Conflict of Evidence

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Reply to Reviewer Comments

Reviewer #1: Thank you for the opportunity to re-review this paper after revisions. Again, Sylvester et al are to be congratulated for this remarkable contribution to European Urology.

The authors have not indicated on their revised manuscript where the revised changes are, making it difficult to find them. Overall, I believe the authors have sufficiently addressed Reviewer #1’s suggestions/comments.

Reply: Thank you. Both the text of the revisions and the line numbers where changes were made to the manuscript were included in the reply to the reviewers.

However for Reviewer #2, there are some inadequate responses from the authors.

Reviewer 2 point 4

- I believe that the authors should include a discussion on other types of meta-analyses, such as diagnostic test accuracy (e.g. PMID 27363387), prognostic factors (e.g. PMID 25559810), and even that of retrospective studies (e.g. PMID 24680361).

- Again, despite what the authors feel about observational data, these represent real-world comparative effectiveness data that are typically of the patients we treat and therefore such data is practical, useful and believable to us as clinicians.

- Additionally, for some rarer diseases such as UTUC, there just are not any RCTs and the best level of evidence will be a meta-analysis of all available retrospective studies.

Reply:

There are 6 important areas to consider when evaluating the validity and risk of bias in studies of prognostic factors (QUIPS): study participation, study attrition, prognostic factor measurement, study confounding, outcome measurement, and analysis and reporting. In order to minimize the risk of bias, prognostic factor studies to be included in a meta-analysis should preferably be prospective and have a protocol which addresses these topics.

Reference:


For diagnostic test accuracy studies, QUADAS-2 provides a tool for the quality assessment of diagnostic test accuracy studies which comprises 4 domains for assessing the risk of bias: patient selection, index test, reference standard, and flow and timing. Once again, in order to minimize the risk of bias, diagnostic test accuracy studies to be included in a meta-analysis should preferably be prospective and have a protocol which addresses these issues.

Reference:

For prognostic factor and diagnostic test accuracy systematic reviews, we agree with the reviewer that randomized controlled trials are not required, however the individual studies included in the meta-analysis should preferably be prospective in nature and have a protocol in order to minimize the risk of bias.

Since the manuscript deals primarily with discrepancies between intervention RCTs and meta-analyses, including meta-analyses involving diagnostic test accuracy and prognostic factor studies goes beyond the scope of the paper, notwithstanding the impact on the word count. Nevertheless, we have, as indicated below, added a sentence concerning them to the Discussion.

We do not agree with the reviewer, however, that non-randomized comparative studies (whether prospective or retrospective) or observational case series should be included in meta-analyses of interventions because of the high risk of bias. Included in a qualitative systematic review, yes, but not included in a quantitative meta-analysis. The reasons for this position have already been outlined in our previous responses and are further discussed below. While we accept that some referees might have a different opinion, as a guideline authority we believe that this is an extremely important principle to uphold.

For non RCT intervention effectiveness systematic reviews, one should present the results of the individual studies from a narrative point of view, in descriptive tables or even in forest plots, but the results of the individual studies should not be combined together in a formal meta-analysis to produce the diamond at the bottom of the forest plot.

Although Stroup et al (MOOSE) provide a Reporting Checklist for Authors, Editors, and Reviewers of Meta-analyses of Observational Studies, they state in the Comment:

“The application of formal meta-analytic methods to observational studies has been controversial. One reason for this has been that potential biases in the original studies, relative to the biases in RCTs, make the calculation of a single summary estimate of effect of exposure potentially misleading. Similarly, the extreme diversity of study designs and populations in epidemiology makes the interpretation of simple summaries problematic, at best. In addition, methodologic issues related specifically to meta-analysis, such as publication bias, could have particular impact when combining results of observational studies.”

For example, the paper “Overall Survival Advantage with Partial Nephrectomy: A Bias of Observational Data?” by Shuch et al (reference 48), illustrates our concerns about bias when comparing partial nephrectomy to radical nephrectomy based on non RCT studies:

“CONCLUSIONS: RN patients had similar OS compared with controls, suggesting that this treatment modality does not compromise survival. Patients undergoing PN had improved OS compared with controls, suggesting possible selection bias. The apparent survival advantage conferred by PN in SEER-Medicare case series is likely the result of selection bias involving unmeasured confounders.”

We thus feel that the risk of bias is too high in non RCT intervention effectiveness meta-analyses (where a formal risk of bias assessment of the individual studies isn’t always done) for their conclusions to directly impact on treatment recommendations and guidelines. Most readers will not be aware of their limitations. We believe that it is better to present such results in a qualitative systematic review rather than to run the risk of publishing incorrect or misleading results in a meta-analysis that may steer further research in the wrong direction or adversely impact on patient care.
This is a paper submitted under Statistics in Urology, therefore the reply that "majority of whom do not have advanced statistical knowledge or experience" does not seem to be appropriate or accurate.

