to tell the criteria by which trials had been selected while others suggesting possible risks had been rejected, by a working party on which the dental view was heavily represented. The senior independent scientists on the York team and advisory committee [7] have meanwhile taken the unusual step of going public three times to remind people of the inconclusive quality of the current evidence; but their voices are not easily heard. We now await the recommendations to Government of the Chief Medical and Dental Officers.

So much for the science, after fifty years of fluoridation. One could have hoped that York would put an end to the selective quotation which has bedeviled this debate on both sides, but there is little sign of this. One point is worth stressing. We could all agree what the evidence tells us, and still be no nearer a policy decision. This is because no one is asking a key question: How good does the evidence have to be before we fluoridate? If hexafluorosilicic acid were comparable to, say, homoeopathic Arnica at a 30C potency, then one could say it had limited application, was no charge on the state, and any risks to health were implausible; randomised controlled trials would of course be nice, but were not a high priority. If it were a vitamin or essential nutrient, one would want to look at dosage, but there is no requirement under food law for very stringent testing. Go on up the scale, to a drug prescribed one-to-one by a doctor who knows your history, where randomised controlled trials are the norm and a product license is mandatory.

Then consider what might be the kind of measure which demanded the ultimate in standards of safety and efficacy. It would probably be a treatment which was given at public expense, to millions whose medical histories were not known, many of whom had not consented to it, did not want it, and could not benefit from it, by an uncontrolled dose, for a lifetime, without a medicinal license and without any monitoring worth the name. This is a factual, not an emotional description of this most curious of medical interventions. Standard textbooks tell of its high relative toxicity; the MRC acknowledges its narrow therapeutic window. To argue that it should be subject to less rigorous testing than a normal licensed drug is surely untenable. Artificially fluoridated water would not get very near a product license on present evidence.

There are other arguments. Civil liberties are sometimes mentioned, but I think it is more properly a question of medical ethics. We live in a society, and are part of a larger community, where it is accepted (and codified [8]) that a patient has the right to refuse medical treatment. This is where the comparison with seat belts and crash helmets falls down, quite apart from the difference in seriousness in the dangers addressed. Informed consent to treatment is at the individual level, and cannot be overridden by a community vote which is what has been suggested. Those who want to break this code for causes other than public safety surely have a duty to explain themselves, which they have not discharged by appeals to a fairly modest reduction in tooth decay among some children. And if one were going to medicate the many for the sake of the few one can think of a range of life-threatening conditions which would take precedence, perhaps beginning with statins in the water for heart disease.

There are two other significant areas for which there is not enough space to discuss here. Cost is one. Claims for the cost-effectiveness of water fluoridation need to be treated with the same caution as for its safety and efficacy, since little good work has been done. At least two European countries have given up fluoridating because (in part) over 99% of water misses its intended target. The second area is that of environmental effects. At least two other countries have rejected it largely for environmental reasons, a debate we have not even begun in this country despite fluoride's long-standing classification as a Dangerous Substance under European Directive 76/464/EEC for the protection of the aquatic environment, and despite the hard-hitting recommendations of the June 2003 report of the Royal Commission on Environmental Pollution, Chemicals in Products [9]. In international terms fluoridation is very much a minority pursuit, confined largely to the English-speaking world. None of this can be gleaned from standard fluoridation briefings.

Those who are interested in understanding more about this long-running dialogue of the deaf might like to read the account of an independent social scientist Brian Martin, who in Scientific Knowledge in Controversy: The Social Dynamics of the Fluoridation Debate [10] analyses the forces that have shaped the debate, and argues persuasively that not even the science can be understood without some grasp of the social history. It certainly makes what I described at the beginning as a "curious issue" more easy to understand.

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House of Lords

References and footnotes

1. For fluoride in water as a medicinal product, see the opinion of Lord Jauncey in McColl v. Strathclyde Regional Council, Sessions Cases 1983, 244; see also Codified Pharmaceuticals Directive (2001/83/EC), Article 1.
2. Editorial, Chemical & Engineering News 1988; 66: 3.
3. Baum M. Screening for breast cancer: a cruel deception. HealthWatch Newsletter issue 48, January 2003.
4. NHS Centre for Reviews and Dissemination, Report 18. A Systematic Review of Water Fluoridation, September 2000. Downloadable from: http://www.york.ac.uk/inst/crd/fluores.htm
5. Sweet M. Lessons from the HRT story. BMJ 2003; 326: 58.
6. Medical Research Council Working Group Report. Water Fluoridation and Health, September 2002.
7. One or more of: Profs. J.Kleijnen, T.Sheldon, G.Davey Smith, Sir Iain Chalmers.
8. The Patients' Charter and the European Convention on Human Rights and Biomedicine 1997 (Articles 2 & 5) both provide for the need for individual informed consent. The UK has not signed up to the latter.
9. Report of the Royal Commission on Environmental Pollution, Chemicals in Products: Safeguarding the Environment and Human Health. June 2003. Full report and summary both downloadable from the Commission's website http://www.rcep.org.uk/chemicals.html
10. State University of New York Press, 1991.

