

## LETTER TO THE EDITOR

# Spinal cord stimulation

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Dose *et al.*<sup>1</sup> are to be congratulated on their paper describing spinal cord stimulation (SCS) generating activity in spinal networks, and thus emphasising the significance of the undamaged central nervous system in the restoration of function following a central nervous system lesion. After 30 years as the chairman of the scientific committee and trustee of the International Spinal Research Trust, as a lone voice advocating a shift of emphasis from the lesion to the intact central nervous system, with, it has to be said, very little effect, I feel this is a most welcome paper, highlighting as it does the importance of neuromodulation.

However, I would like to put the work of Dose *et al.*<sup>1</sup> in some historical perspective. SCS was, of course, first used in the treatment of chronic pain as a direct result of the seminal work of Melzack and Wall,<sup>2</sup> but the first person to demonstrate improvement in *neurological deficit* with SCS was Cook<sup>3</sup> in the early 1970s. He was carrying out this procedure for pain on a young sufferer with multiple sclerosis when to his great surprise he saw a marked improvement in spasticity with consequent improvement in ambulation. Cook<sup>3</sup> had contacted me following a paper in *The Lancet*<sup>4</sup> in which I had suggested that experimental changes in the central nervous following partial denervation and the effect of repetitive stimulation<sup>5</sup> suggested an approach to neurological deficit via the intact central nervous system rather than the lesion itself. The speed of change suggested that this was almost certainly due to an increase in inhibition. Cook<sup>3</sup> took his observations to the neurologists in his hospital and later to the neurological societies of New York. They refused to even investigate this. I knew the chief of neurology at Cook's hospital and he told me that the general consensus was that, although Cook's<sup>3</sup> results were remarkable, 'these things just don't happen'. Subsequent studies by myself and colleagues<sup>6</sup> (summarised in *Spinal Cord Dysfunction Volume III*) demonstrated, for the first time, recordable and reproducible neurophysiological changes in patients at spinal and brain-stem levels with epidural electrode SCS and eventually led to the formation of the International Neuromodulation Society.

What Cook<sup>3</sup> had observed was no more than that reported by Frolich and Sherrington<sup>7</sup> in 1902: after decerebration in cat, dog and Macaque, stimulation of the lower thoracic and lumbar region of the spinal cord showed '...an effect...constant and regular...evoked marked inhibition of the rigidity...'.  
The convention of prior acknowledgement is important, as well as being courteous. Correct referencing demonstrates that the authors have read widely in their subject, indicates support for their hypothesis and adds credibility to their work.

Cook<sup>3</sup> deserves much greater recognition.

### CONFLICT OF INTEREST

The author declares no conflict of interest.

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- 2 Melzack R, Wall PD. Pain mechanism: a new theory. *Science* 1965; **150**: 971–979.
- 3 Cook AW, Weinstein SP. Chronic dorsal column stimulation in multiple sclerosis. Preliminary report. *N Y State J Med* 1973; **73**: 2868–2872.
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- 7 Frolich A, Sherrington CS. Path of impulses for inhibition under decerebrate rigidity. *J Physiol* 1902; **28**: 14–19.