Reply: It was submitted under Statistics in Urology for the lack of a better category. The most appropriate category would have been Guidelines, however this category does not exist. The paper is aimed at clinicians and guidelines developers and not at statisticians. In any case, the majority of readers, including those who read articles under the topic of Statistics in Urology, are urologists who do not have advanced statistical knowledge or experience.

This point should be addressed and included in the manuscript, rather than brushed aside, given the substantial proportion of systematic reviews and meta-analysis of not just RCTs, but other types of studies.

Reply: As indicated in the paper’s title, the scope and subject of the paper is to resolve discordant findings between RCTs and meta-analyses. It was not our intent to deal with prognostic factor or diagnostic test accuracy meta-analyses, but only with intervention meta-analyses. Nevertheless, in accordance with the reviewer’s comments, the following modifications been made to the manuscript:

Lines 321 – 323:

It is important to reiterate that combining observational studies in general, and even comparative non-randomized studies with RCTs in an intervention MA, may produce unreliable results and is not considered valid.

In addition, the following text has been added in lines 330 – 333:

Although non RCTs can be included in SRs, we have emphasized that only RCTs should be included in intervention MAs. RCTs are not required for prognostic factor and diagnostic test accuracy MAs, however the studies included in these MAs should preferably be prospective in nature and based on a protocol to minimize risk of bias.

Reviewer #2:

The authors have addressed most of my concerns. While I disagree with some of their responses, like those to questions #3, #4, and particularly #6, I think their responses are well thought out and certainly reasonable. I do feel, however, that these responses sure smell like those coming primarily from individuals that do not treat many patients.

Reply: Seven of the 14 co-authors are urologists who regularly treat patients.

The ITT vs PP analysis problem in my opinion clearly is best managed by presenting both results. How can one argue otherwise (i.e. for a less complete revelation of the data)?

Reply: Unfortunately the risks associated with the results of a PP analysis are not often presented in the paper. Nevertheless, lines 93 - 96 have been modified as follows:

In some RCTs, not all participants receive their randomized intervention; they may, for example, cross-over to the other randomized treatment, in which case a per-protocol analysis may also provide useful information.
Similarly, the hierarchy of evidence is actually not based in evidence! I wonder what would happen if we randomized patients to be treated by statistical robots or by experienced physicians? I bet the robots miss the boat because of the innumerable immeasurables that physicians, and not data-analysts, recognize and utilize. There is a reason some MDs get better results than others, and it is not better access to trial data.

Reply: Yes, quality of results by MD or by institution is an important topic, and variations in outcomes may be linked to pre-existing experience, education, training, one’s innate ability to learn and adapt, institutional support and other elements of the learning curve. See, for example, the conclusions of the following paper:

Take Home Message

New or existing RCT data can lead to conflicts with MA data. In this paper, we present examples of, and explore reasons for, such conflicts. Guidance is provided to guideline developers on how to assess conflicting data in such circumstances to help determine which source is more reliable. For guideline organizations, both within and outside of urology, having a well-defined and robust process to deal with such conflicts is essential to improve the quality of their guidelines.
Conflict of Evidence: Resolving discrepancies when findings from Randomized Controlled Trials and Meta-analyses disagree

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Abstract: 299 words

Text: 3898 words

Keywords: conflict of evidence; meta-analyses; randomized controlled trials; systematic reviews; treatment guidelines
Abstract

Context: Clinicians and treatment guideline developers are faced with a dilemma when the results of a new, large, well conducted, randomized controlled trial (RCT) are in direct conflict with the results of a previous systematic review (SR) and meta-analysis (MA).

Objective: To explore and discuss the possible reasons for disagreement in the results from SRs/MAs and RCTs and to provide guidance to clinicians and guideline developers for making well informed treatment decisions and recommendations in the face of conflicting data.

Evidence Acquisition: The advantages and limitations of RCTs and SRs/MAs are reviewed. Two practical examples which have a direct bearing on EAU guidelines treatment recommendations are discussed in detail to illustrate the points to be considered when conflicts exist between the results of large RCTs and SRs/MAs.

Evidence Synthesis: RCTs are the gold standard for providing evidence of the effectiveness of interventions, however concerns over an RCT’s internal and external validity may limit their applicability on clinical practice. SRs/MAs synthesize all evidence related to a given research question but two urological examples show that the validity of their results depends on the quality of the individual studies, the clinical and methodological heterogeneity of the studies, and publication bias.

Conclusions: Although SRs/MAs can provide a higher level of evidence than RCTs, the quality of the evidence from both the RCT and the SR/MA should be investigated when their results conflict to determine which source provides the better evidence. Guideline developers should
have a well-defined and robust process to assess the evidence from MAs and RCTs when such conflicts exist.

Patient Summary: We discuss the advantages and limitations of using data from randomized controlled trials and systematic reviews/meta-analyses in informing clinical practice when there are conflicting results and provide guidance on how such conflicts should be dealt with by guideline organizations.

