Does STN-DBS really not change emotion recognition in Parkinson’s disease?

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Dear Editor,

A recent publication in *Parkinsonism and Related Disorders* concluded and titled that “*STN-DBS does not change emotion recognition in advanced Parkinson’s disease*” (1). We read this paper on the emotional effects of subthalamic (STN) deep brain stimulation (DBS) with considerable interest, but wish to express three points of concern with respect to i) the interpretation of the background literature, ii) the clinical characteristics of the patients included in the sample, and iii) the experimental design and emotional tasks, which strongly limit the inferences that can be made from its results.

Our first point relates to the *rationale* of Albuquerque et al.’s study, which is based on the authors’ *interpretation of previous findings in the literature*. In their Introduction, Albuquerque et al. claim that emotional disturbances following STN DBS have only ever
been observed in PD patients “shortly” after surgery, and that these emotional disturbances were “potentially related to behavioral correlates”. Although this is true for the patients included in Péron et al.’s (2) and Dujardin et al.’s (3) studies, the mean interval between surgery and the emotional assessments in the other studies cited in this part of the Introduction was 17.7 months ($SD = 16.2$ months, range = 1-72 months) which is not what we would describe as *shortly* after surgery, especially bearing in mind that in Albuquerque et al.’s own study, the interval was just 12 months. In addition, regarding the potential relationship between emotional disturbances following STN-DBS and secondary clinical variables, contrary to what these authors claim in their Introduction, none of the studies that included these analyses reported any significant correlations between recognition of (facial and vocal) affect and the tests included in the general neuropsychological and psychiatric battery, age, level of education, or disease duration (for a detailed review, see 4).

Our second point relates to the *patients’ clinical characteristics*. We note that there was no significant change in either the UPDRS or the Hoehn and Yahr scores between the pre- and postoperative conditions, suggesting that surgery only brought about a relatively minor improvement in the motor state of this subset of patients. These results raise questions about the whole efficacy of the surgery in these patients and/or the location of the DBS electrodes; an important point (because emotional effects are critically determined by the exact position of the electrode stimulation contact (4)) not touched on in the paper.

Our third and final point concerns the *experimental design and emotional tasks*. We noted several major drawbacks, notably regarding the sensitivity of the emotional tasks used, which dramatically limit the inferences that can be drawn. First of all, as underlined by the authors themselves, the number of stimuli presented to the patients was critically low and probably not sufficient to obtain the variance needed to guarantee the correct use of the statistical tests chosen in the study. Each patient was presented with a total of 62 stimuli in the three
emotional tasks they performed (n = 22 for the discrimination of emotional faces, n = 22 for the naming of emotional faces, and n = 18 for the naming of emotional prosody; the discrimination of identity and prosody are not emotional tasks). Second, the discrimination tasks seemed to be very easy to perform, as confirmed by the high percentage of correct responses provided by the patients, raising the possibility of a ceiling effect. Such an effect would hinder the observation of any emotional modifications between the pre- and postoperative conditions. Third, categorization and forced-choice tasks (naming of emotional faces and emotional prosody) are far less sensitive than visual (continuous) analog scales to emotional effects, chiefly because they induce categorization biases (5). This is precisely what was observed in a previous study exploring emotional prosody recognition following STN-DBS in PD (6). In this study, the authors failed to find any significant difference between the pre- and postoperative conditions for the categorical judgments (like those used in Albuquerque et al.’s study). However, when they proceeded to a second level of analysis, involving the investigation of continuous ratings on both target and nontarget scales, the authors were able to probe the data in greater depth, and results seemed to confirm the poor sensitivity of the categorical judgment methodology. For example, these contrasts revealed that, compared with pre-operative patients and healthy matched controls, the postoperative group rated “happiness” more intensely when they listened to fearful stimuli, and rated “surprise” significantly more intensely when they listened to angry or fearful utterances. Albuquerque et al. touched on these results in their Discussion, explaining that previous studies reporting significant results following STN-DBS were obtained when “more complicated judgments of intensity of emotions were considered”. We would argue that these judgments were not “more complicated” but simply more sensitive, and that Albuquerque et al.’s methodology was not sensitive enough to detect the slight emotional impairment that seems to occur following STN-DBS (4). Finally, the lack of counterbalancing between the
emotional tasks used in the pre- and postoperative conditions is highly problematic, as it means that a learning effect could have masked the potential emotional effects of surgery.

Based on the concerns expressed in this letter, we believe that Albuquerque et al. only failed to reject the null hypothesis, which is very different from demonstrating that “STN-DBS does not change emotion recognition in Parkinson’s disease”, and the latter conclusion should therefore be viewed with caution.
References


