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Surgery for Aggressive Behavior Disorder

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We read with interest the article published in the last issue of *Stereotactic and Functional Neurosurgery*, entitled 'Bilateral anterior capsulotomy and cingulotomy applied to patients with aggressiveness' [1]. The authors report on a retrospective series of 10 patients treated with combined stereotactic bilateral cingulotomy and anterior capsulotomy for the treatment of refractory aggressive disorder. This is the largest series in the literature of patients treated for such a condition. The authors found that aggressiveness and functioning, as measured by the Overt Aggression Scale and Global Assessments of Functioning, respectively, were improved at 6 months in 10 patients but not in the long term in the 4 patients followed up for 4 years. The procedure allowed some of the patients to be reintegrated into certain social situations.

Although aggressiveness disorder, albeit difficult to define, can be a debilitating condition with few good solutions, we are

very concerned with the message this report sends to the neurosurgical community regarding (1) the surgical treatment of aggressiveness disorder and (2) the standards in scientific and ethical rigor in the study of neuromodulation of mood disorders.

We believe it is premature to be performing bilateral, ablative, irreversible lesions in these patients with aggressive behavior disorder. There are no studies, and no rationale in the literature, neither in humans nor in animals, that have shown the efficacy of capsulotomy and cingulotomy in the treatment of aggressive behavior except for a highly questionable publication [2]. Regarding cingulotomy alone, the same can be said, except for a publication from a congress proceeding in 1970 in which the authors mention 5 'poor results' in 10 patients [3]. The combination of these two types of surgeries, as is the case in this study, has never been described in the treatment of aggression.

We also read with interest the previous report by the same group published 1 year ago of very similar structure [4] and content to the present article [1]. That previous paper reported on a series of patients operated on for the same condition, with the same procedure, by the same surgeons, at the same institution, during the same time interval. We were surprised that there is no mention of this previous work in the present paper. We invite the authors to clarify several significant discrepancies observed between their previous report [4] and the current article published in *Stereotactic and Functional Neurosurgery* [1].

The initial population diagnosed with a 'neuroaggressive' disorder during the same period (1997–2004) decreased from 25 to 23 from the first to second publication (fig. 1). What happened to

Fig. 1. Differences in the patient populations of both studies by Jimenez et al. [1, 4].

Fig. 2. Complication profile of both studies by Jimenez et al. [1, 4].

Cir Cir 2011			Stereotact Funct Neurosurg 2012	
Adverse effect	Frequency		Side effect	Frequency
	n	%		
Hyperphagia	4	31	Hyperphagia	4 (40%)
Somnolence	3	23	Somnolence	3 (30%)
Disinhibition	2	15	Disinhibition	2 (20%)
Hypersexuality	2	15	Hypersexuality	1 (10%)
Infection	1	8	Infection	1 (10%)
Paraparesis	1	8	Death	1 (10%)
Total	13	100		

Five patients (41.6%) presented on a transitory basis (1 day to 4 weeks). Note that in 80% of patients who reported complications, hyperphagia was a recurrent symptom. No severe adverse effects were seen.

Unfortunately, 1 patient died as an outcome of anesthetic hypersensitivity complications before stereotactic surgery. We also found that in 80% of the patients who reported complications, hyperphagia was a recurrent symptom.

the 2 omitted patients? Four men who were operated on and who appeared in the 2011 paper [4] were not reported in the present paper (6 instead of 10), while 2 new women operated on were added (4 instead of 2). Also, regarding complications, despite the fact that only 4 patients are common to both publications, we see that the number of binge eating, drowsiness, disinhibition, hypersexuality and infection remains, nevertheless, exactly the same (fig. 2). We invite the authors to clarify these discrepancies for the readers.

In a study on neuromodulation of the mood and mind disorders, such as this one, scientific rigor is essential. We encourage the authors to publish the size and anatomical location/distribution of capsulotomy/cingulotomy in all 10 of their patients rather than just 1 representative image (fig. 3). Although the authors suggest that all lesions were correctly located, the right-sided capsulotomy lesion in the illustrative CT scan image seems to be located far lateral to the anterior limb of the internal capsule. Also, we encourage the authors to address the significant selection bias in their study. It would be preferable for the authors to provide information regarding the 6 patients who could not be assessed at 4 years. Were they lost to follow-up? Did they survive? The results are possibly biased and skewed towards a positive result bias, as poor outcomes could be lost to follow-up. We also encourage the authors to report the current psychotropic treatment of the 4 patients evaluated at 4 years, as this may affect the interpretation of the procedure-related efficacy.

Ethical rigor is also integral to any study evaluating the psychosurgery of mood and mind disorders like severe neuroaggressive disorder. Although the ethics committee of their establishment agreed to the protocol and procedure in each case, it would be important for the authors to clarify if psychiatrists independent of their team performed the recruitment of patients, and agreed to the indication of surgery and postoperative evaluation. As Jimenez et al. [1] correctly state, obtaining an informed con-

Fig. 3. Example of bilateral capsulotomy in which the right-sided lesion seems to be located lateral to the anterior limb of the internal capsule.

sent to perform this type of intervention is essential. Because of the 'neuroaggressive' disorder, consent could not be given by the patients and was obtained by their family. This transfer of consent raises the question of a possible conflict of interest and therefore may threaten the principle of patient autonomy. This is even more crucial in the context that the patients have undergone, for economic reasons, an irreversible lesional technique rather than a

reversible deep brain stimulation technique, which may have been more appropriate in this setting.

Surgical treatment of aggressive disorders and addiction is undoubtedly among the most controversial topics today in medicine. Its possible social and political implications make it an issue for which public opinion is, suitably, extremely vigilant. One may recall the scandal created by the publication of the polemic *Violence and the Brain* by Mark and Ervin in the 1970s [5]. In recent years, because of the reversible nature of deep brain stimulation, psychosurgery has earned a second chance as a hope for patients with severe psychiatric disorders. This new, highly scrutinized, therapeutic hope is made possible because researchers now follow strict scientific rules, which was not always the case in the past [6]. For this last reason, we must be very vigilant about the transparency and scientific and ethical rigor of new studies involving surgical treatment of disorders of the mood and mind [7]. If patients with pharmacoresistant aggressive disorder are to benefit from neuromodulation procedures in the future, it is imperative that evidence of safety and efficacy come first from basic research in animal models and well-designed, prospective, preferably randomized clinical trials.

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