Take Home Message

New or existing RCT data can lead to conflicts with MA data. In this paper, we present examples of, and explore reasons for, such conflicts. Guidance is provided to guideline developers on how to assess conflicting data in such circumstances to help determine which source is more reliable. For guideline organizations, both within and outside of urology, having a well-defined and robust process to deal with such conflicts is essential to improve the quality of their guidelines.

Tweets

Clinicians: SRs/MAs theoretically provide a higher LE than RCTs, but their quality needs scrutiny in case of conflict #eauguidelines

Patient summary: High level scientific publications should be interpreted with caution when there are conflicting results #eauguidelines
1. Introduction

The practice of evidence based medicine means integrating individual clinical expertise with the best available external clinical evidence from systematic research [1].

Treatment recommendations in European Association of Urology (EAU) Guidelines are underpinned, whenever possible, by the results of systematic reviews (SR)/meta-analyses (MA) and large randomized controlled trials (RCT). According to the 2009 Oxford Centre for Evidence Based Medicine, SRs of RCTs (with or without a meta-analysis) that are free of worrisome variations (heterogeneity) in results between individual studies provide the highest level of evidence (LE), 1a, whereas individual RCTs with a narrow confidence interval provide the next highest LE, 1b [2]. As SRs can provide a higher LE than RCTs, the results of SRs are generally considered to take precedence when developing treatment recommendations.

The quality of the results of a SR/MA depends on the quality of the included studies. Kjaergard et al [3] found a correlation between methodologic quality and discrepancies in the results of large and small RCTs included in MAs. Intervention effects were exaggerated in small trials with inadequate allocation sequence generation, inadequate allocation concealment and no double blinding.

Discrepancies have also been noted between large RCTs and previously published MAs on the same subject [4-6]. In 12 large RCTs carried out subsequent to 19 MAs addressing the same question, LeLorier et al [7] found that the results of subsequent RCTs results disagreed with those of earlier MAs 35% of the time.
To illustrate these points and provide guidance to guideline developers in dealing with conflicting data from different sources, two examples which have a direct bearing on EAU Guidelines treatment recommendations are presented. In the first example, the EAU Guidelines Office has recently been confronted with the results of a large RCT which found no beneficial effect of medical expulsive therapy (MET) on stone passage, contrary to results of previous meta-analyses which formed the basis for treatment recommendations [8]. In the second example, which compares the efficacy of partial versus radical nephrectomy for localized renal tumors, discordance between the results of the meta-analysis and the only available RCT are investigated [9,10].

2. Advantages and Limitations of Randomized Controlled Trials

As summarized in Table 1, RCTs have a number of advantages and limitations.

Advantages of RCTs

RCTs are the gold standard for providing evidence on the effectiveness of interventions [11-12]. Randomization balances, on the average, the distribution of both known and unknown prognostic factors at baseline in the intervention groups, thereby minimizing selection bias when assigning patients to treatments. Although adjusting for baseline covariates used in the randomization process can improve statistical power, complex adjustment procedures such as propensity score weighting are not usually required when comparing outcomes. Patients are selected, treated, followed and assessed according to a common protocol testing a specific hypothesis. Blinding of participants and physicians to the allocated intervention may be possible to minimize performance bias, and is especially important when assessing outcomes [13]. Quality control measures and external review of key parameters maximize study quality.
Limitations of RCTs

RCTs can be challenging to design (randomization and blinding), conduct (poor recruitment, loss to follow up), analyze (missing data) and report (patient exclusions).

RCTs require an adequate sample size and follow-up to have sufficient power to detect clinically relevant differences between interventions [14]. In practice, many clinical trials do not meet their pre-specified power requirements so a conclusion of ‘no significant difference’ in outcome should not be interpreted as meaning that two or more treatments are equivalent in effect.

Sample size estimation requires data about expected differences and variability of the primary outcome. Often these data are unknown or only available from observational studies prone to bias.

Although analyses using the intention-to-treat principle can provide an unbiased estimate of the treatment effect, this assumes that there are no differences in follow-up or missing outcome data that may bias the treatment comparison [15]. In some RCTs, not all participants receive their randomized intervention; they may, for example, cross-over to the other randomized treatment, in which case a per-protocol analysis may also provide useful information. Various analysis strategies exist, depending on whether the objective is to estimate treatment efficacy (the intervention effect under perfect conditions, in which case intent to treat can dilute the size of the treatment effect) or effectiveness (the real-world intervention effect with ‘imperfect’ compliance).

