Reviewer's report

Title: Co-expression of nuclear and cytoplasmic HMGB1 is inversely associated with infiltration of CD45RO+ T cells and prognosis in patients with stage IIIB colon cancer

Version: 3 Date: 14 December 2009

Reviewer: Luigi Laghi

Reviewer's report:

Is the question posed by the authors well defined? NO

2. Are the methods appropriate and well described? No, see my previous comments. Additionally, the answer to previous comment 1 is not satisfactory. Specifically, I do not understand what is the "stroma of cancer center". By such an unclear approach, it would be easy to reach unsafe conclusion using methods that might overestimate differences, like the minimum p-value.

3. Are the data sound? No. Statements concerning intra-sample heterogeneity are vague, and no measure is provided. Stating that fields were added to measurements doesn't make measure better, if experimental data are not provided.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition? Not really, in my opinion.

5. Are the discussion and conclusions well balanced and adequately supported by the data? No

6. Are limitations of the work clearly stated? No

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished? Yes

8. Do the title and abstract accurately convey what has been found? I believe the title is an overstatement

9. Is the writing acceptable? Somehow

Level of interest: An article of limited interest

Quality of written English: Not suitable for publication unless extensively edited

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:

I declare that I have no competing interests' below. If your reply is yes to any, please give details below.