

Oponentský posudek na dizertační práci MUDr. Ireny Holečkové Kognitivní kapacita u pacientů s poruchou vědomí/ Cognitive capacity in unconscious patients.

In the Introduction, the principles of evoked potentials are described. The specificity of stimulation by using the patient's name is properly described. The rationale for using this stimulation with comatose patients is presented. This part of the paper is comprehensible, with a good overview of the current literature.

p.8 (on peut associé ces pics astructures nerveuses bien précises): This is a quite optimistic notion. This is more true for early EP than for long latency EP.

p.18 (Générateurs impliqués...): In contrast to PET, the recent intracranial EP studies are not cited.

p 35: In the context of this evoked potential study, the definition and discussion of consciousness should also include electrophysiological studies using subliminal stimuli.

p.39: The definition of coma by Plum and Posner is still appropriate, but why avant-gardiste?

p. 41: What is meant by "Cairns xxx"?

Hypothèse de travail: This is interesting and well explained.

Chapter III:

The article "Brain responses to a subject's own name uttered by a familiar voice" was published in *Brain Research*. The publication in this scientific journal guarantees the high scientific quality of the article. In the context of the presented thesis, it is valuable to start the research by testing the hypothesis on a healthy population. I have a few questions:

I wonder whether the comparison with the novelty P3 is appropriate for the protocol used in this study. In fact, the whole procedure was passive. No active response was required from the patients. From this point of view, the deviant stimulus was also "novel". The attention of the

patients was not focused on the deviant stimulus as in classical novelty protocol with a target stimulus and a distractor.

Moreover, the frequency of the appearance of the deviant stimuli and the SON was different. How did this fact influence the obtained results?

It is interesting that the difference between the familiar and unfamiliar voice appears so late. Could the author explain this?

The difference between the FN and PN is interesting; nevertheless, the localization of generators based on mathematical analysis of scalp recording has been largely speculative. The intracranial studies should be taken in account and discussed.

Chapter IV is a logical follow up to the previous chapter.

The studied patients, the methods and the results are carefully documented. Why was the stimulation by familial voice not tested?

The authors are right when suggesting studies with more patients for confirmation of their results; nevertheless, the number (25) of studied patients in the present study is relatively high. I think that the discussion and the conclusion might take a bolder stance in pointing out the clinical utility (or its absence) of ERP in certain comatose patients.

Was this study suggested for publication?

p77: "Les PEATC et les PEALM ont été enregistrés selon une technique décrite antérieurement (Fischer et al. 1994)."

A thesis should always include a detailed description of the methods.

Chapter V includes another article that was published in *Brain Research*: "Subject's own name as a novel in a MMN design: A combined ERP and PET study." Again, I have to state that the publication in this scientific journal guarantees the high scientific quality of the article. Several questions that I would have were already posed above.

Chapter VI: L'activation cérébrale en réponse à la déviance de durée, au propre prénom du sujet et à la familiarité de la voix chez les patients inconscients mesurée par des potentiels évoqués cognitifs et le débit sanguin cérébral.

This chapter provides the rationale for the whole study. First, a detailed overview of the literature about the electrophysiology as well as metabolic studies in comatose patients is presented. An original study combining the techniques used in the authors' previous studies in comatose patients follows. The studied group is small. The results are very interesting, though it is questionable whether more clinically useful information could be obtained even with larger studies given the heterogeneity of the cerebral lesions in patients in a vegetative state.

The study of akinetic mutism is performed in only two patients.

As the results are different, it is very hard to conclude anything from this part of the study. It is true that the akinetic mutism is a rarely-occurring condition and it would be not easy to compose a larger study.

The general conclusion is acceptable.

Conclusion:

I am really impressed by the logical construction of this thesis. The thesis develops an idea from the formulation of a hypothesis based on a critical review of the literature through testing of healthy subjects, testing on patients up to a combined use of electrophysiological and of metabolic studies for explaining the obtained results. The high scientific quality of the thesis is expressed by the publication of two articles in Brain Research. The general standard of this thesis is high. The thesis is a good example of the usefulness of international co-operation.

I am pleased to recommend this thesis for the acquisition of the title Ph.D.

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