Top of page

Modern myths challenged: How much water should you drink?

There is a popular belief that we need to drink 1.5 litres of water a day, in addition to other fluid intake. The origins of this belief may be traced back to misinterpretation of the physiology of fluid balance, it is also possible (although there is no evidence) that it has been popularised by companies selling bottled mineral water. Certainly it is now relatively common to see people walking along the street sipping water from a bottle.

A story I was told by a colleague in the Clinical Chemistry Department: one day a number of 24 hour urine samples arrived for analysis, and all were exactly 1.5 litres. A junior nurse, who had read in her physiology text book that average urine output was 1.5 litres per day, had discarded the excess from larger samples, and added water to smaller samples.

The average daily output of urine is often said to be 1.5 litres (an over-estimate, see table, below), and at first glance it might seem obvious that we would need an intake of the same amount of fluid to replace this loss. However, as shown in the table, total fluid output from the body is about 3 litres for an adult man and about 2.1 litres for a woman; urine accounts for less than half of this. The remainder is made up of sweat, water in exhaled air, other "insensible losses" and a relatively small amount in faeces (this latter is increased by a high fibre diet—one beneficial effect being that the fibre retains water in the intestinal tract, so softening the faeces).

Daily fluid balance (source: reference 2)

		adult man		adult woman	
		ml/day	%	ml/day	%
intake	fluids	1950	65	1400	67
	water in food	700	23	450	21
	metabolic water	350	12	250	12
	total	**3000**		**2100**	
output	urine	1400	47	1000	48
	sweat	650	22	420	20
	exhaled air	320	11	320	15
	insensible losses	530	17	270	13
	water in faeces	100	3	90	4
	total	**3000**		**2100**	

On the input side, fluid consumption in beverages accounts for only about 65% of total fluid intake. In addition to

water in beverages, our food provides a significant amount of water—21 to 23% of total intake, and more if we eat the recommended 500g of fruit and vegetables per day. Fruits and vegetables contain between 60—90% water; indeed, celery, lettuce and melon are more than 95% water. A further source of water is known as metabolic water—the water produced when fats, carbohydrates and proteins are oxidised to yield energy; this accounts for about 12% of total water "intake", and more on a high fat diet, or when we are metabolising fat reserves (eg while slimming).

One of the arguments put forward by those extolling us to drink 1.5 litres of water a day is that beverages such as tea, coffee and alcohol are dehydrating, and cause more loss of water than they contain. This is possibly true of small cups of very strong coffee, and undiluted spirits, but not of normal beverages. In the olden days, workers in the steel mills of Sheffield—a very hot environment, where sweat losses were considerable, maintained fluid balance perfectly well by drinking copious amounts of strong (and often cold) tea. No self respecting steel worker would drink water!

Sweat losses obviously depend on the temperature and physical activity, and we do indeed need to drink more in a hot environment or after strenuous exercise; losses in exhaled air, faeces and other insensible losses are relatively constant; urine output varies widely, depending on how much fluid has been consumed. Although average urine volume is 1 to 1.4 litres per day, this reflects average fluid intake; in fact the output of urine required to ensure adequate excretion of waste material and maintain fluid balance without becoming dehydrated is no more than about 500 ml.

Put simply, the more you drink, the more urine you will produce. In her book "Water Detox" [1] Jane Scrivner tells us "Every time you go to the toilet take 8 mouthfuls of water to replace the fluids lost" Surely this is self-perpetuating. The more you drink, the more urine you will produce; the more urine you pass, the more water you are told to drink!

Scrivner [1] also lists some health claims for drinking water. She states that five glasses of water per day decreases the risk of colon cancer by 45%, breast cancer by 79%, bladder cancer by 50%. She cites no evidence for this claim, and I know of none. She also tells us that arthritis is due to lack of water, so bones become dehydrated and cause friction, back pain is caused by lack of water because the sacs of fluid between the vertebrae dry out, and high blood pressure is because our blood becomes dehydrated and fails to flow as freely as it should. There is no basis for the first two statements, and the last is incorrect—the physiological response to dehydration is a fall in blood pressure, not an increase.

