Reviewer's report

Title: Spinal Cord Stimulation in the Treatment of Refractory Angina: Systematic Review and Meta-Analysis of Randomised Controlled Trials

Version: 1 Date: 2 July 2008

Reviewer: Linda Sharples

Reviewer's report:

Based on a well-conducted systematic review this manuscript combines the major clinical outcomes from 7 RCTs comparing Spinal Cord Stimulation (SCS) with either active controls or an alternative treatment (CABG or PMR), in patients with angina refractory to conventional treatment. I agree that this is a worthwhile aim and that the paper is very well-written, I wonder if there are sufficient trials of a reasonable size to justify a meta-analysis at this stage. Certainly I agree that further research on the effectiveness and cost-effectiveness of SCS is warranted.

Major Compulsory Revisions

Generally this work is high quality and there are few major revisions required.

The random effects distribution is not described. Was a normal distribution assumed? Whilst that is usually justified using a Central Limit Theorem argument, the argument is an asymptotic one and with such a small number of trials it is impossible to verify. The likely result is that the between-trial variance is underestimated and consequently the overall treatment effect is estimated with spurious precision. Resulting p–values from tests of treatment effects will be too small. This should be discussed and results interpreted rather more cautiously.

The SPIRiT trial very recently published 24 month results (see http://www.trialjournal.com/content/9/1/40). I think it would be unfair to expect the analyses to be repeated including the update but it should be included in the discussion, particularly since it includes EQ5D and cost-effectiveness analyses.

Quality of life assessment is multidimensional and often uses more than one instrument. Can the authors explain how this was reduced to a single measurement and justify their choices.

Minor Essential Revisions

In the introduction it would be worth pointing out that published trials are all very small and unlikely to be convincing on their own.

There is little discussion of heterogeneity, possibly because there are so few trials and all are small. Thus there is no possibility of identifying and explaining heterogeneity. This should be acknowledged.
There are quite a few spelling mistakes, missing words and other typographical errors, listed below.

Abstract:
Spelling of library in methods line 2;
Missing word ‘medication’(?) in methods line 3
Delete ‘of’ in results line 4

Introduction:
Delete ‘A number of’ before reviews and replace with ‘Some’ or ‘Published’.

Methods:
‘Data were’ rather than ‘data was’.
Final paragraph ‘only the first period…were and their variance’ should be replaced by ‘only the first period….was included and the variance’.

Results:
Within-group comparisons are irrelevant and should be removed. Thus columns 2-3 in Table 3 could be removed and replaced by data summaries.
Remove the full stop after ‘ESBY study’.
Missing ‘was’ before ‘a superior exercise’
Missing ‘in’ before ‘exercise capacity at follow up’.
Replace ‘included’ by ‘including’ in Adverse events paragraph.
Replace ‘in’ by ‘into’ before ‘account the cost of’ in the Healthcare utilisation paragraph.

Discussion:
Delete ‘that’ after ‘two therapies’.
Final sentence of section on Mechanism of action doesn’t make sense.
Delete ‘also’ after ‘Such a trial will also need’.
Missing ‘be’ in Conclusion section.

Competing interests:
Missing word (interests?) in last sentence.

Discretionary Revisions
I have no discretionary revisions.

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

**Quality of written English:** Acceptable
Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I was an independent investigator in the SPIRiT trial that was funded by an grant from Medtronic.