PERSONAL VIEW

Is arthroscopy of the knee completely useless?

META-ANALYSIS - A REVIEWER'S NIGHTMARE

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I have personally always thought of meta-analyses as a poor man's research – no original thinking or idea, no designing and validating a study, no need to obtain ethics committee approval or funding, no chasing of patients for long-term evaluation – just sitting in an office, in front of a computer screen, judging the value of other peoples' work.

As a reviewer, being asked to judge the value of this type of research is a nightmare, as the conclusions of such studies are often used to justify or withdraw the funding of a particular surgical practice. A case in point is the recent publication of Thorlund et al[1] in the *British Medical Journal* which came to the conclusion that:

"The small inconsequential benefit seen from interventions that include arthroscopy for the degenerative knee is limited and absent after two years. Knee arthroscopy is associated with harm. Taken together, these findings do not support the practice of arthroscopic surgery for the middle aged patient or older patient with knee pain."

This was followed by an editorial which stated that:

"Supporting or justifying a procedure with the potential for serious harm, even if this is rare, is difficult when that procedure offers patients no more benefit than a placebo. If, as reported, the mortality associated with arthroscopic knee surgery is 0.96 (95% confidence interval 0.04 to 23.9) per 1000 cases and the rate of deep venous thrombosis is 4.13 (1.78 to 9.60) per 1000 cases then, with rates of surgery at their current level, a substantial number of lives could be saved and deep venous thromboses prevented each year if this treatment were to be discontinued or diminished. We may be close to a tipping point where the weight of evidence against arthroscopic knee surgery for pain is enough to overcome concerns about the quality of the studies, confirmation bias, and vested interests. When that point is reached, we should anticipate a swift reversal of established practice."

This second statement was picked up by the media in sensational fashion. It is unreferenced, and not supported by the evidence in the paper by Thorlund et al and although anecdotal, in my immediate peer group we have performed over 60 000 arthroscopies without a death.

So, is Thorlund et al's paper all it seems, and can these conclusions and statements be justified from the evidence presented? The reviewer's problem is the size of the analysis, with apparently over 4000 papers considered. Clearly, they cannot all be listed, and so it is impossible to say if valid pieces of work have been discarded or ignored.

The authors exclude all but randomised controlled trials. This sounds good, but a study where patients are randomised into surgery or physical therapy is reviewed.[2] However, a prospective study on surgery for patients who are symptomatic but have already failed conservative treatment, is apparently invalid.[3]

There is certainly one prospective, single surgeon, consecutive, randomised, controlled study,[4] showing that 50% of patients improved five years after surgery, which does not seem to have been included and, if there is one omission, there may well be others.

A total of nine papers out of 1789 were analysed. Papers were largely excluded on a review of the title and abstract. One paper was excluded because it was a five-year review of the same cohort previously published after a two-year review. Interestingly, the discarded paper had a different conclusion to the two-year review and included stating that one third of patients continued to experience disabling symptoms when receiving physical therapy, but improved after arthroscopic intervention.[5]

Of the nine papers, three relate to the arthroscopic treatment of osteoarthritis (OA), five to degenerative meniscal tears (one of which is a trial assessing the contribution of depression and anxiety[6] and one to a combination of OA and meniscal tears).[2]

Of the papers specifically related to degenerative change, one, with a placebo arm,[7] and which only enrolled 55% of eligible patients, was undertaken in a population of veterans

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who had no incentive to improve. Indeed, the authors appear to have changed their method of statistical evaluation three times until they achieved the desired result. A paper by Chang et al[8] concluded that "surgery may be beneficial" (and is a smaller series than Hubbard et al's[4]), and in Kirkeley et al's paper,[9] 26% of those patients who were eligible did not complete the trial, patients with early OA were excluded, and 56% of their patients had grade 3/4 OA who would not routinely be offered arthroscopic surgery in the United Kingdom.

The papers assessing outcome for degenerative meniscal tear had dubious criteria for surgical intervention, with an over reliance on MRI diagnosis and "one symptom that might be attributable to a meniscal tear". In the paper by Sihvonen et al with a sham surgery arm,[10] only the meniscal tear was addressed at surgery, yet 70% of the patients had significant degenerative change at arthroscopy, and symptoms were unlikely to have been attributable solely to the meniscus.

In the paper by Katz et al[2] on intervention for a combination of OA and meniscal tears, of 1330 patients eligible for the study, only 351 opted to be enrolled. Of the 150 patients in the physical therapy group, 51 had crossed to the surgery group in the first six months, and a further eight did so between six and 12 months. After surgery, these patients improved to the level of the initially operated on patients. One must ask the question if the most likely reason for refusal to enter the trial is that the patient had already had physical therapy and did not want to enter a trial in which the treatment was one that had already failed them.

The evidence that arthroscopic intervention for "knee pain" is of no benefit would seem to be thin at best, and Thorlund et al[1] have made some sweeping conclusions that do not seem to be justified by the evidence presented. An amalgamation of conclusions that are not based on hard science does not equate to a new "evidence based" conclusion. My own experience mirrors that of the classic study from Aichroth et al,[3] with the majority of patients experiencing prolonged improvement after surgery, after having initially failed conservative management.

Thorlund et al[1] state that "arthroscopy is associated with harms" and go on to analyse a completely separate series of papers, reduced from 2330 to nine. Only two papers from the "benefit" analysis make it through to the "harm" analysis. It cannot be scientifically valid to take the conclusion from one set of analysis and apply it to a completely different set of papers.

The harm analysis is largely based on registry studies, which include all arthroscopic surgery including complex ligament reconstruction, washout for septic arthritis following total knee arthroplasty etc., and not just straightforward and simple interventions. Maletis et al[11] analysed about 21 800 cases, and report only one surgically attributable death. Hetsroni et al[12] quote a 2.8 /10 000 rate of pulmonary embolism after arthroscopy of the knee, but this is strongly associated with increasing age, complexity of surgery and operating time.

Only two papers make it to each group. In the paper by Sihvonen et al,[10] the only adverse event was a deep infection, which occurred four months after surgery following a dental procedure. Katz et al reported two deaths and 31 adverse events, but one death and 15 adverse events occurred in patients receiving only physical therapy.

Whilst any surgical procedure may be associated with harm, the incidence of those associated with simple arthroscopic intervention would seem to be extremely low, and nothing like that reported in the sensationalist quote from the *British Medical Journal*.

When I was a trainee, I was told never to read the abstract and conclusion of a paper, but to read the materials and methods and the results sections and then make my own mind up if the conclusions of the paper could be justified by the evidence presented, a process I still follow as a reviewer. Reviewing a meta-analysis presents unique difficulties, namely that it is very difficult to be certain the authors have looked at every paper and then reduced this to an appropriate, unbiased selection. As a reviewer, all you can hope to do is to look in detail at each paper that is included and judge whether they are sufficiently robust from which to draw further conclusions. This does not appear to have been carried out with sufficient rigor in the case of the paper by Thorlund et al.[1]

It is the responsibility of us all not just to accept all papers and especially meta-analyses at face value, but to dig deep and see if the conclusions are truly valid, particularly in these times where these type of papers are often used to grant or withdraw funding for a particular procedure.

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