our article proved that doctors were able to identify which patients would benefit from CABG operations. The SYNTAX score may provide an additive value to the Euroscore; whether this will be a better predictor than an experienced clinician, we'll need to wait and see!

Appropriateness of study design

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COMMENT ON

Bhatia M, Singh B, Nicolaou N, Ravikumar KJ. Correlation between rotator cuff tears and repeated subacromial steroid injections: a case-control study.
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I have a number of comments related to the design of this study and the results produced. The authors describe this as a case-control study. A case-control study is an observational study which defines case and control group by the measure of outcome. According to the title of this study, the outcome measure was 'rotator cuff tears'. In fact, the authors used historical data to separate their cohort into groups based upon exposure, *i.e.* frequency of subacromial injections. This is, therefore, better described as a comparative cohort as it does not conform to a case-control design.

The fundamental assumption regarding a cohort study is that all individuals were disease-free at the beginning of the study period – in this study 'without rotator cuff tears'. A cohort study then seeks to measure exposure frequency and outcome to identify a relationship. A problem with the design of this study is that outcome status (the presence of a rotator cuff tear) was not known at the inception of the study and was determined at some point after the study had begun; consequently, it is not known if cuff tears occurred following steroid use or indeed preceded steroid use.

The inclusion criteria to the study may also limit the ability of the study to find a difference in frequency of rotator cuff tears related to steroid use – should such a difference exist. The study only included those individuals who had undergone magnetic resonance imaging (MRI) based on clinical need. I understand the practical nature of this; however, the very nature of having an MRI scan may indicate people have severe pathology. This may indicate that the people in the 'control' group have a different burden of disease to those in the case group, as they have undergone MRI despite only having had one or two injections. If this were the case, the relative risk of disease would tend to 1.0 and no difference would be identified even if a true relationship were to exist.

The authors suggest ethical concerns in producing a prospective cohort study to answer this question. I recognise the practical difficulties in such a study in terms of cost and time; however, it seems unlikely that an ethical objection would exist to answer this question as this would merely require baseline MRI followed by routine treatment based on clinical need. There does not appear to be a reason to randomise treatment groups as the authors suggest.

I commend the authors for the size of their study and work involved; however, I feel it is difficult to produce meaningful results to this question without performing a prospective investigation with strict protocols and entry criteria.

Clarity of language

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COMMENT ON

Chaganti S, Kumar D, Patil S, Alderman P. A language for effective communication between surgeons and radiographers in trauma theatre. *Ann R Coll Surg Engl* 2009; **91**: 509–12

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The language proposed by Chaganti and colleagues for effective communication when using an image intensifier in the trauma theatre remains likely to cause confusion. They focus on using the names given to each movement of the image intensifier rather than communicating where the image needs to be centred and with what degree of obliquity. Radiologists and radiographers use the latter method with commands such as 'centre up', 'centre down', 'centre left', 'centre right', 'centre on a particular structure' to move the image intensifier towards a particular structure. The degree of obliquity is then communicated with commands such as 'left anterior oblique', 'right anterior oblique', 'craniocaudal angulation', 'caudocranial angulation'. A typical command proposed by the authors might be 'on the wheels toward the patients head, orbital with X-ray source 45 degrees towards the head'. With the language developed in the radiology department, the same command would be 'centre up, 45 degrees craniocaudal angulation'. The command is short, unambiguous, and avoids the confusion when using the names of each individual image intensifier movement. In addition, the terms used will be familiar to all radiographers and easily learnt by trauma surgeons.

AUTHORS' RESPONSE

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We appreciate that Day also agrees there is a definite role for unambiguous language between doctors and radiographers. Unfortunately, trauma surgeons do not work with radiographers as closely as radiologists and may have to work with different radiographers with various levels of experience. We respect what appears to have developed in his department. However, that has not become wide-spread as a language and certainly was not published in the literature at the time when we submitted our work for publication to the *Annals*.

As soon as it is appreciated that a common language is required between doctors and radiographers, a shorter, user-friendly version of commands is more than likely to evolve if one is working with the same team. The language suggested in our article is just an example. We accept some of the commands are longer than desired but shorter abbreviated commands leave room for ambiguity especially if one has to work with different radiographers sometimes in the same procedure.

Indications for tonsillectomy

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COMMENT ON

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We published a three-cycle audit on a similar theme in 2002.[1] We looked at 181 children at a paediatric teaching hospital undergoing tonsillectomy in three cohorts to determine adherence to the SIGN guidelines for tonsillectomy and to assess adequate record keeping. The first cohort of patients was listed for surgery prior to the publication of the Scottish Intercollegiate Guidelines Network (SIGN) guidelines; the second after. We then analysed the data for these two cohorts, presented them to the relevant clinicians, and kept a reminder poster (of the SIGN guidelines) placed in each out-patient room prior to the third cohort.

Although we found a significant improvement in record keeping (chi-squared test) in cohort 1 to 2 $P < 0.001$, and cohort 2 to 3 $P < 0.001$, we did not find a statistically significant result in adherence to the SIGN guidelines

In our study we set a standard of 95% adherence to the SIGN guidelines, which we were successful in achieving during the third cycle. There is no specifically mentioned standard in the audit of Toh *et al.* and we note that, after their second cycle, adherence to all four SIGN recommendations was still only 44% (74% during our second cycle), despite dissemination of audit findings of the first cycle to clinical staff, SIGN guideline posters and introduction of rubber stamp; perhaps this could be improved upon further with a third cycle to meet an appropriate standard? We acknowledge that any improvement is desirable; however, one would expect better results when all other indications for tonsillectomy have been excluded and there was an additional change implemented (the rubber stamp) in comparison with our study. We emphasise that a third cycle in the audit would have contributed even further in improving clinical practice.

It is also surprising that the paper makes no reference to the National Prospective Tonsillectomy audit[2] or Scottish tonsillectomy audit[3] that provide valuable reference points for any study on tonsillectomy.

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AUTHORS' RESPONSE

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