An RCT with double blinding, little missing data and good compliance will have a high internal validity, but if an RCT recruits only a very select population, the external validity (generalizability) may be low. This can happen due to overly restrictive inclusion/exclusion...
criteria or including only expert clinicians in select sites [16]. Single-center RCTs typically have lower external validity compared with multicenter RCTs which allow the comparison of results between centers. Finally, robust, adequately powered RCTs with long term follow up are difficult to organize, expensive and resource-intensive. Thus many RCTs focus on short-term or surrogate outcomes, the clinical significance of which is often uncertain. Any short-term benefits might not be maintained over longer time horizons which are more relevant to patients, clinicians and policy makers [17].

3. Advantages and Limitations of Systematic Reviews and Meta-analyses

Table 2 outlines the advantages and limitations of SR/MAs.

Advantages of SR/MAs

A SR is a literature review focused on a research question that tries to identify, appraise, select and synthesize all research evidence relevant to that question. SRs are a priori defined in a PICO (Participant, Intervention, Comparator, Outcome) based protocol outlining the study inclusion criteria. They are the only transparent and replicable form of literature review that provide a rigorous and critical qualitative appraisal of the evidence related to an intervention. SRs explore the findings of individual studies, draw attention to their differences and identify sources of bias [18].

A MA is a statistical technique for quantitatively combining the data from two or more separate RCTs asking the same or a similar question [19]. They should only be done as part of a SR, otherwise it is a combined analysis, susceptible to study selection bias. Two different types of
meta-analyses exist: literature-based or aggregate data (AD) MAs and individual patient data (IPD) MAs [20, 21].

MAs provide an overall estimate of the size of the treatment effect, giving due weight to the size of the individual RCTs. They are useful when individual studies are underpowered, yield inconclusive or conflicting results, or when an overall, more precise estimate of the size of the treatment effect is required. MAs increase the power to detect moderate but clinically meaningful differences in treatment outcome and assess if the treatment effect is similar across different studies or types of patients [22]. They are useful in exploring the effects of an intervention in subgroups of patients, especially in IPD MAs [20, 21].

SRs and MAs are vital for guideline developers, healthcare providers, patients, researchers and policy makers in order to guide clinical practice, research and healthcare policies [23].

Limitations of SR/MAs

The validity of a MA depends on the quality of the systematic review upon which it is based. SRs and MAs have a number of potential limitations including poor quality of included studies, heterogeneity, and publication bias.

The literature summary provided in a SR and the results of a MA are only as reliable as the quality of the included studies. Although IPD meta-analyses and multicenter RCTs can be analyzed using the same statistical techniques for clustered data, where the clusters are studies and centers, respectively, there may be important clinical and methodological heterogeneity between the studies in a MA since they are not carried out based on a common protocol. The studies may be heterogeneous regarding patients included, the intervention or the assessment of treatment outcome. Although heterogeneity in treatment effect can be better investigated
in IPD MAs, the primary studies should be similar enough to be combined, otherwise genuine differences in effects may be obscured [24,25]. Since institutions participating in a multicenter study are supposed to treat, follow up and assess patients according to a common protocol, there is potentially a greater degree of standardization and higher quality data in multicenter clinical trials as compared to studies included in meta-analyses.

If bias is present in the individual studies included in a MA, MAs will compound these errors and produce a biased result. The risk of bias (RoB) on the outcomes in each study should be systematically assessed and sensitivity analyses performed to examine the effect of RoB on the conclusions. Observational and non-randomized comparative studies in SRs of interventions should not be included in MAs because the MA may provide very precise but spurious results due to confounding and patient selection bias.

Only a non-random proportion of research projects ultimately reach publication in an indexed journal and become readily identifiable for systematic reviews. Statistically significant, ‘positive’ results favoring an intervention are more likely to be published, published quicker and published in higher impact journals, leading to publication bias [26]. When these trials are pooled together in a MA, this may lead to an exaggeration of the treatment effect. Begg and Egger have both proposed tests along with funnel graphs and plots to detect publication bias, however they have limited power in small meta-analyses, for example those including less than 10 studies [27]. In order to minimize publication bias, authors should perform a comprehensive systematic literature search, looking not only for published trials in various electronic databases, but also search trial registries for unpublished studies and conference abstracts or proceedings [18].
4. The Results of a Randomized Controlled Trial are in conflict with the Results of a Systematic Review/Meta-analysis

It is not uncommon for the results of a large RCT to appear to be inconsistent with evidence from SRs/MAs. The most extreme is when an intervention thought to be beneficial is demonstrated to be harmful in a large RCT [9,10]. More commonly, an RCT may show a treatment to be ineffective, or less effective than that found in a previous MA, or perhaps only effective in a subpopulation of patients. Assuming the conflicting RCT was of high quality, a number of issues should be explored to try to explain the discrepancies.

Quality of the systematic review

The starting point is the methodological quality of the SR. AMSTAR and DART checklists [28-30] allow readers to judge a review’s quality by focusing on the essential components of a well-conducted SR. Items include the comprehensiveness of the search strategy, a description of the characteristics of included studies and an assessment of their scientific quality. A poor quality SR/MA may produce biased results that conflict with a large RCT.

