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Editorial to the Paper of Ute Linz

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It is good scientific practice to critically review treatment modalities which have been applied for years on a theoretically sound fundament, but with less clinical success than originally anticipated. A good historic example is the hyperbaric oxygen therapy once introduced to improve the radiotherapy of tumors with a significant amount of hypoxic cells. Enormous technical efforts were made at that time to oxygen pressure chambers to irradiate patients in an oxygen enriched atmosphere. This treatment approach emanated from *in vitro* and animal studies demonstrating better survival when reducing the radiation resistant hypoxic tumor cell fraction.

Another story is the radiation therapy with cyclotron-generated neutron beams. Mainly due to toxic effects in normal tissue, they were finally recognized not being worthwhile to pursue.

Albeit the many short-term innovations in medical oncology, in cancer treatment new modalities appear and disappear every now and then. But isn't it the essence of science to create new ideas, to translate them from the lab to bedside and to evaluate pros and cons, *i.e.*, benefits and risks for the patient? However, there is no other way for validating innovations than standardization of methods, quality assurance of procedures, and evaluation in clinical trials.

In this issue, a scientific commentary on the present status of BNCT of brain tumors (gliomas) by Dr. Linz is published, which I enjoyed reading. No doubt, it is very discerning, probably sometimes a bit provocative, at least for fervent advocates of BNCT. But isn't it time to pause for a moment and look back on the 50 year old story of BNCT?

Are there at least proven results of BNCT in glioma treatment (mind, it is just BNCT of gliomas which is debated in Dr. Linz' paper) or is it still a kind of ultima ratio method in the treatment of this particularly disastrous tumor type? Shall we continue the BNCT route and recommend it to our glioma patients or are there equivalent alternatives, which are more cost-effective? All these and more questions on BNCT of gliomas are addressed in Dr. Linz' commentary.

I very much appreciate the editor's decision to accept this paper for two reasons: firstly, the author challenges clinicians, physicists, and biologists to carefully consider her arguments why BNCT of gliomas is not a breakthrough and – as she claims – most likely will never be. She underlines her view with facts, arguments, and a highly valuable collection of data on BNCT from relevant literature. Secondly, Dr. Linz is right in requesting from the BNCT community improvements in standardization, quality assurance, and clinical trials. The principles of evidence based medicine are to decide whether BNCT should be pursued or not.

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