Left ventricular function and high-voltage electrical injury

To the Editor:

Kim et al recently reported assessment of left ventricular function in patients with high-voltage electrical injury (1). Using transthoracic echocardiography they found no differences with control subjects in terms of left ventricular function using classic tools of evaluation of left ventricular function. The main interest of the work is the use of speckle tracking and analysis of strain and strain rate in such patients. As explained by the authors, this technique allows investigation of the different myocardial segments and analysis of strain and contraction. However, the authors chose to average values of the different segments in their final analysis. Regarding the mechanism of the possible myocardial injury and the hypothesis that an increase of catecholamines secretion may be suspected in their patients, local analysis of myocardial function may be of interest. Catecholamine-induced myocardial dysfunction may concern only some segments of the left ventricle. Tako-Tsubo or stress myocardiopathy was described as a transient left ventricular apical ballooning. A typical akinesia of the left ventricular apex or the midportions of the left ventricle as well as a hypercontractile base are consistently found with echocardiography (2). Pathophysiology of the syndrome may be partially explained by an increased sympathic activity, similar to the hypothesis of the authors. Increased serum concentrations of catecholamines have been shown to generate direct myocyte injury. Patients with high-voltage injury may theoretically present such complication. It would be of interest to know if some patients in the study experienced apical kinetic disorders or strain rate abnormalities in these segments, or if there was a difference between basal and apical left ventricular segments. Additionally, the authors argue that serum troponin is not useful for detecting left ventricular dysfunction in such patients. Considering the small increase of serum troponin reported and the fact that no patient experienced left ventricular dysfunction, the study data do not support such a conclusion.

The author has not disclosed any potential conflicts of interest.

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The authors reply:

We thank Dr. Fichet for his interest and constructive comments on our recent publication (1). Several recent reports have documented that the administration of intravenous catecholamines such as epinephrine and dobutamine resulted in stress cardiomyopathy even in standard doses used in clinical practice (2, 3). Considering that increased levels of catecholamines might have been associated with functional changes of left ventricle (LV) as one of the possible mechanisms in our study, there is a high probability of stress cardiomyopathy in patients with high-voltage electrical injury. To date, all studies on stress cardiomyopathy have demonstrated a marked reduction of LV ejection fraction by conventional echocardiography, as well as significant decrease in systolic strain rate and postsystolic shortening of apical segments by two-dimensional strain rate imaging (4, 5). However, we could not identify any of the patients with specific patterns of the LV regional wall notion abnormalities that are shown in stress cardiomyopathy. Compared with controls, each strain rate of 16 LV segments from the base to apex were evenly increased. In addition, postsystolic shortening of apical segments was not observed in any of the patients. Although catecholamine-mediated myocardial dysfunction has been proposed as one potential mechanism to explain the unusual characteristics of stress cardiomyopathy, the development of this syndrome seems to be influenced by various genetic factors and sex hormones (2, 6). The strong female preponderance might potentially explain why no patients in our study had a stress cardiomyopathy develop after high-voltage electrical injury. Further studies including female patients are needed to see whether stress cardiomyopathy after high-voltage injury could be developed.

Finally, Dr. Fichet has also raised an issue relating to the study's conclusions in the abstract and discussion. The study hypothesis stated that the increase of serum troponin after high-voltage injury would be a useful marker in predicting the impairment of LV function. However, no patient experienced LV dysfunction, although serum troponin levels were increased in some cases. We agree with his comment. As a conclusion of this study reflecting this inference, it may be better to conclude that a small increase of the serum troponin after high-voltage electrical injury is not associated with LV functional changes.

The authors have not disclosed any potential conflicts of interest.

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Steroid use in acute lung injury/acute respiratory distress syndrome: What about the acute lung injury from H1N1?

To the Editor:

We read with great interest the article by Tang et al (1) published in the May 2009 issue of *Critical Care Medicine*. The study analyzed published data over the course of four decades on the early use of low to moderate doses of steroids in acute respiratory distress syndrome (ARDS), and they identified an important "window of opportunity" for its use in patients with ARDS. Their findings demonstrated that steroids, used early and in appropriate doses, improved mortality, length of ventilation-free days, length of intensive care unit stay, multiple organ dysfunction syndrome, lung injury score, and PaO_2/FIO_2, without increasing infection, neuromyopathy, or other major complications (1). Furthermore, the editorial by Meduri et al (2) in that same issue reviewed more data and basic science studies related to infection and the use of steroids, and supported those findings. The recommendation was to use steroids early, in smaller doses (not exceeding 1 mg/kg per day methylprednisolone) and not to discontinue abruptly. Meduri et al (2) dispelled all concerns about superimposed infections. In fact, they proposed a new paradigm in the understanding of the relation of infection and inflammation, in which a dysregulated increase in inflammation is thought to be a source of bacterial infection (2).

