purchasers might consider "buying" inadequately evaluated therapies, on the same basis that physiotherapy was derided for accepting electrotherapy by "... giving credence to unscientific hype". Caveat emptor.

I fully endorse the author's call for more research into the efficacy of physiotherapy, and already many of the obstacles that impeded physiotherapy research are being addressed. In a recent letter in the British Journal of Rheumatology I explained that through the creation of university departments, the expertise and career structure exists to enable us to advance research in physiotherapy. We are now successfully competing for funding to critically evaluate our treatments, so that we can deliver the most effective treatment to our patients with the optimal use of resources.

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AUTHOR'S REPLY: I am delighted that Dr Hurley agrees with me that much physiotherapy requires proper evaluation. This does not, however, imply repeating experiments indefinitely until the answer that the researcher wishes to have obtained is well conducted piece of research may well be all that is necessary to answer a question, and at the least it requires an equally valid reply rather than prejudice hidden words such as "measured judgments".

Had Dr Hurley read my editorial carefully he would have realised that I nowhere advocated the use of massage. He must accept, though, that massage and other complementary therapies are already high on the list of purchasers' wishes. A recent survey by the National Association of Health Authorities and Trusts showed that 65% of District Health Authorities and 70% of Family Health Services Authorities favoured purchasing such therapies as part of their NHS provision. Probably many of them act only by a placebo effect, but few are likely to be purchased on their merit, as Dr Hurley does for physiotherapy, the use of complex pieces of electrical equipment such as lasers as placebos.

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Distinction between initiation and progression of the osteoarthritis process

I read with positive interest but negative feelings the article by Cumming et al. Their conclusions and distinctions of the hip should be included in the list of factors that protect against hip fracture, is in line with our previous observation on the inverse relationship between osteoarthritis and osteoporosis, and in particular with the recent epidemiological evidence revealed in the MEDOS Study. The MEDOS study is also based on self-reported osteoarthritis in a large series of controls and hip fracture cases. In both studies the inverse relationship between osteoarthritis and osteoporosis is independent of body weight, which supports the hypothesis that there is a direct causal relationship between osteoporosis and osteoarthritis.

A disturbing element in the paper by Cumming and Klíneberg is the confusing terminology used throughout the paper. The term 'arthritis' is used interchangeably with 'osteoarthritis'. We do not agree that this interchangeable terminology should be used in an international rheumatology journal. The term arthritis is so bound to many other forms of arthritis, in particular rheumatoid arthritis, gout and Pelvispondylitis, that this will inevitably lead to confusion in later citations. Although the term osteoarthritis is also not the best one, this term is now well accepted as an alternative to osteoarthritis. According to our opinion and to many others, such as, Radin, clear distinction should be made between initiation of the osteoarthritis process and progression. That secondary inflammation might be involved in the progression of osteoarthritis is well accepted, but whether inflammation is the primary trigger of osteoarthritis is doubtful. A number of studies on the initiation of the osteoarthritis process support the possibility that the increased bone density reduces the mechanical stability of subchondral bone to deform under impact loads with resulting damage to the articular cartilage and osteoarthritis.
Distinction between initiation and progression of the osteoarthritis process.

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