quoted, repeat measurements obtained at least three years apart to monitor the effectiveness of treatment are likely to be of little clinical use.

Compston and colleagues conclude that the United Kingdom does not have enough bone densitometry machines, with 1.6 dual energy x ray absorptiometry machines per million people compared with 2.9 in the United States and 13.5 in Greece. This conclusion is not supported by their data. They estimate that 175 bone density scans are needed per 100 000 population. If we assume that a machine is used for 50 weeks a year, five days a week, and with a throughput of 10 scans a day (a conservative estimate) then one machine could provide 2500 scans a year. This is enough for a population of $2500 \times 10^6 / 1750 = 1.43$ million people. On the basis of their figures the United Kingdom would therefore require 0.7 machines per million people to meet the needs identified in the paper. It seems that we have more than an adequate supply.

The fact that the per capita supply of dual energy x ray absorptiometry machines in the United Kingdom is lower than that in most other countries possibly reflects the greater ability in the NHS to control the diffusion of health technologies and to control costs. The United States has eight times the number of magnetic resonance imaging scanners and Japan 12 times the number of computed tomography scanners and eight times the number of extracorporeal shockwave lithotripsy machines per million population as does the United Kingdom.¹ I hope that the authors would not interpret such variation as implying that the number of machines in the United Kingdom should rise to similar levels. The purchase and use of health technology need to be part of a planned service based on the assessment of cost effectiveness and need and should not be driven by commercial interest, clinical enthusiasm, or naive international comparisons.

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Is indicated after long term treatment with heparin

EDITOR,—There is an important omission from the clinical indications for bone densitometry listed in J E Compston and colleagues' review article¹—namely, long term treatment with heparin, which causes osteoporosis.² The most common indication for long term heparin is thromboprophylaxis in pregnant women. It is our policy to treat women at high risk of thromboembolism (those with a history of recurrent thromboembolism and those with thrombophilia or antiphospholipid antibodies) with subcutaneous heparin throughout pregnancy and the puerperium. In some units even women at lower risk of recurrent thromboembolism (for example, those who have had only a single thromboembolic event) may receive heparin throughout pregnancy.

Part of the problem is that pregnancy itself may cause a reduction in bone density. In a prospective study of bone mineral density in pregnancy Dahlman *et al* used single photon absorptiometry and found a 5% reduction in trabecular bone

(equivalent to two years' postmenopausal loss) immediately postpartum compared with the value at 12 weeks' gestation in women receiving prophylaxis with heparin.³ The same group found an incidence of osteoporotic vertebral fractures of 2.2% in 184 women given subcutaneous heparin for antenatal thromboprophylaxis.⁴

Our current practice is to offer dual x ray absorptiometry post partum to any woman who has received heparin for more than 12 weeks during pregnancy. Four of 13 such patients had osteopenia (bone density > 1 SD below the mean for non-pregnant women matched for age) when studied one to 10 months after delivery. Current evidence suggests that the effects of pregnancy and heparin on bone density are reversible. Since women who require prophylaxis with heparin in one pregnancy usually require it in subsequent pregnancies, assessment of bone density is clinically important. For example, women found to have osteopenia are advised to delay further pregnancy pending the result of repeat bone densitometry after six to 12 months.

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Author's reply

EDITOR,—Catherine Nelson-Piercy and Michael de Swiet are right to draw attention to long term heparin treatment as another important, if relatively rare, risk factor for osteoporosis.

I welcome the opportunity to respond to Trevor Sheldon's views. Contrary to the implication in his letter, the conclusion that mass screening of perimenopausal women is not justifiable was reached by several experts^{1,2} before publication of the *Effective Health Care* bulletin, the conclusions of which were fortuitously correct although based on inadequate and selective information.³

In our review we emphasised that bone densitometry could be justified only if the result influenced management; it has been shown that the finding of low bone mineral density improves compliance with treatment and changes in lifestyle.⁴ Furthermore, Sheldon is incorrect in stating that multiple risk factors in elderly women are more important than low bone mineral density in predicting hip fractures. In the study quoted the two were shown to be complementary, the rate of hip fracture in women with multiple risk factors being nearly three times higher in those in the lowest than those in the highest third of bone density.⁵

Sheldon does not provide evidence to support his assertion that the precision of measurements of bone density in routine settings is poorly documented. Many centres have reported precision of $\leq 1\%$ and 1-3% for spinal and femoral measurements respectively. As with all treatments, responses in patients with osteoporosis are heterogeneous, and repeated estimates of bone mineral density at appropriate time intervals provide the only means by which responses may be monitored.