Those articles come at a time when our emergency rooms are filling with patients with the H1N1 infection and, as intensive care physicians, we are preparing for a surge in patients with acute lung injury (ALI)/ARDS that may result from this infection. Naturally, the question is whether we should add steroids in the suggested dosage (2) to the therapy of those patients. Regrettably, previous outbreaks did not give us straight answers, as summarized by the World Health Organization (WHO) updated advice in 2007 during the outbreak of avian influenza A (H5N1) (3). Steroids were mostly used haphazardly at high doses and late, and their positive effects could not be discerned. The WHO shied away from recommending their use in patients infected with avian flu and ALI in the absence of septic shock (3).

However, placing the findings of Tang et al (1) and the analysis by Meduri et al (2) in this context of ALI caused by influenza A, the answer should obviously be "yes" for steroid use. In support of their concept of inflammation and infection, it is worth mentioning the findings by Mores et al (4) reported last autumn in the *Journal of Infectious Disease*. In that report the authors examined the lung pathology obtained from 58 patients who died of the "Spanish flu" in 1918 to 1919 and reviewed pathologic and bacteriologic data from published autopsy series that described 8398 individual autopsy investigations. Their findings clearly demonstrated an overwhelming bacterial infection in the lungs that was associated with an enormous level of inflammation (4).

Luckily, so far, our medical intensive care unit admissions of patients with H1N1 have been limited to bronchitis with a dominant bronchospasm that responded quickly to the classic measures, which of course include steroids. However, in case we will face in the future a situation like the "Spanish flu" with ALI or ARDS, early use of steroids should be considered as an addition to the therapy.

The authors have not disclosed any potential conflicts of interest.

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Red blood cell transfusion and mortality

To the Editor:

In their retrospective, single-center study comprising 79 patients with spontaneous subarachnoid hemorrhage and red blood cell transfusion, Broessner et al (1) conclude that red blood cell transfusions are not associated with intensive care unit (ICU) mortality or unfavorable long-term outcome. They present several models with either ICU mortality analysis or Glasgow Outcome Scale at an average of 3.3 yrs after discharge as outcomes. The authors have to be congratulated for the inclusion of a relevant measure of disease severity, namely Hunt and Hess grading, and a functional long-term outcome variable in their analysis. However, we are concerned that their conclusions may be misinterpreted.

In their study, the adjusted odds ratio of red blood cell transfusion for the dichotomized long-term outcome with Glasgow Outcome Scale was 1.41 (95% confidence interval, 0.57- 3.51). The authors interpret their result as no association. Unfortunately, they included no discussion of sample size and the chance of type II error. A more cautious interpretation of the confidence intervals in their analysis would be that the study cannot exclude a clinically significant 3.5-fold increased risk with red blood cell transfusion for an unfavorable outcome assessed by Glasgow Outcome Scale (or a decreased risk, either). In addition, ICU length of stay may be influenced by different treatments, e.g., transfusion practice, and decisions of treatment withdrawal or improvement in the clinical course may cause selection bias with patients. Although the patients who died during the first 3 days were excluded from the additional models, the conclusion that "even pro-

longed ICU care does not additionally harm patients" may still be misleading because sick people tend to die earlier in the course of their disease. In addition, their retrospective review revealed that transfusion threshold varied from >7 g/dL to >9 g/dL in one single unit and in a homogeneous patient group. This raises concerns about important unmeasured confounding factors related to transfusion practice that can also affect outcome (2, 3).

The potential risks of increased mortality, increased infections, and increased ICU length of stay associated with red blood cell transfusions have been extensively highlighted in a recent systematic review (4). Regrettably, the study by Broessner et al (1) did not provide us with sufficient evidence to challenge the literature, and larger prospective data continue to be needed in specific patient groups in the ICU because the current guidelines possibly may not apply to all patients (5). We agree more with Dr Broessner and associates in that the optimal pre-transfusion hemoglobin for different patient groups remains an important and inadequately addressed clinical question.

Dr. Cooper has received a grant from the American Red Cross Blood Service. The remaining authors have not disclosed any potential conflicts of interest.

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The authors reply:

In their response to our original article (1) on transfusion of red blood cells in patients with spontaneous subarachnoid hemorrhage, Pettilä et al have mentioned some interesting thoughts about a possible interpretation of our results.

