Reviewer’s report

Title: Cognitive rehabilitation in Ugandan children after severe malaria: effects on cognition, academic achievement and behaviour

Version: 1 Date: 14 March 2011

Reviewer: Etienne de Villers-Sidani

Reviewer’s report:

Paul Bangirana et al. reported in this manuscript the impact of a computer-based cognitive training strategy on several measures of cognition in sixty-one children recovering from severe malaria. The authors should be lauded for their initiative, which addresses both a devastating illness and a burgeoning and promising new field in neuro-therapeutics. The manuscript is well written and easy to follow and appears devoid of significant methodological/statistical flaws. While having a larger study would have helped draw stronger conclusions, the group sizes in this pilot study were sufficient to evidence some differences between the experimental and control groups. I do have major reservations however on how the data was interpreted and on some conceptual aspects of the study. My concerns and suggested revisions are suggested below.

Major compulsory revisions:

1. The main problem I have with the manuscript is that the authors seem to think that their findings (or lack of findings) generalize to ALL computerized cognitive rehabilitation strategies. Their conclusion is that “computerised cognitive rehabilitation three months after severe malaria had a short-term effect on cognitive outcomes but did not appear to affect short-term academic achievement or behaviour”. Hundreds of animal and human studies (for short reviews see for example Mahncke et al., 2006, Prog Brain Res, 157, 81-109 or Tallal et al., 1998, Scand J Psychol, 39, 197-9) clearly show that for behavioral training strategies to achieve benefits they have to be carefully designed to specifically address the underlying perceptual/cognitive deficits. In other words, these approaches have to be considered exactly like a drug or medication. In that context, the conclusion of the authors in this manuscript is akin to stating “all medications (current or future) three months after severe malaria had a short-term effect on cognitive outcomes but did not appear to affect short-term academic achievement or behaviour”. It is of course not possible to make such sweeping generalizations. What adds to the problem is the fact that it is not even clear that the training strategy used here is specifically targeted to improve the main cognitive deficits experienced by these children (and what are the main impairments experienced by them in the first place). In my opinion, the only possible conclusion from this study is that whatever subset of the “Captain’s Log software” they used “had a short-term effect on cognitive outcomes but did not appear to affect short-term academic achievement or behaviour.”
2. As introduced in the point above, I believe it is absolutely crucial that more information be provided on the actual training the children underwent. Without that information, it is impossible to reproduce these results or perhaps more importantly, attempt to understand what type of computer-based training does NOT work in these children. Some attempt to link the principles behind that training strategy with the deficits encountered by the children would also be necessary in my opinion. Again, think of it as a drug, there should be some rationale for using it this particular condition. Would it make sense to treat heart failure with an anti-epileptic? Or, conceptually more similar, to what extent would practicing arithmetic help for the remediation of dyslexia? Without a rigorous approach to cognitive training, it will be very difficult for this new field to gain any traction and credibility.

3. The outcome measures and their relevance should be more explicit. The main effects of the training strategy in this study were on “learning” and “working memory”. “Learning” can mean dozens of different things. A more thorough discussion of what exactly is being measured will also be very helpful to understand what could have worked/not worked with this particular training strategy.

Minor essential revisions:

1. On page 5 first line, the authors write that “cognitive deficits after MNI get more severe with time”. A reference would be needed for this. And if the authors really meant that the gap between the cognitive age vs actual age of the children is widening, I would suggest reformulating the sentence, as it would be misleading as it is. Are the cognitive performances really worsening (raw scores) or are these children more and more behind in their cognitive performance? Both scenarios would have very different implications. Depending on the authors’ response, the sentence found shortly after in the text “This implies that interventions done long after the illness may have little benefit since the severity of cognitive deficits seems to worsen as the child matures” might have to be removed or changed.

2. On page 6, the authors indicate that 15 children had withdrawn from the study or been lost to follow up. Is it known why they withdrew? Were they doing better? Is it conceivable that they formed a homogeneous group that would have been more likely to respond to therapy? I realize that the answer to this might not be known.

3. Page 8, in the paragraph concerning the WRAT-3, the last sentence is redundant.

4. Based on the WRAT-3 results the authors conclude that the training strategy “did not appear to affect…academic achievement”. Could the authors indicate whether this test can really be used as a predictor of academic achievement? This test seem to correlate with past academic achievement and it is not clear to me that retaking the test 3 months later, considering a possible practice effect
would allow to make such a conclusion.

5. Page 8, the authors explain that attention deficits are common in children recovering from severe malaria. It would be important to note/discuss somewhere that attentional difficulties will cloud all other cognitive measures and that a training strategy failing to address this difficulty would have a high risk of failure. It is also unclear to me how results on the KABC-II and WRAT-3 should be interpreted in the context of severe attentional benefits.

6. Table 2 and 3, I believe that actual scores and not only differences between experimental and control groups would have been helpful. These would help decide whether cognitive performance really decreases with time in these children and could show important differences between the two groups.

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I declare that I have no competing interests