Reviewer’s report

Title: Cost effectiveness of epidural steroid injections to manage chronic lower back pain

Version: 2 Date: 25 January 2012

Reviewer: Alyn Lewis

Reviewer’s report:

Major compulsory revisions:-

The paper often lacks clarity. The overall objective needs to be clearly spelt out early on – that this paper (though using data from a clinical trial) is a cohort study which compares costs in relation to effects for the same patients based on a before- and after- evaluative design. To this end baseline data is directly compared to post-treatment data over the follow up time period (12 weeks) – this being done twice due to the underlying trial design and the data pooled for both baseline timepoints which are then compared to the pooled data for the two follow up periods.

The Methods section is rather scant and lacks detail for the ensuing evaluation. Indeed, some of this methodology is actually presented in the Results section. To my mind this is misplaced and should come earlier i.e. in a Statistical Analysis subsection of the Methods.

Costs are focused on intervention costs only. The authors mention that had a societal perspective been considered (including indirect costs) the findings would probably be more favourable towards the injections. To this end, I introduce my other large concern over the study: namely timeframe. This is indeed true for the first 12-weeks post-treatment. However, it may be quite different if a longer time horizon was adopted. Figure 1 clearly shows that health status deteriorates by 3 months to a point at which it worse than at baseline. Injections are known to have a fairly quick effect but there are concerns that they may be worse in the long run than simply a wait-and-see policy. Hence, the authors need to consider their Discussion in light of the limited timeframe of assessment. Similarly, I could not understand why the authors averaged week 0 and 12 values ‘as the control data’: shouldn’t this have been baseline scores (combined) versus the post-treatment scores of weeks 1 to 12 (combined). Again – this could be more clearly described.

Aggregated data seems to be implied. There needs to be a more coherent step-by-step build-up of results towards mean QALY differences between comparison groups and mean cost differences between groups to the final cost-per-QALY estimates. Perhaps further tables/graphs (if permissible) could be helpful to illustrate the process-findings.

Similarly, the Abstract needs a clearer account of the exact objective, evaluative
design being used and build-up of key results.

How were the confidence intervals for the QALY calculated?

Minor comments:-

Given the perceived objective of the paper I am not sure why the authors present week-to-week comparisons (at the start of the Results section), and statistically test for these giving results of ‘non-significance’ when the data may not be powered for these tests in the first place – again, this is confusing and goes against the grain of the objective as I see it.

**Level of interest:** An article of importance in its field

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

No competing interest.