Reviewer’s report

Title: Cytokine profile, proliferation and phosphorylation pattern of ERK1/2 and Akt in circulating mononuclear cells from individuals with chronic intestinal phase of Schistosomiasis mansoni

Version: 1 Date: 17 June 2012

Reviewer: Quentin Q Bickle

Reviewer’s report:

This is a well conducted study which contains as significant body of work. The main original component is measurement of phosphorylation of ERK1/2 and Akt in PBMC from S. mansoni infected donors, non-infected endemic area donors and naive humans in response to recall stimulation with SWAP and SEA. This showed essentially no difference between the groups. The remaining studies represent valuable information necessary to provide the context of phosphorylation experiment but these observations are not novel. Nevertheless, the work is solid and worthy of publication. However, there are a few issues which the authors should address before the manuscript is considered further.

Overall:

The script has not been carefully read. The English needs attention in terms of meaning, spelling and grammar. Below are a few examples from the Abstract but there are many other in the manuscript.

line 2 “...host liver and is modulated...”
line 5 “...response than in acute infection....”

Lines 5-8: “To study..... endemic area”. This is not a sentence, it doesn’t have a verb.

Line 10-13 : I suggest: “When compared with the BD group, CD4+ T lymphocytes (remove the “s”) proliferation was lower in the XTO group but not in the NI group in both unstimulated and antigen (SWAP and SEA) stimulated cultures.”. NOTE: there are many places in the manuscript where the definite article “the” is omitted. This should be checked and corrected. “Individuals” could be used sometimes in place of “group” to avoid over use of “group”.

Background:

Lines 1-4. The references given are for the mouse work on modulation of granuloma size and so the, admittedly scanty, evidence for changes to granuloma size in chronic human disease should be cited and described.

Line 5 (and other places). Schistosoma is a genus name and should be in italics.

Line 13 – references cited – in fact ref 2 was perhaps the first to demonstrate this and so should be included here.

Line 16: “..... lower anti-SEA ... response relative to acute patients... “.
Line 15-18: This statement is not entirely correct since in ref 9 responses to both SEA and SWAP were lower in the chronic patients.

Page 4, Lines 14-16 “The two signal....”. A reference for this statement would be helpful and it seems more logical to me for this sentence to be moved earlier in the Background section– to before Page 3 line 25 i.e. before “One of the phenomena......”

Page 4, Lines 21-22: “Such as....” this sentence does not make sense.

Methods:

Page 12: It would be helpful to report the numbers of infected people found in these two communities so that the reader has an idea of the prevalence of infection. Also state the numbers of XTO and NI individuals selected from these two areas. The origin of the BDs is not clear. The Abstract states “... individuals with no prior parasite contact”. If they were simply from an area not endemic for schisto this should be stated. If they are not is should be explained how they were deemed not to have had exposure.

Page 13: lines 2-3: the organs used for recovery of eggs should be stated.

Page 13: Particularly i-gn view of the relatively high production of IFN-g in the BD with SEA and SWAP compared with medium alone (Figure 3c) it would be helpful if endotoxin levels had been established for these antigens and found to be acceptably low for use in these cultures. Also the authors should comment in the Discussion on the possible reasons for this IFN production to SWAP and SEA in the BD group.

Results:

Lymphocyte phenotyping: Although the gating is described briefly in the methods, it would be helpful to show an example of the FACS plots for this data to illustrate the gating.

Can the authors comment on whether MFI showed any differences?

Although the lack of proliferation is consistent with earlier work (Figure 2) were mitogen controls included in the study just to confirm the viability of the different cell populations? If so some reference to this would be helpful.

Figures: The use of asterisks and letters to indicate P values is cumbersome. The authors should consider using lines between groups on the figures themselves. The only significance level reported was <0.05. Lower P values should be reported if observed.

Discussion:

Page 8 Lines 14-16. Can this be said to be a Th0 profile when the IFN response was not reported as significantly increased and furthermore the increase in IFN production with SWAP in BD was even greater?

Level of interest: An article whose findings are important to those with closely related research interests
**Quality of written English:** Needs some language corrections before being published

**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