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Why good evidence is important for our clinical practice

In a perfect world we would all know which were the most effective and safest treatments for each of the pain situations in which we work. While we are nowhere near acheieving that ideal we are moving forward. It is instructive to look back forty years. At that time Moertel and colleagues compiled a comparison of various oral analgesics (1), and that was the best direct comparative data available at that time. Since then there have been a great deal more trials in pain, both acute and chronic, but very few of these trials compare different treatments. What they usually compare is the test drug and placebo, when the information we all want is to know how well one of our drugs performs against the others.

We have used meta-analysis to calculate from these placebo controlled trials league tables of the relative efficacy of analgesics. By calculating how well each intervention performs against placebo we can produce an indirect comparison; for instance, drug A might be five times better than placebo while drug B is only twice as good. It is as if we were working out who was the quickest runner by making each of us run against the clock (indirect comparison) rather than all together in a single race (direct comparison). In most circumstances the results from the indirect comparisons agree well with results from the direct (2).

One concern however is that people use these estimates of relative efficacy in an uncritical way. It is the uncritical use rather than the estimates per se that is the concern. An example is the two recent reviews of different medication in the management of neuropathic pain (3, 4). These fine reviews are important because they attempt to use the results of all the trials to derive clinically useful advice for managing neuropathic pain. My concern is that the information is used by others without being aware of the weaknesses of the data.

For several of the medications the number needed to treat (NNT) is based on very few patients studied in the trials. An obvious example is the selective serotonin reuptake inhibitors (SSRIs). From just 81 patients studied a relatively 'poor' NNT of about 7 (for 50% pain relief), which compares poorly with the NNT quoted for tricyclic antidepressants of under 3. If you unpick the trials which provided the 81 patients studied there is the well-known 'failed' trial of fluoxetine (5), but there is also a trial in which the efficacy of paroxetine was 'the same' as a tricyclic (6). I am uneasy that the reviews perpetuate a dogma, that SSRIs are

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ineeffctive in neuropathic pain, when we have so little evidence, so few patients studied, and within that small number there is disagreement. If SSRIs were to be proved effective (in a further trial or trials) then because of their adverse effect advantage over tricyclic antidepressants this is potentially important for patients.

It is good to see space being given to reviews, and that people in the pain world are thinking critically about the evidence. Part of thinking critically is to be aware of the weaknesses of the data as well as the strengths (7). We need to be sure that we do not simply swallow the conclusions without taking the opportunity to think critically.

H. McQuay

CORRESPONDENCIA
Henry McQuay
Pain Relief Unit
Churchill Hospital
Oxford OX3 7LJ
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