exercise data and recovery phase data may further refine this technique.

Declaration of interest

None declared.

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Cardiopulmonary exercise testing and survival after major surgery

Editor—We read with interest the recent article by Colson and colleagues1 on cardiopulmonary exercise testing (CPET) predicting 5 yr survival after major surgery. We would like to raise a few points regarding this article.

First, the authors stratified outcome according to seven classes of surgical intervention. It is worthwhile noting that the upper gastrointestinal patients fared the worst. The 5 yr survival for oesophagectomy is 25% from a tumour survival point of view. It is therefore not surprising that the upper gastrointestinal patients, the majority of whom will have been for oncology surgery, had such a low survival. As far as we can tell from the paper no effort has been made to determine whether these patients died from malignancy or a non-malignancy-related pathology. While malignancy may well have a deleterious effect on muscle physiology, we think that it is erroneous to include the malignancy-related deaths in this outcome data. It could be argued that the cumulative survival for the varying groups of patients simply gets worse as the incidence of operations for malignancy goes up. In order to compensate for this confounding variable, we think it is important that data are represented without malignancy-related deaths.

Secondly, we read with interest that the commonly thought of predictive physiological parameters that are measured during a CPET test were not associated with predicting outcome at 30 days. Many other studies have used both the anaerobic threshold and the VE/VCO2 at the anaerobic threshold as predictors of outcome. The 30 day mortality is the mortality rate most affected by postoperative complications and it is therefore interesting that the 30 day mortality was not predicted in any way by the standard variables used in many other studies to predict a poor outcome.

Thirdly, the authors comment that they do not measure a VO2 peak in patients during their CPET protocol. While the authors do not believe that this is predictive, many other studies have shown that a VO2 peak is strongly predictive of outcome. It is possible that if they had measured this variable, it would have been strongly predictive of 5 yr survival. We feel it is important that this point is reiterated.

Fourthly, the authors found that age was not predictive of outcome. However, to a certain extent, many of the variables are interlinked. The anaerobic threshold is linked to the VO2 peak, which in turn is linked to age. An anaerobic threshold of 9 or 10 may be entirely ‘normal’ in an 85-yr-old patient, while it would not be in a 35-yr-old patient. While they have not found age per se to be a good predictor of outcome at 5 yr, they have found an age-related variable to be important and therefore it is hard to discount the importance of age in this determination.

Fifthly, the authors quote a posterior probability of <75% as showing a weak or evidence against an effect being correlated with outcome. Table 3 indicates that only gender, FECR, and surgery type were associated with a 5 yr probability of >75%. None of the other variables achieved this level.

Finally, we think it is important to note that the authors specifically did not look at subgroup co-morbidity. This co-morbidity could massively swing the data as the authors have not commented on whether the co-morbidity was evenly distributed among the various patient groups.

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Reply from the authors

Editor—We thank Dr Low for his letter and welcome the opportunity to respond to the legitimate issues raised.

As correctly noted, many of our subjects were undergoing cancer surgery.1 To have eliminated these patients from the analysis would have amounted to a selection process, resulting in analysis of a subgroup bearing little resemblance to the cohort of patients generally undergoing major surgery in most western hospitals.

While it is relatively straightforward to identify those patients dying with malignancy, proving that the malignancy actually caused death is much more difficult, and therefore error-prone. We would argue that the distinction is not dichotomous, but rather a continuum in which the interaction between malignancy and patient fitness determines survival.
Indeed, survival itself is not even dichotomous, since—like it or not—we are all on an inexorable decline to our ultimate death. Survival is a special form of continuous variable the analysis of which needs to acknowledge that each day one survives is a day less to live.

Perhaps more importantly, while physicians tend to obsess about causes of death, patients care little for such notions. They are likely to be more interested in how long they will live rather than whether they, for instance, died from malignancy or heart failure. Eliminating malignancy-related deaths from our analysis would have been highly artificial and potentially arbitrary.

We agree that the lack of predictive value of the conventional physiological variables for 30-day survival was surprising. But it needs to be noted that just 2.7% of our study cohort died within 30 days of surgery. In essence, death within 30 days is a relatively rare event which makes the task of identifying this small subgroup statistically challenging.

We did not claim that peak VO\textsubscript{2} was not predictive of outcome—but rather that there was no evidence it was a superior predictor to sub-maximal exercise data. The anaerobic threshold is undoubtedly related to age, and, indeed, co-dependency exists between many of the CPET variables. An anaerobic threshold of 9 is only ‘normal’ in a 85-yr-old because 85-yr-olds are ‘normally’ unfit. The Bayesian model averaging (BMA) technique efficiently filters-out variable co-dependence. If much of the predictive information of one variable was also contained in another variable, BMA will still identify the most predictive variable. Our results show that most of the predictive information contained in a patient’s age is in fact contained in their fitness data. Specifically, when the trillions of models potentially explaining our data were distilled down to the few hundred very best models via the BMA process, 66% of those models predicted survival without requiring the input of patient age. The unexpected nature of this finding is remarkable, and probably explains the correspondent’s difficulty accepting that it could be true.

The apparent inconsistency of the interpretation of the results of BMA can be explained by the differing contexts in which this topic is discussed. In pure statistical texts, a posterior probability of \(< 75\%\) is considered weak evidence. However, in perioperative medicine (and indeed most biological fields), it would be considered strong evidence, given the relative weakness of existing evidence. The discrepancy highlights the fact that Bayesian techniques are not commonly encountered in the medical literature. Some of the associated conventions and subtleties do not readily translate from the hardcore statistical realm into the biological sciences. We believe the interpretation of Bayesian posterior probabilities to be one such example.

We felt it important not to look at subgroup co-morbidity. The charge that we should have included co-morbidities into the modelling would be valid had our study found no relationship between CPET performance and long-term survival. On this point, our results speak for themselves. We deliberately did not include these items, and yet still detected a powerful predictive effect. Clearly, this effect may have been even more powerful had they been included, but then readers may have concluded that CPET was adding little to the risk-stratification process. Clarifying the relative contribution of each component would have added a further layer of complexity to the analysis which we sought to avoid.

**Declaration of interest**

None declared.

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**Cardiopulmonary exercise testing**

Editor—Colson and colleagues’ study\textsuperscript{1} on the predictive power of cardiopulmonary exercise testing (CPET) for 5 yr survival after major surgery found that no single CPET parameter was predictive, and that the parameters measured in CPET were only weaker predictors than general predictors available without CPET. The three major predictors were gender (female in favour of male), type of surgery, and forced vital capacity ratio. These findings are in agreement with survival statistics in general, as regards gender, and with models derived in the past, cited by the authors, with regard to the type of surgery and FVCR.\textsuperscript{2} It caught my attention that the study brought a surprising result: survival was independent of age. This would seem counter-intuitive. Age is a predictive factor in actuarial calculations for life insurance, and appears in some predictive models, also cited by the authors.\textsuperscript{3} Would the authors please comment?

**Declaration of interest**

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