Reviewer’s report

Title: PDGFRalpha/beta AND VEGFR2 polymorphisms in colorectal cancer: Incidence and implications in clinical outcome

Version: 2 Date: 24 August 2012

Reviewer: Torben Hansen

Reviewer’s report:


First of all, the authors should be congratulated with a fine study. I especially acknowledge two parts of the design. The originality of focusing at a specific location of the genes of interest and afterwards testing the possible functional influence of their results based on CRC cell lines.

Having said that there are also a few smaller issues that should be clarified:

Discretionary Revisions

1. The introduction (p. 3 l.14): It could be specified, that bevacizumab targets VEGF-A instead of writing just VEGF especially since the authors later in the introduction decides to differentiate between the ligand subtypes.

2. Material and Methods. The source of DNA for the present study is not entirely clear. The author’s state that DNA was isolated from tissue sections from the tumours and by that must represent tumour-derived genomic DNA which is not necessarily the same as “normal” genomic DNA. Please clarify this.

3. Table 2: According to the results section, the rare allele of the PDGFR# 19 SNP exists in heterozygous as well as homozygous genotypes. Why is this not shown? The 45 patients are grouped together under “SNP”. This could easily influence the presented relationship and especially when near significant results are obtained (the correlation with tumour location) it becomes even more important for the reader to have access to the “raw data” showing the relationship between all three genotypes and tumour location.

Minor Essential Revisions

4. Table 1: Why is tumour differentiation only available for 80 patients? – and in the same context this could very well have an influence on the near significant results presented in Table 2 regarding the correlation between “SNP 13” and tumour differentiation.

5. Table 3: The presented results regarding PDGFR-A13 are a bit surprising. After 37 months (approximately 3 years) 50% of the WT (AA) patients have died and after two more years (5-year survival) 14% is still alive. This seems plausible. On the other hand when one takes a look at data for the SNP (AG)
group results are a bit surprising. 50% have died after less than 2 years (21.7 months) and for the following 3 years only an additional 9% dies? I know that this group only consists of 13 patients but it still surprises be – are these numbers correct?

6. Figure 2. Please specify the reasons for censoring survival data.

**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