Small study effects and publication bias

Small study effects and publication bias can individually and jointly produce results in a SR/MA that conflict with a large RCT. Studies have shown that small RCTs can exaggerate intervention effects due to shortcomings in methodological rigor which may then introduce bias [3]. Small studies that find statistically significant (but unrealistically large) treatment effects are more likely to be published than negative studies and then included in an SR and MA, leading to publication bias. Both of these phenomena can be investigated using funnel plots [31].
Heterogeneity

Heterogeneity within a SR/MA can arise from many sources, including the population recruited (age, sex, disease severity, etc.), the intervention(s) and control treatments, and the definition and timing of outcome measurements. If studies included in a SR/MA differ substantially from a subsequent large RCT, then judgement is required on whether similar findings should be expected.

Another source of heterogeneity is differences in the methodological quality of the included studies. Deficiencies in the generation and concealment of the allocation sequence, adherence to treatment, handling of missing data, and outcome assessment can all introduce bias in the outcomes reported in the included studies [18]. Bias may then be propagated in meta-analyses through the pooling of biased study effects, thus contributing to different estimates of effectiveness between a SR/MA and subsequent large RCTs. Nevertheless, since a MA is generally seen to have a higher LE than a single RCT, the results of a poor quality MA may have more impact than a well-conducted RCT.

Heterogeneity should be assessed using both clinical knowledge and statistical methods. If substantial heterogeneity from any source is suspected, random effects models are recommended, however the pooling of data and estimation of an overall treatment effect may be inappropriate with any statistical model in the presence of heterogeneity. Meta-regression is a useful tool to explore the relationship between RCT effect sizes and characteristics on a study level [32], however IPD are required for assessment on a patient level [21, 33]. Appropriate statistical modelling may show that after correcting for sources of bias and heterogeneity,
discrepancies between SR/MA and definitive RCTs are reduced. Whatever the approach, interpretation of results is less straightforward when heterogeneity is present.

In order to provide guidance to clinicians and guideline developers when there is a conflict of results between a large RCT and a SR/MA, a practical checklist of points to consider is provided in Table 3.

5. Examples of discrepancies between findings from meta-analyses and large randomized controlled trials

Medical expulsive therapy

Five SRs and MAs on the management of uncomplicated symptomatic ureteric stones using medical expulsive therapy (MET) were published in the past 10 years [34-38]. All five suggested that alpha blockers and nifedipine were more effective in increasing the spontaneous passage of ureteric stones compared to control (risk ratios ranging from 1.45-1.59). The reviews identified numerous sources of potential bias which limited the strength of evidence and the authors concluded an urgent need to conduct a large, robust, multicenter RCT to address these shortcomings. Pickard et al [8] published the results of such an RCT in 1167 patients and found no evidence that either tamsulosin or nifedipine increased the rate of spontaneous stone passage compared with placebo. Results were consistent across subgroup and sensitivity analyses.

We compare the Pickard et al RCT [8] to the meta-analysis with the most studies, Seitz et al [36], to explore and discuss discordant findings. Most RCTs included in Seitz’s meta-analysis were small and recruited from a single-center; only 6 of 35 (17%) recruited more than 100
patients. The majority had low internal validity and only one RCT reported allocation concealment. As small RCTs may report larger effect sizes compared to larger RCTs, a meta-analysis of small RCTs can lead to biased estimates of treatment effects [39]. Seitz also found evidence of publication bias which can lead to an overestimation of treatment effects and compromise the validity of the meta-analysis findings [40].

There was evidence of clinical heterogeneity in Seitz’s review concerning the patient inclusion criteria, stone characteristics, intervention, treatment in the control group, and outcome measurement. In the MA, the primary outcome of being stone-free was inconsistently defined, assessed using different imaging modalities, and measured at a variety of time points. In Pickard, the primary outcome was need for further intervention within 4 weeks of randomization, which is compared here to being stone-free. In the control group, 80% of patients were stone-free in the Pickard RCT whereas in Seitz, the stone-free rates ranged from 4% to 78%, which highlights the potential impact of the heterogeneity in the included studies. With contrasting primary outcomes and different baseline event rates in the control groups, it is not surprising that the RCT and the MA reported discordant findings. The choice of primary outcome is clearly of paramount importance in any trial. Heterogeneity in the conduct, design and reporting of trials in this MA makes pooled treatment effects difficult, if not impossible, to interpret.

**Partial versus radical nephrectomy**

In an EORTC RCT involving 541 patients with a solitary T1-T2 N0 M0 renal tumor ≤ 5 cm, 21 patients progressed, 9 after radical nephrectomy (RN) and 12 after partial nephrectomy (PN). An intent to treat analysis found an overall survival (OS) advantage in favor of RN (HR = 1.5, p =
0.03), however only 12 of the 117 deaths were due to kidney cancer, 4 on RN and 8 on PN [10].