With regard to Sheldon's repeated references to cost effectiveness we estimate that the introduction of a clinical bone densitometry service would result in an annual reduction of about 14 vertebral

fractures, three forearm fractures, and one hip fracture for the average health authority, with a cost per fracture averted of £1391 and a cost per life year saved of £22558. We agree that cost effectiveness is important but cannot accept the implication that this should drive the provision of health care. Those of us responsible for the care of individual patients recognise that the benefits of clinical practice cannot be wholly captured by such a conceptually simplistic notion, which is itself an inexact science. Improvements in medical care require a balanced and rational approach to the assessment of new technologies; the negative attitude and tunnel vision exhibited by Sheldon are not constructive in this respect.

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Publishers' copyright policy

EDITOR,—In my retirement I have completed a bibliography of research on infant feeding for distribution to doctors and others professionally interested in the subject. The bibliography is on a floppy disk and contains about 3200 references, with their published abstracts in most cases, together with built in software for making computer searches and for adding users' own references. My intention is that it should be distributed at nominal charge to cover postage and packing etc, especially to colleagues out of reach of library services, including those working in underprivileged communities around the world. I have a commercial sponsor, which has defrayed most of the expense, but neither the sponsor nor I will make any money out of it, and that has not been our intention.

To be able to include the abstracts, I wrote to the roughly 160 publishers of the journals involved, explaining my intentions and requesting their permission to include the abstracts. All but about five readily accepted, including the BMJ Publishing Group, with many of them expressing approval and interest and some even offering to collaborate.

Sadly, however, three publishers of prominent journals in my subject refused or (in the case of the *New England Journal of Medicine*) imposed conditions that were impossible to accept and ignored my subsequent appeals. They were the Massachusetts Medical Society (*New England Journal of Medicine*), Raven Press (*Journal of Pediatric Gastroenterology and Nutrition*), and Scandinavian University Press (*Acta Paediatrica Scandinavica* and related journals), which account for about 320 of my 3200 citations.

From this experience it is clear there is no uniform attitude among publishers. In the course of the many hours I spent deleting the 320 abstracts, I reflected on the great loss not only to users but also to the authors of the papers concerned. In the days when I wrote papers I would not have offered them to any publisher that, for no obvious reason, was going to deprive me of readers of merely my abstracts, my signposts to the scientific community.

My two reasons for writing are, firstly, to alert today's authors to this small minority of publishers

that refuse to allow free dissemination of their abstracts (even though these are readily available to the CD-ROM services) and, secondly, to ask whether the BMJ Publishing Group could exert some influence on other publishers. Is there no way that publishers could get together, if only in the interests of standardisation of policies? Could not all publishers agree that abstracts should be as free from constraints as reference citations?

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*Although abstracts of scientific articles enjoy the same copyright protection as the articles they summarise (except in the United Kingdom), publishers of scientific periodicals have traditionally made them available to abstracting and indexing services without charge. Over the past few years, however, there has been a huge increase in the number of requests to use abstracts in various electronic products: diskettes, CD-ROMs, and online services. Many medical journals have also introduced structured abstracts. These changes are causing publishers to rethink. They are also worried that with digitisation they may lose control over the use and even integrity of their abstracts. We may thus see more publishers either refusing permission for their abstracts to be used or levying a fee for digital use; but it is very much the policy of the BMJ Publishing Group whenever possible to make all our material available as cheaply as possible, especially to doctors and others working in very poor countries.—EDITOR

Reasons for increased incidence of tuberculosis

EDITOR,—A Hayward and colleagues¹ disagree with our conclusion that the 12% increase in the incidence of tuberculosis in England and Wales between 1988 and 1992 affected the "low risk" white and West Indian communities as well as the "high risk" Indian subcontinent, African, and refugee communities.² They restate our conclusion that the data on incidence by local authority districts in England and Wales are uninformative on this point because the increase was concentrated in poor districts and ethnic group and poverty are highly correlated across districts. Data on incidence over time according to ethnic group are necessary to examine the issue, and such data were available for the London Borough of Hackney.