What constitutes "water"? Some proponents of detox diets allow you to drink tap water, but would prefer you to purify it with a (relatively inexpensive) filter or (rather expensive) reverse osmosis apparatus. Many diet book authors consider the aluminium in tap water to be hazardous, but Scrivner [1] tells us that "aluminium helps protect the immune system" (there is no evidence that the small amounts of aluminium in tap water arising from the purification process is hazardous, but equally none that it is beneficial. As far as I know, aluminium has no physiological functions). Some tell us that it must be bottled mineral water, not from the tap; some allow plastic bottles, some do not; some consider carbonated (fizzy) water to be acceptable, others do not. Water mixed with fruit juice is OK, water mixed with fruit cordials is not; tea and coffee do not count as water, but herbal teas do!

A final problem is whether water is the most appropriate liquid to drink, especially after considerable losses in sweat. The answer is, probably not. Profuse sweating involves loss of mineral salts as well as water, and these losses have to be made good. This has led to the development of carefully formulated sports beverages, providing carbohydrate (as an energy source) and mineral salts to replace losses. In Japan these are sold from vending machines in the street under the delightful name of "Bottled Sweat"; in this country and the USA they have more attractive trade names. The mineral salts are obviously important to people engaging in very vigorous exercise involving significant sweat losses. Whether those of us who engage in more moderate exercise need to worry, is less clear.

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2. Snyder WS, Cook MJ, Nassett ES et al (1975). International Commission on Radiological Protection, Report of the task group on reference man. Pergamon Press, Oxford

Top of page

Chairman's report: An independent HealthWatch: low in funds but ever richer in influence

John Garrow, HealthWatch's chairman, gave this cautiously upbeat assessment of progress at the Fifteenth HealthWatch Annual General Meeting at the Medical Society of London held on 28th October 2003

IT IS MY job to tell you what has happened in HealthWatch since the last AGM. I know I am biased, but I am glad that, after 15 years, we are still making increasingly significant steps towards our charitable objectives. These are to promote proper testing of treatments and better regulation of practitioners, and to inform the public and the media that valid clinical trials are the best way to ensure that patients are given treatments that work.

These are laudable aims, but there are consumer-protection organisations (like CA) with similar aims. There are also government committees, with NICE acronyms and a lot of money, that also aim to raise the standard of clinical practice. What can HealthWatch do that these powerful organisations cannot do far better? If we had a lot more money would we do much better?

In a way, I believe that our lack of financial clout is a source of strength, though of course it is also a serious limitation on the activities we can support. But scientific evidence is not the sole determinant of the vigour with which health-promotion programmes are pursued. Politicians have their own agenda (namely to be re-elected) so they will not adopt policies that are politically incorrect. Last year the issue of the cost-effectiveness of screening for breast cancer was brilliantly analysed by Professor Michael Baum, our Award Winner for 2002. But even if the ratio of risk to benefit is shown to be unfavourable, what government minister would dare to admit that a programme, for which they have claimed such credit, was of doubtful benefit?

Take another example. Absolute honesty is a requisite of reliable medical research, but substantial funding is also required. If the research shows that a treatment is ineffective or dangerous, the company funding the research may try to bury this bad news by financial pressure on the researchers or their host institution. In medical research, as in other spheres, the sponsor who pays the piper usually tries to call the tune. Who is to challenge him? This year our Award Winner 2003, Dr Peter Wilmshurst, has found that challenging vested interests is a difficult and dangerous policy. It can only to be attempted by those who are independent of sponsors who have academic, political or financial vested interests.

So my reason for saying that we are stronger for our lack of financial clout is that we are truly independent of any sponsor who could bias our attempt to achieve our charitable aims. HealthWatch has a current interest in observing the Ask about Medicines campaign, which encourages patients to obtain more information about medicines from healthcare personnel. The aim is laudable, but it is possible that Pharmaceutical Companies could use the procedure as a 'back door' method of promotion for prescription-only products.

In conclusion, here below are the activities which currently contribute to achievement of our aims:

- HealthWatch Newsletter (editor Mandy Payne)
- HealthWatch website http://www.healthwatch-uk.org (managed by David Bender)
- Helpline 0208 789 7813 (run by Michael Allen)
- HealthWatch Annual Award to the scientist or journalist who has contributed most to reliable healthcare information, past winners include Professor Sir Richard Doll, Professor Michael Baum, Dr Geoff Watts, John Diamond
- The Student Prize (funded by AJAHMA Charitable Trust) to encourage healthcare students to learn how to examine critically evidence of efficacy.

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Top of page