Subsequently, Kim et al published a SR and MA including some 41,000 patients which found statistically significant improvements in both OS (HR = 0.81, p < 0.001) and disease specific survival (DSS) (HR = 0.71, p < 0.001), but this time in favor of PN [9]. How can this discordance be explained?

The Kim meta-analysis has a number of limitations. Firstly, the 38 included trials were mostly retrospective, single center studies. The only RCT was the EORTC study. No information was provided about the distribution of follow up or patient characteristics by treatment group (T category when > T1, tumor size, grade, cell type, or renal function). Consequently, the observed differences in survival may not be directly due to differences in treatment efficacy. In addition, it is not clear to which patients the results can be generalized. Lastly, there was significant heterogeneity in the size of the treatment effect across the studies so the overall estimate of the HR is not meaningful. Nevertheless, the EORTC RCT also had limitations and should be interpreted cautiously: 55 patients crossed over to the other randomized treatment, 140 patients were clinically or pathologically ineligible and there were few cancer related events.

The MA found that PN was associated with a decreased risk of severe chronic kidney disease (CKD), however the EORTC study only found a reduced incidence of at least moderate renal dysfunction, not of advanced kidney disease or renal failure, and this was not associated with a corresponding difference in survival [41]. The studies in the MA did not always specify the status of the contralateral kidney whereas in the EORTC study the contralateral kidney had to be normal.
Critical information regarding the biases of the studies included in the SR were not made explicit since a GRADE approach to assess the quality of evidence was not done [42]. The quality of the studies in the SR and heterogeneity of results call into question the validity of the conclusions of the MA which should thus be viewed with skepticism. The same year, another SR suggested that localised RCCs are best managed by PN where technically feasible. However, the evidence base had significant limitations due to studies of low methodological quality and high risks of bias [43].

Further non-randomized studies have found improved survival with PN [44,45] and a reduction in the risk of cardiovascular events relative to RN [46], however patients chosen for PN had a higher baseline likelihood of long-term survival [47,48]. In another study, only stage-II CKD patients had a decreased risk of developing significant renal impairment on PN [49]. More recently, a SR and MA of 21 non randomized comparative studies in patients with clinical T1b and T2 renal tumors found better tumor control and survival with PN as compared to RN [50], but it is subject to the same biases as the Kim MA.

Taking into account all available efficacy data and a perceived advantage in renal function, the 2016 EAU Guidelines recommend, with several exceptions, that localized renal cancers are better managed by PN than with RN.

6. Discussion

It is generally accepted that a high quality SR of RCTs and associated MA can provide a higher level of evidence than a single RCT addressing the same question [2]. It can be problematic,
However, when the results of the MA are in direct conflict with the RCT, making it difficult for guideline organizations to interpret the evidence and issue recommendations.

Guideline groups should follow well-defined methodological rules to assess the studies in these situations. RCTs should be appraised on their internal and external validity using established tools [51]. The conflicting SR/MA should be appraised in the same fashion, to determine the methodological quality of the review, the quality of the included studies, inconsistency within the studies, unexplained heterogeneity, and likelihood of publication bias using tools such as AMSTAR [28,29] and DART [30]. In some cases, the discrepancy may be due to errors in the MA in applying study eligibility criteria or even data extraction [52], hence the need for a SR/MA protocol and strict quality control.

When MAs include many small underpowered studies, especially combined with likely presence of publication bias, there is immediate concern for over-inflation of, or completely erroneous, effect size measurement. Additionally, when a great degree of heterogeneity exists in the MA which cannot be easily accounted for, the results may be highly unreliable. In this regard, IPD MAs provide a better platform for assessing and explaining heterogeneity than aggregate data MAs.

Two examples were discussed in this manuscript to illustrate the assessment process. In the case of MET for ureteric stones, a large, high quality RCT [8] contradicted many well established MAs which pointed to a benefit with this therapy. Analysis of a representative MA [36] revealed the inclusion of many small RCTs, poor internal validity, significant study heterogeneity and likely publication bias. When such MA concerns are present, a single high quality RCT may be considered as having the higher LE. For guideline organizations, this
process can be used to justify a change in recommendations based on methodologically sound principles.

Radical versus partial nephrectomy provides a more complex example. The MA [9] included only a single RCT, which was the study in conflict with its own results. The other included studies were all retrospective, which in general provide a lower LE. Risk of bias was poorly assessed, and significant study heterogeneity was present. It is important to reiterate that combining observational studies in general, and even comparative non-randomized studies with RCTs in an intervention MA, may produce unreliable results and is not considered valid. In light of all this, the single RCT [10] in this circumstance might provide more guidance than the MA if it was of significantly high quality. However, this RCT also had some methodology concerns, so the comparison is not so simple.

Instead of automatically assigning a higher LE to SR/MAs which conflict with RCTs, these examples have shown that the quality of the evidence and the RoB of studies included in SRs/MAs should be assessed to determine which source provides the better evidence.

