The most contemporary and authoritative review of psychiatric practice in this field[2] strongly endorses the use of ECT for the management of refractory Parkinson's disease, citing numerous references from the neurology and psychiatry literature in support of this endorsement. Many psychiatrists who administer ECT are aware of this literature.

I would appreciate the authors' comments on the available evidence for the effectiveness of ECT in Parkinson's disease. If warranted, ECT should then be given its appropriate place in the treatment algorithm for this illness.

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References

1. Guttman M, Kish SJ, Furukawa Y. Current concepts in the diagnosis and management of Parkinson's disease. *CMAJ* 2003;168(3):293-301.
2. Task Force on ECT (Weiner RD, chairperson). *The practice of electroconvulsive therapy: recommendations for treatment, training and privileging.* 2nd ed. Washington: American Psychiatric Association; 2001.

[One of the authors responds:]

Although we did not mention ECT in our article,[1] we agree that it may have a role in the treatment of specific symptoms of Parkinson's disease.

Parkinsonian patients who are severely depressed and whose condition is refractory to antidepressant therapy are candidates for ECT to treat their depression. Patients with drug-induced psychosis that is resistant to atypical neuroleptic medication who cannot tolerate reductions in their antiparkinsonian medication may also be candidates for ECT. However, ECT should not be offered to patients with dementia because there is the potential that such treatment may cause worsening of cognition and may induce delirium. There is insufficient evidence to suggest that motor symptoms related to Parkinson's disease should be treated with ECT, and in our opinion this should not be considered an indication for its use.

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Reference

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The return of "negative" trials?

I was surprised that several important issues were not addressed in the original reports[1,2] and editorial[3] about rate versus rhythm control in atrial fibrillation published in the *New England Journal of Medicine*, or in the review[4] and editorial[5] published subsequently in *CMAJ*.

The Atrial Fibrillation Follow-up Investigation of Rhythm Management (AFFIRM) investigators found no statistically significant difference between rhythm control and rate control.[1] However, one cannot rule out the possibility of a type II error, given that a sample size of 5300 was planned[6] but only 4060 patients were enrolled in the study.

In the noninferiority study by Van Gelder and associates,[2] the efficacy of rate control was within the upper bound of the 95% confidence interval of that of rhythm control. However, 3 concerns must be addressed.

First, it is not clear if the rhythm control strategy is a suitable active comparator. Neither the authors nor the practice guidelines cited[7] provided details on any earlier trials that showed rhythm control to be consistently better than placebo. Thus, it is not possible to assess the similarity of the current trial to those earlier trials, the expected effect size of rhythm control relative to placebo[8] or the consistent responsiveness to rhythm control of the composite endpoint components[9] used in the current trial.