LETTER TO THE EDITOR

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Meta-analysis of coronary CT angiography in the emergency department

We read with interest the recent meta-analysis of coronary CT angiography (CCTA) in the emergency department (ED) by D'Ascenzo *et al.*[1] Our group has conducted a similar analysis[2], and some important comparisons should be discussed.

First, in regards to data pooling, the authors state that 'no difference was noted between random and fixed effects models'. However, the endpoint of increased invasive coronary angiography (ICA) after CCTA is significant in a fixed effects model. Such a method would be more appropriate in the setting of low or no statistical heterogeneity and would result in a more precise estimate (smaller confidence interval). Additionally, they evaluated downstream ED visits and hospital admissions but not downstream ICA and coronary revascularization. This is of clinical significance as many chest pain patients may be 'ruled out' in the ED, but undergo ICA and/or revascularization for a significant coronary lesion in close outpatient follow-up. If including downstream ICA, most of which will be driven by the index CCTA, the odds of ICA becomes significant [1.36 (1.02–1.83, $P = 0.03$)] whether by fixed or random effects and the odds of revascularization remains significant.[2] This finding challenges the hypothesized but unproven benefit of CCTA—avoidance of downstream ICA due to its higher sensitivity, which may not hold true in all populations studied.[3] We feel the conclusion that CCTA 'seems not to increase subsequent invasive coronary angiographies' is inconsistent with the trend of increased ICA in all studies, overlooks ICA in near term follow-up, and, thus, might misguide the reader. Of the multiple ways to evaluate ICA after CCTA based upon these data, the authors' method is an outlier in that it does not reach significance.

Secondly, the high inconsistency (I^2) for the cost and time data (98% for time and 99% for cost) limits the validity of these pooled endpoints. Nevertheless, the authors did not mention this as a limitation. This suggests that 98 and 99% of the variability of the data are due to study design or population factors not explained by the analysis alone. Current practice of meta-analysis[4,5] would suggest that these outcomes should be stated to be exploratory and potentially unreliable. It is important to note that not only are the endpoints of time and cost in these studies highly statistically inconsistent, but also there is marked clinical heterogeneity. Costs were evaluated in different systems using different methods of collecting different data and in particular, the enrollment time and inclusion criteria differed for each study (e.g. <6h from entry to ER for ROMICAT II vs. <12 for other studies, etc.). In spite of a very high (97–99%) inconsistency[4] in three of six forest plots depicted, the authors only mentioned that, 'inconsistency was <50% for most of the examined outcomes'. According to established guidelines for meta-analysis[5] that the authors reported to follow, the pooled analysis of cost and time outcomes should be considered exploratory and accompanied by a more in-depth analysis regarding causes of high statistical inconsistency.

Conflict of interest: none declared. The opinions and assertions herein are the authors' alone and do not represent those of the Department of Defense or the U.S. Government.

References

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Meta-analysis of coronary CT angiography in the emergency department: reply

We welcome the opportunity offered by Hulten *et al.* to engage in a scholarly debate regarding meta-analyses, in general, and our recent one on coronary computed tomography (CCT) in patients presenting with low-risk chest pain to the emergency department (ED), in particular.[1]

First, Huelten *et al.* state that according to their prior analysis, patients in the CCT group were at a higher risk of undergoing invasive coronary angiography (ICA) when a fixed-effect model was exploited [odds ratio (OR) = 1.35 (95% confidence interval 1.00–1.81), inconsistency (I^2) = 30%]. From a methodological point of view, we chose to report only a random-effect analysis, both because the fixed-effect one was close to non-significance, and because we followed the recommendation of The Cochrane Collaboration to use the most conservative analytical approach. Random-effects and fixed-effect models rarely differ, and we believe that when this happens reporting the random effects could be the most accurate choice.[2]