Although non RCTs can be included in SRs, we have emphasized that only RCTs should be included in intervention MAs. RCTs are not required for prognostic factor and diagnostic test accuracy MAs, however the studies included in these MAs should preferably be prospective in nature and based on a protocol to minimize risk of bias.

Despite the availability of MAs and RCTs, and also in cases where high level evidence does not exist, we may still not know what the best treatment is. The GRADE system, which takes into account the quality of evidence (high, moderate, low, very low) for critical outcomes, provides strengths of recommendations (strong, weak) for or against a treatment to aid clinicians in their
practice when consensus is not possible [42,53]. A decision curve approach, which takes into account a patient’s values and preferences, may also be used to help choose between the different treatment options.

7. Conclusions

New or existing RCT data can lead to conflicts with MA data. In this paper, we present examples of, and explore reasons for, such conflicts. Guidance is provided to guideline developers on how to interpret conflicting data in such circumstances to help assess which source is more reliable. For guideline organizations, both within and outside of urology, having a well-defined and robust process to deal with such conflicts is essential to improve guideline quality.

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Table 1: Advantages and Limitations of Randomized Controlled Trials

<table>
<thead>
<tr>
<th>Advantages</th>
<th>Limitations</th>
</tr>
</thead>
<tbody>
<tr>
<td>Randomization minimizes the influence of both known and unknown prognosis variables on treatment outcome</td>
<td>It may be difficult to recruit and follow up patients</td>
</tr>
<tr>
<td>RCTs can demonstrate causality</td>
<td>Ethical considerations may make randomization difficult</td>
</tr>
<tr>
<td>Patients are treated according to a common protocol</td>
<td>Required study power might not be met</td>
</tr>
<tr>
<td>Quality control of treatment and outcome assessment</td>
<td>Generalizability may be low</td>
</tr>
<tr>
<td>RCTs provide the strongest empirical evidence of treatment efficacy</td>
<td>RCTs are expensive and resource intensive</td>
</tr>
</tbody>
</table>
Table 2: Advantages and Limitations of Systematic Reviews and Meta-analyses

<table>
<thead>
<tr>
<th>Advantages</th>
<th>Limitations</th>
</tr>
</thead>
<tbody>
<tr>
<td>Focused well defined clinical question with a clear objective and explicit, predefined study eligibility criteria</td>
<td>Depends on the quality of the included studies</td>
</tr>
<tr>
<td>Comprehensive literature search strategy to guarantee the identification of all potentially eligible studies</td>
<td>Susceptible to the effects of heterogeneity of included studies</td>
</tr>
<tr>
<td>Critical appraisal of all the included studies that is used to guide the analysis and conclusions</td>
<td>• Clinical heterogeneity:</td>
</tr>
<tr>
<td></td>
<td>- Participants <em>(e.g. age, gender, disease severity, disease subtype, study eligibility criteria)</em></td>
</tr>
<tr>
<td></td>
<td>- Interventions <em>(e.g. drug doses, duration/intensity of treatment, delivery, co-interventions, surgeon experience)</em></td>
</tr>
<tr>
<td></td>
<td>- Outcomes <em>(e.g. definition of outcome, outcomes reported, timing and method of measurement, follow-up duration, cut-off points)</em></td>
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<tr>
<td></td>
<td>• Methodological heterogeneity <em>(e.g. different study designs, reporting bias across studies)</em></td>
</tr>
<tr>
<td></td>
<td>• Statistical heterogeneity</td>
</tr>
<tr>
<td></td>
<td>Publication bias</td>
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<tr>
<td></td>
<td>Time and resource consuming</td>
</tr>
</tbody>
</table>
Table 3: Checklist of points to consider when the findings from a systematic review and meta-analysis differ with those from a large randomized controlled trial

