The British Orthopaedic Association Response to:-
Nice Assessment report
Total hip replacement and resurfacing arthroplasty for the treatment of pain or disability resulting from end stage arthritis of the hip.

Author
[redacted]

As ever NICE (and specifically the team undertaking this analysis) are to be congratulated on the thorough and detailed work that has taken place in this assessment. An enormous amount of excellent analysis has been carried out, any subsequent comments below should be taken as such; are meant to be helpful; and not direct criticism.

Comments on introductory section.
(Accepting this is not part of the formal assessment)

The description of the patient group is apt but underlies fundamental problems in diagnosing "osteoarthritis" which is, as noted briefly, a collection of end state symptoms, signs and disparate pathological features that are not caused by a currently diagnosable inflammatory disorder (rheumatoid, itself defined by a committee) and in many cases may be due to a variety of as yet unidentified genetic metabolic traumatic or mechanical issues. Perhaps the wording on page 28 goes some way to trying to tease this out yet somehow manages to imply that it is one, understood, condition. An understandable but misleading overstatement.
This is further highlighted when the difficulties of correlating the radiographic appearances with symptoms severity of symptoms and the benign effects of age are discussed. Though not formally part of this assessment I would suggest a little more is written in this section, there is a widespread belief that radiographs are the arbiters of benefit. Management based on radiograph alone is not supported in the NICE document on OA hip, this paragraph could do with a little expansion from the previous guidance.

Page 31 penultimate paragraph the cost of conservative treatment is mentioned.

Page 35 3.1.4.4 second paragraph again mentions opiates however I believe this relates to codeine type opiates not buprenorphine and other highly addictive narcotics recently available for home use specifically for the management of patients with severe pain secondary to terminal cancer.
This is an important distinction, few patients come off these strong opiate drugs after successful hip replacement removes their reason for pain.
When using very strong opiate analgesics for benign pain this is now known to create dependency and increase attendance at GP surgeries with several recent reviews on their use for pain from a benign condition which in this case is reversible.
I am not aware of proper papers on the use of strong opiates for the longer term management of single joint arthritis (which is reversible by surgery). Yet there use has become widespread.
This is neither the subject of this assessment nor is their sufficient evidence at present to give definite advice however the use of the word "opiate" could be taken to construe that the use of buprenorphine

etc. should be tried before surgery, this would be a derogation from evidence based medicine. Indeed there is current advice that they are avoided as they are known to increase visits to the general practitioner and have considerable side effects with concerns about driving ability.
Could I suggest that the word opiate is modified to minor opiate (personally I would suggest that patients are referred for surgery before they are considered for strong opiates for isolated joint disease…. But then I have an inherent bias!

Figure 1 algorithm looks satisfactory however I would suggest in box 1b simple analgesia. Actually the evidence for this is thin and the evidence for strong opiates thinner.

Figure 2 most patients are followed up at 6 weeks then 1 year around the country, very few are reviewed at day 10 as noted here. After subsequent review at one year most units only follow patients at 5 yearly intervals unless there are problems such as the MoM recall. After a certain age follow up is often discontinued (around75) as the patient is likely to have increasing difficulty attending and less likelihood of revision.
There is considerable variation around the country with many commissioners only allowing one postoperative visit.

Section 3.2.1.5 this section is surprisingly short and fails to mention the problems of the interaction between the head size sliding distance (volumetric wear) and thickness of bearing surface all of which interact an d cause problems. This section needs expanded. In the evidence section table page 164 there is reference to bigger heads causing more wear from the English NJR. (Haven't checked may refer to MoM only). Appreciate that head is a confounding factor in most if not all studies however it should perhaps be mentioned that patients come in many sizes with outside cup diameters from 38 to 60+ and that this places practical constraints on the size of head that can be inserted and the type of bearing surface available (in the smaller cup sizes only polyethylene may be available and head size may be as little as 22 mm for practical limitations.

Section 3.2.3.1 dislocation first paragraph needs a range of dislocation rates for revision.

Second paragraph the most common cause for early dislocation is mal-positioning, late dislocation wear and soft tissue problems become more common.

Section 3.2.4 one other reason for increased revision rates is the metal on metal problem, this should be highlighted.
Perhaps the significant increase in overall numbers is due to catch up on waiting lists and a short comment on the overall rate of hip replacement compared with other European countries should be made.

Section 3.4 again OA is what is left after the inflammatory arthropathies have been removed. It is really an end stage pathological state rather than a diagnosis (feel free to ignore this comment).

The sections based on national registry results are a welcome feature as it moves the information from full randomized trial ***efficacy*** to real world ***effect***. The data has been used well and effectively recognizing its limitations and accepting the undoubted knowledge they impart.

Page 186 the deviation from scope is entirely appropriate.

The methodology is beyond my personal knowledge but seems overall appropriate the assumptions reasonable and the various categories sensible (with the caveat about head size mentioned earlier).

The Industry responses are interestingly variable but understandable.

The DePuy paper comes to the same overall conclusions however they support a variety of implants in their model. We should not be surprised and they do make several valid points about the learning curve (surgeon's abilities) and the patient groupings are homogeneous. That it can be criticized is perhaps a reflection of the smaller amounts spent on implants compared to drug marketing.

The issue raised by DePuy (the effect of surgeon's skills) raises questions recently considered in papers from the Australian register about how many of one particular procedure needs to be done before competence is gained. There are many surgeons who do relatively few procedures extremely well but stick to one implant. There are some high volume surgeons who have not made a good implant selection so their results are less than would be expected. Surely we can say that most surgeons should stick to one implant.

As there is little clinical difference between groups (other than resurfacing) and that the advice is made on economic grounds between the THR groups (and the differences in cost are less than the use of LMWH as prophylaxis) there will inevitably be continuing debate. However the lack of clinical difference actually makes the point that a surgeon should stick to one particular implant.

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