Declaration of interest
None declared.

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Women at the table
Reply from the authors to Dr Ward
Editor—Thank you for giving us the opportunity to respond to the letter from Dr Ward—we thank him for his comments.

While we applaud the messages in the most recent Davies report1 to which Dr Ward refers, his reference to Sheryl Sandberg’s new book, Lean in: Women, work and the will to lead2 deserves further discussion. In our editorial,3 we referred to Sheryl Sandberg’s TED Talk from 2010 where she described how women must take some ownership for (unintentionally) holding themselves back when it came to their careers. This talk, which has been viewed more than 2 million times, encouraged women to ‘sit at the table’—in other words, put themselves forward. The book goes a bit further and gives the feeling that somehow Sheryl is putting the blame on women’s own shoulders just a little more than we find comfortable.

We commend Sheryl Sandberg for all she is doing for women. But this book has its issues. A quick look at the multitude of book reviews will reveal that we are not the only ones who feel uneasy about her message. Although this book is billed as giving practical advice on how to mix professional accomplishments with more personal goals, it is not really like that. The anecdotes are a bit cheesy and the whole book is superficial and is targeted almost exclusively at women with a partner and children.

The anecdotes ARE funny but then a comedy writer wrote it—not Sheryl. It was actually written by a ghost-writer, Nell Scovell, who incidentally, wrote ‘Sabrina the Teenage Witch’ and other TV screenplays. We suggest that perhaps this book should be just part of the conversation about women.

Read the Davies report to which Dr Ward refers—that is, well-thought-out strategies which can be applied to Boards whether professional or business based. Women can help themselves too of course, but actually everyone can make a difference. The Lean in book has a lot of Sheryl Sandberg’s own experiences in it, but it has little of her soul. Perhaps it would have struck a better note if she had actually written it herself.

Declaration of interest
Both authors are editors of the BJA and members of the BJA Board of Management, and both authors are women.

Anticipation of the difficult airway
Editor—we have with great interest read the article by Cattano and colleagues1 on anticipation of the difficult airway (DA). We would like to thank the authors for addressing this pivotal area of our profession. However, we have some major concerns regarding the used study methodology. We find the trial at risk of (1) systematic errors (bias), (2) random errors, and (3) other design errors.

(1) The study is presented as an individually patient-randomized trial, but is in fact randomized in clusters, each population managed by a resident being a cluster. As the study is conducted on one department, how did the authors control for a potential spillover effect from the experimental to the control group? Residents constituting the control group must inevitably have gained information about trial intervention in the experimental group thus influencing their performance. It opens up for huge bias in comparison of the two groups of patients (should have been analysed in clusters).

(2) The reader is not presented with a sample size or a power estimation, which should have been based on a clearly stated outcome measure and adjusted for intra-cluster correlation because of cluster randomization. The number of patients needed in a cluster-randomization is highly dependent on both intra- and inter-cluster correlation and may exceed the number of patients needed for an individual randomization substantially.2 This may induce huge risks of random errors.

(3) We would like to suggest an alternative primary outcome measure as the difference in correct prediction rate of DA between the two groups, hypothesizing superior prediction accuracy using the ASA guideline. It is unclear whether the authors employed intention to treat- or per protocol analyses. The flow diagram (Fig. 2) and the numbers in the text are inconsistent. Also, we find the definition ‘Accuracy of DA prediction’ (Table 2) somewhat misleading. The presented figures are the number of preoperative airway assessments that are in agreement with the actual airway management. Fortunately, DA is a rare situation, also documented by the authors’ result of 11.97% DA corresponding
to a rate of 88% non-DA. The presented percentages of 71.2 and 69.1%, respectively, are comprised by true positives and true negatives—a vast majority of these being true negatives, that is correctly identified non-DAs. The trial report therefore does not give a clear picture of the prognostic accuracy (‘accuracy rate’) with which the DA is predicted. Further, the level of agreement between preoperative airway assessment and the subsequent actual airway management cannot be assessed by evaluating only true positives and true negatives as random agreement is not taken into consideration this way. Thus, ‘Accuracy of DA prediction’ would be better presented by sensitivity; specificity; positive and negative predictive values, and likelihood ratios. Figures and measurements appear inconsistent and incorrectly calculated in some cases. We find a concerning discrepancy between the study reporting and the recommendation in the consolidated standards of reporting trials (CONSORT) statement3—with which clinical trials published in British Journal of Anaesthesia ought to comply.4 Further, we would like to ask the authors whether a protocol was published or the trial was registered in a trial registry at the beginning of randomization?

Finally, we agree with the authors that a multi-centre trial is needed in order to promote understanding of the optimal use of tools for prediction of DA management. This should be a large trial designed with low risk of bias in agreement with the standard protocol items: recommendations for interventional trials (SPIRIT)5 and CONSORT statements.

**Declaration of interest**

None declared.

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**Reply from the authors**

Editor—We thank Nørskov and colleagues for their interest in our prospective, observational study on airway management and resident education.1 The trial was initiated as a resident education project, continued as a quality improvement project (2005–6), and then supported as a clinical–educational trial by FAER (2007) to assess the efficacy of a comprehensive airway assessment that included all recommendations of the 2003 ASA Difficult Airway Management Guidelines,2 and a previous pilot study.3 Nørskov found our study important because it addressed a pivotal clinical issue in our profession but voiced concern regarding the methodology utilized, claiming the trial was at risk of systematic errors, random errors, and other design errors. We concur with Nørskov that the majority of the points raised are not only valid and legitimate, but offer a point of scientific discussion that otherwise would have been neglected, for the sake of brevity and editing necessities in our original work.

We would like to point out that, in general, any study is at risk for such errors, and that proper design tends to mitigate the occurrence of errors. Technically, there is no such thing as a perfect study. The first criticized point of our investigation was the randomization: this occurred based on the resident year, on a 1:1 allocation, and it assumed an equal distribution in terms of experience, skills, and standard training. However, our study did not evaluate education matters only, but and before all, airway management based on patients features and modalities. Our assumptions were similar training between and within the residents, and also a large sample size. Nørskov’s concern that our study was presented as a patient randomized trial but should have been randomized as subjects in clusters: this could have been an option. A cluster randomized trial is a randomized trial in which patients are not allocated to treatments independently, but randomized to different treatment groups at a ‘group’ level. Therefore, the patients in one cluster may behave similarly to members in the same cluster. In general, analysis of cluster trials needs to take into account the clustered nature of the data, otherwise the risk of a type I error rate inflates. Intra-class correlation coefficient is a statistical measure of the dependence within a cluster. Owing to this intra-class correlation, the sample size needed to achieve a specified power is larger than the individual randomized controlled trials. Ignoring this correlation and assuming the subjects of the investigation are independent will mislead the results and overestimate the treatment effect (narrow confidence interval and small P-values). A limitation that is worth mentioning, but also another reason for deciding not to use a cluster analysis, was the preoperative evaluation was not necessarily performed by the same resident who performed the airway management.

Avoiding a ‘spill of effect’ was in our specific study impractical and not possible. Risk for knowledge contamination, data manipulation (from the experimental resident group), and Hawthorne effect needs to be taken into account. Indeed, certain precautions and study control checkpoints were