<table>
<thead>
<tr>
<th>Criteria to consider</th>
<th>Questions to ask</th>
<th>Rationale</th>
</tr>
</thead>
<tbody>
<tr>
<td>Selection bias</td>
<td>Were the sequence generation and allocation concealment adequate in both the studies included in the SR/MA and the subsequent trial?</td>
<td>If the sequence generation was not truly random or the allocation was not effectively concealed, this can lead to exaggerated estimates in individual studies and these may be amplified in MA.</td>
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<tr>
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<tr>
<td>Confounding bias</td>
<td>Were the groups balanced for known prognostic factors at baseline and were any imbalances controlled for in the analysis?</td>
<td>Imbalances in known and unknown prognostic factors are possible even in well-designed RCTs. Baseline imbalances may explain differences in estimates of effect if not controlled for in the analysis.</td>
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<tr>
<td>Performance and detection bias</td>
<td>Where possible, in all the studies included in the SR/MA and for the new trial, was blinding of study participants, clinicians administering the treatment, ancillary care-givers and outcomes assessors done? When blinding is not possible, could knowledge of the treatment received affect interpretation of any of the outcomes?</td>
<td>Some objective outcomes are unlikely to be affected by knowledge of the intervention arm, but failure to blind (particularly for subjective outcomes) may lead to an exaggeration of effect sizes in individual studies and these may be amplified in MA.</td>
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<tr>
<td>Attrition bias</td>
<td>Were all dropouts documented and unlikely to be related to the treatment outcome in the studies included in the SR/MA and in the new trial?</td>
<td>If drop-out rates differ between the treatment arms, then the reasons may be related to the outcome of interest and may hide important outcome effects.</td>
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<tr>
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<tr>
<td>Reporting bias</td>
<td>Were all outcomes that were stated in the methods and/or protocol for all the studies included in the SR/MA and in the new trial reported in the trial report? Were all the outcomes measured appropriately (as defined in the protocol) or were deviations</td>
<td>Selective reporting of outcomes, or selective methods of reporting, may lead to exaggerated estimates of effect</td>
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<tr>
<td>Publication bias</td>
<td>Were funnel plots used to investigate publication bias in the SR/MA? Is the funnel plot symmetrical or is there reason to believe there is a systematic difference between published and unpublished studies? Note: this is difficult to assess when there are less than 10 RCTs contributing to a MA.</td>
<td>Asymmetric funnel plots raise suspicion that there are systematic differences between published and unpublished studies and that some positive or negative trials may be unpublished. The may lead to exaggerated effect sizes in a MA.</td>
</tr>
<tr>
<td>Consistency and heterogeneity of outcome</td>
<td>Did the studies included in the SR/MA have overlapping 95% CIs for the outcome? Was variation more than would be expected by chance alone? Was the I² statistic &lt;40%? (Cochrane/GRADE rule of thumb...) Were subgroups used to explain any observed heterogeneity? Were event rates in the control group similar in the different studies? Note: Subgroups of the population, the intervention/control types, or the outcome measurement may explain heterogeneity.</td>
<td>If the outcomes can be shown to be more effective in certain subgroups, or with variations of an intervention (e.g. a higher dose), then this explained heterogeneity may indicate a key difference which may justify the results in the new trial. Where unexplained heterogeneity exists, then the estimate of effect is likely to be uncertain, even if precise.</td>
</tr>
<tr>
<td>Directness</td>
<td>Do the studies included in the SR/MA and does the new trial both directly assess the research question about the population, interventions and outcomes?</td>
<td>Indirect populations, interventions, surrogate outcome measures or indirect comparisons may conceal or exaggerate important differences within and between studies and may impact upon the estimate of effect.</td>
</tr>
<tr>
<td>Precision</td>
<td>Were the sample sizes of the studies included in the SR/MA and the new trial powered to address the outcomes of interest? Does the 95% CI in the MA include clinically judged appreciable benefit and harm?</td>
<td>If any of the SR/MA included trials, or the new trial were not powered to detect a clinically meaningful difference in the effect estimate, this may reduce our confidence in the estimate of effect. If the lower and upper 95% CI thresholds indicate that at one end the intervention may be beneficial, but at the other, it</td>
</tr>
</tbody>
</table>
may be harmful, this will likely reduce our confidence in the estimate of effect.

| Sensitivity analyses | When some studies included in a SR/MA are judged to be at high risk of bias, and others at low risk of bias, or extreme variations in the included studies’ populations or interventions are apparent: did the authors conduct a sensitivity analysis to ascertain the estimates of effect on only those studies judged to be at low risk of bias? | Sensitivity analyses are different from subgroup analyses. Some studies are actively omitted as we are only interested in the results when the biased or ‘different’ studies are omitted. |

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- conception and design                      Sylvester, N'Dow

- acquisition of data                       Sylvester, Lam, Marconi, S. MacLennan, Y. Yuan, Van Poppel, N'Dow

- analysis and interpretation of data       Sylvester, Canfield, Lam, Marconi, S. MacLennan, Y. Yuan, G. MacLennan, Norrie, Omar, Bruins, Hernandez, Plass, Van Poppel, N'Dow

- drafting of the manuscript                Sylvester, Canfield, Lam, Marconi, S. MacLennan, Y. Yuan, G. MacLennan, Norrie, Omar, Bruins, Hernandez, Plass, Van Poppel, N'Dow

- critical revision of the manuscript for important intellectual content   Sylvester, Canfield, Lam, Marconi, S. MacLennan, Y. Yuan, G. MacLennan, Norrie, Omar, Bruins, Hernandez, Plass, Van Poppel, N'Dow

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- obtaining funding                         Not Applicable

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- supervision                                 Sylvester, N'Dow

- other (specify)                             Not Applicable

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☐ Analysis
☐ Interpretation of the data
☐ Preparation
☐ Review
☐ Approval of the manuscript

OR

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