We agree with the authors that the respective confidence interval (odds ratio, 1.41; 95% confidence interval, 0.57–3.51) cannot fully exclude an increased risk of patients with red blood cell transfusion regarding an unfavorable long-term outcome. From the statistical point of view, this confidence interval allows positive and negative interpretations of the potential effect of red blood cells on outcome; the overall p value, however, is not significant. Therefore, we still believe that in our cohort the interpretation is correct as "no association."

Marik et al (2) clearly stated in their recent literature review that red blood cell transfusions are associated with increased morbidity and mortality in a very heterogeneous patient population. In our highly selective cohort of patients with spontaneous subarachnoid hemorrhage, we could not find a significant association of red blood cell transfusion and neurologic long-term outcome. Whether this finding also applies to different patient populations or even subgroups must be clarified in future studies addressing this question. Further research in this field may influence transfusion policies and might lead to different target values of hemoglobin in various diseases.

The retrospective design of our study has important limitations that must be considered when interpreting the results. To answer the question of the optimal hemoglobin value in such a patient population has to be approached in a prospective manner. Retrospective collection of possibly confounding factors such as infections is methodically not correct and might only add to the existing confusion; therefore, it was not included in our study design.

We share the opinion of Petillä et al that a randomized, prospective trial including even more patients may bring better insight to this discussion and is urgently needed by the international scientific critical care community.

The authors have not disclosed any potential conflicts of interest.

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Predictive models: The angel is in the details

To the Editor:

We enjoyed reading the editorial by Dr. Kramer (1) regarding predictive models. As Kramer reports, there is current interest in developing scoring systems for predicting adverse outcomes (e.g., unanticipated intensive care unit admission, death, preventable cardiac arrest) in ward patients, but existing ones are predominantly based on practical experience and opinion, rather than scientific proof.

We agree that simplicity is desirable in such scoring systems in the absence of appropriate supporting electronic data capture and calculation technology. Such technology has particular requirements in a clinical environment; it needs to be acceptable to front-line clinical staff, should be part of routine clinical practice, universally deployed, and integrated

with the hospital information technology infrastructure. These are now achievable.

Our hospital (Portsmouth Hospitals NHS Trust) has collaborated in the development of such a system (2) with a commercial company (The Learning Clinic, London), and this is now installed in several United Kingdom hospitals. By April 2009, the system, VitalPAC, had facilitated the collection of more than two million complete vital signs datasets from patients nursed outside critical care areas and allowed us to independently evaluate the performance of a wide range of rapid response system trigger criteria (3, 4).

The range of area under the receiver-operating characteristics curve values for the aggregate-weighted track and trigger systems (i.e., Modified Early Warning Score) evaluated using our data ranged from 0.657 to 0.782, using admission vital signs and hospital outcome (3). For the same data and outcomes, the sensitivity/specificity values for the single parameter track and trigger systems (i.e., Medical Emergency Team criteria) ranged (sensitivity/specificity) from 52.8% and 69.1% to 7.3% and 98.1% (4).

We have extended this work to study other outcomes and have developed simple, paper-based, and "complicated" electronic aggregate-weighted track and trigger systems that have values of area under the receiver-operating characteristics curve in excess of 0.882 for death within 24 hrs of a given vital signs set. For the same data and outcomes, the sensitivity/specificity values for the single parameter track and trigger systems (i.e., MET criteria) ranged (sensitivity/specificity) from 76.9% and 66.1% to 18.5% and 98.2%.

Additionally, the system has the ability to automatically include parameters other than vital signs (e.g., demographic factors and laboratory results), so enabling the ". . . assimilation and multivariate analysis of high-dimensional data . . ." that Kramer suggests.

The electronic vital signs data gathering system used in this study, VitalPAC, is a collaborative development of The Learning Clinic Ltd and Portsmouth Hospitals NHS Trust. Professor Gary Smith's wife and Dr. David Prytherch's wife are shareholders of The Learning Clinic Ltd. Dr. Schmidt has a directorship in a UK registered company, Proxximity Systems Ltd, which holds a minority shareholding in The Learning Clinic. Dr. Peter Featherstone has not disclosed any potential conflicts of interest.