It was possible to assess changes over time in the size of the ethnic minority communities by using census data for 1981 and 1991 as an unpublished survey by Hackney Borough Council after the census in 1981 showed that the country of birth of the head of household was then an accurate estimate of the ethnic origin of people in Hackney. The data for Hackney showed that the proportionate increase in incidence affected white, West Indian, and Indian subcontinent residents equally. The cases occurring among the relatively large African and refugee communities in Hackney accounted for less than half the increase in cases from 1988 to 1992.

The accuracy of our estimates of the size of these communities is not relevant here; there is no doubt that in most other socioeconomically deprived districts these communities are smaller or non-existent, so there will be even fewer cases than in Hackney. Perhaps the simplest evidence that tuberculosis has increased in white people comes from the Irish data we cited³: the Republic of Ireland has few non-white residents but recorded a greater increase in tuberculosis (16%) than that in England and Wales (12%).

While the recent increase in tuberculosis did occur in white people, the disease remains far more common in certain ethnic minority groups. The data of M J Doherty and colleagues on incidence in London boroughs in 1991 confirm this,³ but they are uninformative on the ethnic mix for the increase between 1988 and 1992.

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Ethical imperative to publish extends to academics too

EDITOR,—I support John Pearn's view that publication of research findings is an ethical imperative.¹ The medical community at large can assess the status of something only if all the relevant information is available.

The pharmaceutical industry is often accused of selective publication of results, and there is some truth in the accusation. There is just as much truth, however, in the charge that academics also fail to publish important items of information. They may not be driven by financial conflicts of interest, but to an academic career other forms of conflict are likely to be just as important, but these tend to be ignored by those drawing up guidelines. The urge to ensure the survival of a point of view with which an academic has become identified is often a stronger driving force leading to bias than is any possible financial gain or loss.

In the context of the pharmaceutical industry I believe that all the research done by a company in both toxicological and clinical areas should be freely published. There are, however, genuine but relatively easily surmountable reasons why this is not a sensible policy for any company that respects, as it must by law, the interests of its shareholders. When a company applies for approval for a new drug in the United Kingdom the law provides that the company's dossier should remain confidential for 10 years. Most other countries have similar laws providing for protection for between six and 10 years. This means that during this period the regulatory authorities cannot use the information in the company's files to license generic copies of the new drug.

If the information in these files is published, however, then the regulatory authorities may decide that there is sufficient information in the public domain for a generic licence to be issued before the expiry of the 10 year confidentiality period. If a product does not have full patent protection then it is in the company's commercial interest not to allow publication before expiry of the 10 year deadline. This is clearly not in the interest of the wider medical community or the public. The situation could easily be corrected. A statement from the European Medicines Evalua-

tion Agency, endorsed by all member countries, could be made to the effect that published material would not influence licensing decisions during the 10 year confidentiality period. This would eliminate a major reason for the non-publication of material by pharmaceutical companies.

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100 years of x rays

EDITOR,—I think it pertinent to mention the importance of the discovery of x rays in another area that has affected our medical lives, besides those areas mentioned by Daniel J Nolan.¹ The news took some time to reach South Australia: all communication took six weeks by boat. The discovery was exciting for my grandfather William Bragg, who was professor of physics at Adelaide University, and figure 1 shows an x ray picture of his hand, dated 1 June 1896. The abnormality of the little finger is a result of a childhood injury. That same summer his son, William Laurence Bragg, who was a toddler, broke his left elbow. I believe that the x ray film of his arm, obtained in 1896, was the first film to be used to diagnose a fracture in Australia.

Both Professor Bragg and his eldest son (later my uncle) became increasingly fascinated by x rays, and this led to the birth of x ray crystallography. In 1915 they shared the Nobel prize for their discovery of the structure of salt. From the beginning my uncle, later Sir Laurence Bragg, foresaw the possibility of discovering the structure of biological chemicals, seeing the excitement of working out biological enzymatic reactions from a structural perspective. I remember visiting the Davy Faraday Research Laboratories at the Royal Institution in the 1950s, just as the structure of lysosyme was being discovered.

Sir Laurence went on to supervise the work of Crick and Watson when they discovered the double helix in Cambridge, and this was the crowning of his career in x ray crystallography: his original vision had been fulfilled. It was a joy to him to travel to Stockholm to celebrate the 50th

FIG 1—x Ray picture of Professor William Bragg's hand, obtained in 1896 (reproduced from reference 2 by permission; source—Flinders Medical Centre, Adelaide, Australia)