Reviewer's report

Title: Intraoperative Electron Radiation Therapy (IOERT) in the Management of Locally Recurrent Rectal Cancer

Version: 1 Date: 20 October 2012

Reviewer: John Plastaras

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Major Compulsory

1. The mix of previously radiated and non-irradiated patients, 2 distinct groups that received distinct treatments. They did not include prior RT as an independent factor in their analysis. It gets buried in with the grouping (EBRT or not) and (neoadjuvant EBRT or not). If the tumor recurred after prior RT, I think that is a significant difference in terms of the expected biology and the expected response from neoajuvant EBRT or IOERT. This should be separately analyzed.

Minor Essential Revisions

2. The use of the term "central control" is confusing. What they are describing is within the IOERT area, which sounds more like what I would call "local control." They use the term "local control" to describe pelvic control, which I would call "regional control." These should be changed to "loco-regional control" and "local control" as a more standard terms. The definitions are only found in the methods, so those reading only the abstract are likely to have a hard time interpreting the results.

3. There is at least one important reference that was not included from MDACC (Das P et al IJROBP (2010) 77:60-5.) Here, they reported pts who had re-rradiation and surgery had a 3 yr OS of 66%, which is a bit better than the reported 3 yr OS of 52% (although the number of resected pts was lower in that series). In the decision tree, a patient who has had prior radiation and a potentially resectable cancer, which is the better route? External beam re-RT and surgery (as described by Das et al) or the current series of resection IOERT. This type of comparison is missing in the discussion, despite the authors point that re-irradiation with external beam is probably the best course of action.

4. With regards to the statement that that doses over 60 Gy result in excessive small bowel toxicity should probably reference direct data. A better reference would be Mohiuddin et al IJROBP 1997 39: 643 (which used lower doses for re-irradiation, but did report s bowel toxicity).

5. I fundamentally disagree with the statement that there is a higher biological effectiveness of a large single dose (referenced 15). This is simply not true -- fractionation has been shown in fundamental radiobiological experiments to be more effective than single doses. A single fraction is a necessity with IORT, but
radiobiological effectiveness is not one of the reasons. This statement should be altered.

6. Should more carefully define all abbreviations in tables.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**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'