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Impact of a prevention strategy targeting hand hygiene and catheter care on the incidence of catheter-related bloodstream infections

To the Editor:

Given our particular interest in the field of infection prevention and control, we read with special interest the multimodal educational before/after study targeting hand hygiene and catheter care by Zingg et al in the latest issue of *Critical Care Medicine* (1). This study adds to the literature that despite a rather limited increase in hand hygiene compliance, an educational effort is able to significantly improve the rate of correctly executed hand disinfection procedures. Furthermore, the authors could demonstrate that the implementation of uniform procedures with regard to strict hand hygiene and catheter care, respectively, dressing of the insertion site, manipulation of tubing, and aseptic preparation of infusates resulted in a >70% decrease of the incidence of intravascular device-related bloodstream infection. The authors consider the bedside-oriented educational approach they implemented to be of key importance to the favorable results obtained (1). Although we generally agree with the strategy proposed, we would like to comment on a few additional related issues. Research evaluating knowledge, attitudes, and perceptions of undergraduate nurse students and critical care nurses about evidence-based guidelines for preventing nosocomial infections showed rather poor findings (2–4). Therefore, the need for a more thorough teaching of evidence-based guidelines/practices as an essential part of the basic nursing curricula was highlighted, because we believe that this may provide a solid basis for continuing refresher courses and specific bedside training (5). As such, the favorable impact of additional interventions may be higher if the underlying principles are better understood. Although we are aware that knowledge does not ensure compliance, we are convinced that it is definitely an elementary condition.

A remarkable finding of the current study is that although the rate of correct performance of the technique for hand hygiene almost doubled during the intervention period (from 22.5% to 42.6%; $p = .003$), compliance did not follow this leap forward but only slightly improved as compared with the baseline period (from 59% to 65%; $p = .466$). This finding reflects the results of a study by our group that focused on identifying predictors and determinants of noncompliance with hand hygiene prescriptions among critical care nurses and found that there were no associations between a good theoretical knowledge and moral perceptions toward hand hygiene practice. Interestingly, in our study, nurses reporting a poor self-efficacy or a poor attitude toward time-related barriers appear to be less compliant (6). These findings may be useful for the future implementation of improvement initiatives targeting hand hygiene compliance in the critical care department. Finally, deterioration of the success of improvement interventions over time is well known. Therefore, we wonder if the authors have planned any specific strategy and goals to sustain a long-term intervention effect. We kindly invite Zingg et al to elaborate

on the issues described and wish them the best of success in consolidating, and hopefully improving, the obtained results in their hospital.

The authors have not disclosed any potential conflicts of interest.

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The authors reply:

We thank Vandijck et al for their appreciation of our work (1). We agree that theoretical knowledge about infection control may not change practice, but a basic knowledge is indispensable to understanding the rationale behind the concepts. Although we do not have individual data, it can be speculated that hand hygiene teaching in our study improved the hand rub technique among those healthcare workers who were already high performers. Whether a healthcare worker is a high performer or a low performer is determined by factors other than knowledge alone. Self-reporting of noncompliance with hand hygiene revealed factors such as insufficient time for hand hygiene, understaffing, overcrowding, the idea that the patient needs take priority over hand hygiene, not thinking about hand hygiene when needed, hand hygiene interfering with the healthcare worker-to-patient relationship, or the lack of a role model among colleagues or superiors (2, 3).

Daily intensive care unit work is becoming increasingly complex, and because complexity is associated with more severe incidents, as is low nurse-to-patient ratio, critically ill patients are particularly at risk for healthcare-associated infections (4, 5). Thus, the finding of Vandijck et al that nurses with a poor attitude toward time-related barriers are less compliant with hand hygiene makes sense. In a complex environment, attention may be drawn away from hand hygiene, and healthcare workers who are less skilled in work organization are more prone to experience stress. The answer would be to reduce the complexity of the intensive care unit work process, but this is often not possible. For this reason, we need infection control measures that are simple and can be easily integrated into daily practice. Furthermore, to ensure that hand hygiene and other prevention measures are observed in daily routine and under stress conditions, we must also ensure that nurses and physicians adopt an infection control culture. Hand hygiene should be as natural as breathing. Culture change is a difficult task to achieve and even multimodal strategies may fail. We must focus on teaching infection control in nursing and medical schools because there is a tendency in people to keep doing things the way they were told to for the first time. So, let us make sure that these students hear the message from infection control experts right from the start. Based on such a natural adoption and internalizing of infection control practices, refresher and bedside teaching courses may become more successful in reinforcing the message.

As to sustainability, we share the concerns of Vandijck et al regarding our ability to maintain the success achieved through our intervention (1). We are planning to involve healthcare workers of individual units in our teaching activities with the aim to further bridge the gap between infection control and the teams directly working with the patient. With this peer-based teaching, we aim to address prevention measures on a regular basis throughout the year and expect that this approach, combined with regular feedback of nosocomial infection rates, should contribute to a stable compliance with infection control measures in our setting.

The authors have not disclosed any potential conflicts of interest.

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