Reviewer’s report

**Title:** Prognostic Value of Cortically Induced Motor Evoked Activity by TMS in Chronic Stroke: Caveats from a Revealing Single Clinical Case

**Version:** 1  **Date:** 19 February 2012

**Reviewer:** Heidi Schambra

**Reviewer’s report:**

The authors’ efforts to gain additional information are noted, and appropriate scientific rigor has now been accomplished. Well done. I still have some significant concerns that must be addressed prior to publication of this manuscript.

The major compulsory revisions are as follows:

1. There are numerous grammatical mistakes throughout the new additions, which at times are nearly incomprehensible. Please address.
2. The clinical description section could benefit from a clinical-language proofreading:
   a. “…64 year-old man …”
   b. “… a score of 3/5 FOR right shoulder … hand strength (singular)…”
   c. “paresis” means weakness and is therefore incorrectly used here, should be rather “… 0 for complete paralysis …”
   d. “MILD paresis of the right LOWER limb”
   e. The patient doesn’t report signs; signs are observed by a clinician. Therefore, the “patient was observed by the neurologist to have right-sided supranuclear facial paresis.”
   f. “DEEP tendon-reflexes were found to be more pronounced and BRISK …”
   g. “Babinski SIGN” not reflex
   h. “epileptic seizures …, NOR (not neither) … which could have interfered WITH …”
3. Pg 6: clinical tests – please explicitly state that the clinical tests were performed only once.
4. It is stated (pg 8): “The FCR and MBB were added to the protocol in order to rule out … failures in peripheral nerve degeneration … etc” However, you rule out these pathologies with peripheral nerve testing/CMAPs, so this rationale no longer makes sense. It would be sufficient to state that these muscle representations were examined to explore differential recovery processes for more proximal muscles (if this was indeed your rationale).
5. “The lack of TMS evoked MEPs found from the affected (left) hemisphere for the contralesional (right) FDI and FCR muscles can be made extensive to an arm
muscle such as the biceps brachii (MBB).” I don’t understand what this statement means. Please clarify.

6. “Similarly, the activation of the affected (right) hand during both grasping and pinching evoked EMG activity only in the FDI muscle and eventually for grasping also the FCR muscle of this very same (right) limb, whereas EMG signals recorded in homologue muscles of the opposite affected (left) hand remained again at baseline levels.” I don’t understand what this statement means. Please clarify.

7. Pg 7: you are recording from FDI, FCR, and BB, yet only FDI and BB seem to be the only ones mentioned for the neurophysiological testing in the methodology section (‘both muscles’ is often stated). In the results, FCR again makes an appearance. Please clarify.

8. Pg 11, 17: “LC is the latency of the D-wave …” D and I waves are not detectable by standard surface EMG recordings. Please change to “latency of the ONSET of the MEP …”

9. The authors do not appear to understand my concern that the data they are reporting as similar (frequency of pronation-supination of the affected vs unaffected limb) are quite dissimilar in their actual results. Especially if the patient is very consistent in his movements, as the authors suggest, running very simple statistics is appropriate and expected. Why the resistance here? While the deletion of the “meaningless” qualifier is heading in the right direction, reporting this result as a ‘slight difference’ is still unacceptable. A simple t-test on the pronation-supination frequency data gives me values of: t=5.1, p = 0.0068. This finding is consistent with the idea that the patient is still mildly impaired in some movements, and would not negate the other results reported. The authors should keep well in mind that their credibility and objectivity are called into question when they state that a difference is ‘meaningless’ or ‘slight’ when it clearly is not. Please change the language and report the simple statistics.

10. The discussion of the clinical data in the discussion is nearly absent. It would be worth a small paragraph to give a quick summation of your results. Here is where the authors can add in the justification of using the NIV over frequency as the better estimate of impairment (as described in their response to me and Reviewer 2).

Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:
I declare that I have no competing interest