time suggested a comprehensive mathematically derived criterion for assessment of observed variability. Given an inherent variability of ± 20% for each method under comparison, the combined variability (i.e. limits of agreement) should not exceed ±30% of the mean SVV. Applying these strict criteria to the data, agreement of methods studied by the authors was unacceptable. Secondly, the authors compared SV derived from different methods at specified experimental conditions. Unfortunately, no information is provided as to the ability of these methods to reflect changes of SVV consistently. It has been suggested recently to analyse the change in a variable after a specific intervention also with a Bland–Altman plot, comparing the mean per cent change of both methods against the difference.4 This analysis would have revealed quickly, if these methods do track ensuing changes of SVV in a comparable fashion. This is a very important issue, since changes of SVV are thought to reflect a change in the fluid responsiveness of an individual patient. Thirdly, the authors compared methods during a single tidal volume (12 ml kg⁻¹). This is quite surprising, as mechanical ventilation has an important impact on SVV, and consequently agreement between methods may be influenced by depth of tidal volume applied.5

B. Bein*
J. Renner
P. Meybohm
J. Scholz
Kiel, Germany
*E-mail: bein@anaesthesie.uni-kiel.de

Correspondence

Editor—We thank Dr Bein and colleagues for their comments on our recent article and for the opportunity to reply. First, they are correct that Critchley and Critchley3 suggested a very useful approach to ‘quantify acceptable limits of agreement between two measurement techniques’. From a meta-analysis comparing absolute values of clinical cardiac output measurement techniques, they concluded that combined limits of agreement should not be above 30% according to an error-gram. This is a possible point for discussion; however, in contrast to our investigation, this was focused on absolute values of cardiac output and a comparison of two clinical methods vs each other. Secondly, there is no analysis of changes in SVV provided in our article, as no change in SVV occurred after alteration of mean arterial pressure. Thirdly, the influence of tidal volume on functional preload indices is well known,6 7 and thus tidal volume was not altered in our study.

J. C. Kubitz1
D. A. Reuter2*
1 Munich, Germany
2 Hamburg, Germany
*E-mail: dreuter@uke.uni-hamburg.de

B-type natriuretic peptide in high-risk major surgery patients

Editor—We read with great interest the article by Cuthbertson and colleagues1 demonstrating the association of preoperative B-type natriuretic peptide (BNP) levels and adverse cardiac events. In 204 low to intermediate risk patients undergoing major non-cardiac surgery, moderately elevated preoperative BNP levels of 40 pg ml⁻¹ predicted early postoperative death or myocardial injury (area under the ROC curve 0.72; 95% CI interval 0.59–0.86). This article underlines recent suggestions that the biochemical markers BNP and N-terminal pro-brain natriuretic peptide (NT-proBNP) outperform existing cardiac risk scores regarding prognostic importance.2 3 Unfortunately, the short in-hospital observation period of 72 h limits the significance and clinical importance of this hitherto largest blinded trial on the prognostic power of preoperative BNP. Although the majority of myocardial infarctions occurs within the first 48 h after surgery, delayed postoperative myocardial infarction is a well-known complication and may have been missed in this trial. Furthermore, as mentioned in the editorial4 accompanying this study, postoperative determination of NT-proBNP provides additional prognostic information to preoperative levels regarding in-hospital and long-term cardiac outcome.5 BNP and NT-proBNP are released from cardiac myocytes in response to ischaemia or myocardial stretch and plasma levels correlate well with the extent of inducible ischaemia.6 Preoperative ‘stable’ levels of natriuretic peptides therefore do not necessarily reflect the variable dynamic

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consequences of the intra- and postoperative stress response which culminates in adverse in-hospital$^7$ and long-term cardiac outcome.$^8$ Thus, although we recognize that the importance of postoperative NT-proBNP determina-
tion in non-cardiac surgery was just very recently pub-
lished, the significance of the results of Cuthbertson and
colleagues would have been further improved by a pro-
longed observation period and by additional postoperative
BNP determinations.

E. Mahla*
M. Vicenzi
W. Toller
Graz, Austria
*E-mail: elisabeth.mahla@meduni-graz.at

Editor—In reply to the interesting points made by
Dr Mahla and colleagues, we agree with many of the state-
ments made in their letter. However, the measurement of
BNP in the postoperative period does not add any additional
predictive power to preoperative BNP measurement in the
prediction of these short-term outcomes such as early post-
operative cardiac events (unpublished data from same
cohort). Further, our work on the predictive power of BNP
for medium term mortality is about to be published in the
American Journal of Cardiology within the next 2 months.
I am sure the respondents will find this paper interesting.
Although we may have missed some of the postoperative
cardiac events occurring in hospital in this cohort due to
our timing of measurements, we feel that any significant
events will be detected in medium term mortality analysis.
As they state, other work suggests this is the case.

B. H. Cuthbertson* (on behalf of the authors)
Aberdeen, UK
*E-mail: b.h.cuthbertson@abdn.ac.uk

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In defence of early warning scores

Editor—Although we acknowledge the valuable recent
contribution made in relation to physiological scoring by
Duckitt and colleagues,$^1$ it is with some concern that we
read the final conclusions in the accompanying editorial.$^2$
As co-developers of the first early warning scoring (EWS)
system, based on aggregate weighted scoring of physio-
logical variables,$^3$ we must re-emphasize that EWS was
designed solely to secure the timely presence of skilled
clinical help by the bedside of those patients exhibiting
physiological signs compatible with established or impend-
ing critical illness. The original EWS was not presented as
a predictor of outcome. The overall clinical course for most
critically ill patients is punctuated by multiple potential
confounding influences$^4$ making such attempts at final
outcome prediction, on the basis of early routine standard
bedside observations, an unrealistic expectation.

Although the medical emergency team (MET) calling
criteria$^5$ do represent a form of physiological tracking, they
do not include the assignment of weighted numerical
values to the degree of deviation of given physiological
variables from agreed normal ranges, nor do they utilize
any form of numerical score or score trigger value at
which skilled help is summoned to the bedside. By the
early 2000s, EWS had become synonymous with a wide
range of variations from the original, including some non-
aggregate weighted scoring systems. In order to address
this particular ambiguity in terminology, the National
Outreach Forum (NorF) adopted the phrase ‘Physiological
Track and Trigger System’ in 2003.$^6$ ‘Physiological Track
and Trigger System’, as a descriptive, accommodates all
systems which include calling criteria based on any form
of physiological tracking, together with a threshold at
which mandatory assistance is summoned.

Cuthbertson and Smith cite the recent publication by
Gao and colleagues$^7$ and quote the authors as concluding
that ‘there was little evidence of reliability, validity and
utility’ in relation to current scoring systems, and that the
sensitivity of such systems was ‘poor’. Gao and colleagues
based their conclusions on use of the composite outcome
measure of death, admission to critical care, ‘do not
attempt resuscitation’ or cardiopulmonary resuscitation. As
far as we are aware, data available to these authors

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