'non-inferiority' trial design to show that the new treatment has a similar effect to the standard treatment, rather than demonstrating statistically significant superiority. However, there are issues with trials of this type that make them considerably less credible than superiority trials[2 3] which I outline in the following few paragraphs.

Blinding is one of the most important bias-avoiding techniques available to clinical researchers and it is not always feasible to blind the investigator or patient to the treatment regimen. However, blinding does not protect against bias nearly as well in non-inferiority trials as it does in superiority trials. In a superiority trial, a blinded investigator cannot influence the results to support a preconceived belief in superiority, but in a non-inferiority trial, there is no protection against a blinded investigator biasing the results towards a preconceived belief in equivalence by assigning similar ratings to treatment responses of all patients.

Intention-to-treat (ITT) is recognized as the most valid approach for the analysis of superiority trials because it adheres to the randomization procedure and is generally conservative. Some may argue that this analysis is overly conservative; however, most would agree that a positive ITT analysis of a superiority trial is convincing. Unfortunately, no such conservative analysis methodology exists for non-inferiority trials. This is largely because including data after study drug discontinuation in the analysis, as ITT does, tends to bias the results towards equivalence. Therefore, non-inferiority trials are often analysed using ITT and per-protocol approaches, and only if both approaches support non-inferiority is the trial considered positive. Even in this case, however, the possibility of bias cannot be ruled out. Another issue is the choice of inferiority margin as this affects the sample size calculation and the conclusion of the study.

Regarding the comments about bispectral index (BIS) as an objective measure of sedation on the intensive care unit (ICU), I would have to agree that it has not been overwhelmingly shown to be a valid measure of sedation. However, a number is certainly an objective measure and there are studies published which do show a correlation of BIS with a sedation scale in the ICU population.[4] I concede that others (including the same group of workers) do not concur with the evidence presented in this paper.[5 6]

While I accept that the numerical inconsistencies in their paper[7] were explained to us in e-mail correspondence (and I very much thank them for engaging with us in this respect), this does not negate the fact that the average reader would potentially have been mislead from the figures in the main text of the paper.

Declaration of interest

N.R.W. is an employee of the University of Aberdeen, and Chairman of the Board of Directors, *BJA*.

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Is sedation by non-anaesthetists really safe?

Editor—I read with great interest the editorial recently published in the *British Journal of Anaesthesia* that questioned whether sedation by non-anaesthetists is really safe.[1] The authors noted their 'grave concerns' about the emerging practice of non-anaesthetist-administered propofol (NAAP) during electrophysiology procedures. I offer the following insights into this issue.

An important factor driving the emergence of this practice did not seem to be considered adequately in this editorial. One rationale behind using NAAP during electrophysiology procedures is that: (i) gaining access to monitored anaesthesia care for sedation in the cardiac catheterization laboratory is difficult in many institutions around the world;[2 3] (ii) propofol might outperform the current standard practice sedative and analgesic medication regimen that is used during electrophysiology without an anaesthetist present, which consists of a benzodiazepine/opioid combination, because its shorter half-life could potentially reduce recovery time (and as a result, healthcare resource costs)[4] and its rapid onset of action might result in increased patient satisfaction with the procedural experience; and (iii) adverse events associated with the use of NAAP in other procedural settings are rare.[5]

Of note, though, while safety data reported in the preliminary studies focused on NAAP outcomes during electrophysiology are encouraging, future research in this field must utilize more rigorous research designs in order to determine whether NAAP actually is the superior sedation strategy, considering the issues highlighted above.[6–8] In this regard, randomized controlled trials comparing NAAP with anaesthetist-

administered sedation and the current standard practice nurse-administered sedative and analgesic medication regimen which consists of a benzodiazepine/opioid combination should be considered.

Yet, instead of taking an 'us against them' perspective, it would be more beneficial to our future patients if NAAP research in the electrophysiology context always involved truly collaborative ventures between cardiologists and nurses and anaesthesiologists. A multidisciplinary approach, incorporating specialist knowledge from anaesthesiologists for the development of protocols for medication titration and patient monitoring and comprehensive education programmes, would ensure the cardiologists and nurses involved in the provision of patient care during NAAP have the necessary knowledge and skills to promptly detect clinical deterioration and effectively apply interventions to support or restore cardiac and respiratory function. From a quick search of authors' affiliations, it does not seem that the current research into NAAP during electrophysiology has managed to achieve such multidisciplinary collaboration.[6–8]

Declaration of interest

None declared.

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Reply from the authors

Editor—We thank Dr Conway for his interest in our article.[1] He is correct that the word limitation of the article precluded as much discussion of the topic as we would have liked. He cites four reasons why non-anaesthetist-administered propofol (NAAP) is used in electrophysiological procedures: (i) it is difficult to find an anaesthetist, (ii) propofol might be better than the current regimen used without an anaesthetist present, (iii) propofol's rapid onset of action may result in increased patient satisfaction, and (iv) NAAP in gastroenterology is safe. We believe that we have covered these issues in the editorial.

We do not believe that difficulty finding an anaesthetist is a good enough reason to reduce safety standards, and also believe that patients are also of this viewpoint. In general, anaesthetists are available if funding is. We agree that with the correct training, it is likely that the use of propofol in non-anaesthetic hands may well outperform benzodiazepines and opioids (as it does in an anaesthetists' hands), but what is the skill set required, and who will pay for the training, even if cardiologists/nurses are keen to undergo it? There is much evidence that patient satisfaction after propofol sedation is greater than after benzodiazepines, we agree.

It is the last statement that we have a real problem with. Adverse events in gastroenterology using intermittent propofol sedation (not infusion) are indeed rare, and as we explained in the editorial, this technique seems to be safer than benzodiazepines and opioid combinations—the reference is quoted by Dr Conway. However, his final three references make our point. The largest of the studies of propofol in electrophysiology that is quoted is Salukhe's, which was 1000 patients—remember the rule of three from the editorial. This trial is much too small to demonstrate safety in all hands. Indeed, in that paper, 15.6% of patients had to be switched from propofol to benzodiazepines, mainly for hypotension. In the final paper quoted by Dr Conway, 582 patients were sedated with propofol by nurses after a training course and proctoring by an anaesthetist—nonetheless, there was a quoted incidence of 'serious adverse events' of 10%, with more adverse events the longer the procedure was. This is a high incidence of problems. In my own institution, where patients are sedated for electrophysiological procedures by anaesthetists if propofol is used (for the reasons relating to the UK guidance as explained in the editorial), adverse events related to sedation are few and far between.

We agree this is not a 'them and us' argument, but it is a question of what is an acceptable risk to patients when viewed from their perspective, and what training and skill set is required to reduce these risks to an acceptable level if