Letter to the Editor

Prostatectomy vs radiation therapy for prostate cancer: The (still) unsolved dilemma

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Recently, Wallis et al. published a meta-analysis that suggests the superiority of prostatectomy over radiation therapy (RT) for clinically localized prostate cancer [1]. The topic has, for a long time, been of crucial interest in uro-oncology. Unfortunately, this meta-analysis shares the same methodological limits of other meta-analyses having addressed this same issue in the past.

The Newcastle-Ottawa Scale (NOS) [2] used for the preliminary evaluation of the included study's bias could not be considered adapted, as a threshold score distinguishing between 'good' and 'poor' quality studies is still lacking in NOS, as is clearly stated in the NOS website [2]. Thus, the initial criteria, even if reasonable, introduce a systematic bias in the preliminary selection of the studies.

In Table 1 in the study of Wallis et al., authors summarize the study's characteristics: it is clear that it is impossible to conclude anything about the effectiveness of RT when 11/19 studies give no information about the total delivered dose, a factor which is known to influence the outcome [3]. Anyway, looking at enrollment periods, it is very likely that the total delivered doses would nowadays be considered insufficient. Moreover, the criterion “before vs after 2005” cannot be a surrogate of the dose levels, as only recently we acquired the strong evidence of the impact of dose escalation on treatment outcomes, based on randomized trials.

More than 80% of the data are taken from registries: their importance in driving the therapeutic choices of physicians is well known, as are the potential major limits of the data quality within them, a factor which strongly influences the final quality of the interpretation of the results.

Randomized and retrospective studies already showed the impact of adjuvant and/or salvage RT in improving disease free survival and overall survival [4,5]. The information on the proportion of patients having received RT after prostatectomy is lacking in this meta-analysis: this is another important limit of the studied data, and hence of the results.

Finally, this meta-analysis presents a clear confounding by severity bias: likely, in the considered patients, the severity of the disease acts as a confounding factor, as prostatectomy will likely be proposed easier to patients with a good performance status and/or earlier stage disease [6].

In conclusion, any analysis aiming at obtaining results based on weak data will be affected by major methodological bias, leading to a correspondently weak conclusion. Only the ongoing prospective randomized trials will answer the yet unsolved question addressed by the study of Wallis et al. In just a few months, the British ProtecT randomized trial (NCT02044172) will report on large numbers of men managed by high quality surgery or radiation therapy or by active surveillance, with a median follow-up of over ten years. Whatever the ultimate results of that trial, it will have far greater meaning and influence, making this meta-analysis completely redundant. Until that point in time, RT +/- hormonal therapy should be considered a valid first-choice curative option in the therapeutic approach to prostate cancer.

References