are being fruitfully cultivated. Superficial simplicity and holistic elegance (e.g., a single elementary basis of consciousness for slime mold and Homo sapiens) couched in awe-inspiring quantum terminology, drawing support from eclectic collections of arbitrarily selected random observations from all spheres of science, render unitary theories popular among revelation-seeking consumers of popular science. The disconnectedness of such theories from contemporary neurobiology in general and anesthetic mechanisms research, in particular, remains largely overlooked by their aficionados. In complete disregard of Carl Popper’s thoughts, formulation of testable hypotheses (not to speak of actual experiments aimed at falsifying these theories) has never been a notable strength of unitary approaches. But are (sub)molecular unitary theories irrevocably dead? Are there levels of integration in complex nervous systems where a common network mechanism might be identifiable? My personal answer is a “probably yes” to both questions.

Let us also keep in mind that lack of recognition among contemporaneous peers, per se, does not invalidate a theory. The history of science provides many well-known examples of initial ridicule followed by delayed vindication. However, it is frequently forgotten that the unglamorous trash heap of science is overfilled with theories that were both dismissed by peers and did turn out to be abysmally wrong (anesthetic action by lipid elution being a handy example).8 In summary, however, the passage of time and the application of the scientific method9 will relegate barren ideas to the footnotes of history—be they unitary or multifactorial.

Misha Perouansky, M.D., University of Wisconsin School of Medicine and Public Health, Madison, Wisconsin. mperouansky@wisc.edu

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Washing May Dilute the Results…..

To the Editor:
Weber et al.1 published data that point-of-care testing guiding hemostatic therapy reduced allogenic blood transfusions in patients undergoing complex cardiac procedures. The authors need to be commended on providing strong evidence of “qualitative” over “quantitative” testing. I would like to point out two important considerations that may affect the interpretation and applicability of the results.

1. Patients in the conventional group received approximately twice the amount of salvaged washed erythrocytes intraoperatively. This indicates that patients in the conventional group had twice the amount of intraoperative blood loss and double the amount of “lost” blood cleared of clotting factors. Using rough approximations and excluding any blood loss through the washing 1.5 blood loss in the conventional group, versus 760 ml in the point-of-care group (lost Hct 50, retransfused after washing Hct 57).2 could have clearly affected coagulation in the conventional group leading to an increased transfusion requirement.

2. It is unclear if the cost of unnecessary testing was included in the analysis. Patients were enrolled and randomized preoperatively to start the algorithm with testing for the clopidogrel effect with the Multiplate' (fig. 1C, original article). Then after the release of the aortic cross clamp, the intrinsic and extrinsic coagulation pathways were tested with the ROTEM. For an $MCF_{FIB} = 0$ mm fibrinogen (25 mg/kg) was administered before protamine was given. These interventions were done before a patient could show any signs of diffuse bleeding. This cost would reduce the “cost benefit” of the point-of-care testing. Most likely this would not have substantially influenced the cost as shown in a retrospective analysis when point-of-care testing is applied to all cardiac patients.3 But this is important to consider if physicians want to implement the algorithm. There may be substantially less cost savings if only a relatively small number of high-risk, complex, cardiac procedures are being performed in your institution. This also applies if there are only a few patients requiring blood transfusions after cardiac surgery.

Patrick Ziemann-Gimmel, M.D., Coastal Anesthesiology Consultants, St. Augustine, Florida. pziemann@yahoo.com

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Choosing Inclusion Criteria and Publishing Mortality Data: A Critique

To the Editor:
We would like to make some remarks about the article by Dr. Görlinger and his colleagues. Their study demonstrates vividly the primary end point: Hemostatic therapy based on point-of-care testing reduced the number of packed erythrocytes after cardiac surgery. The study has a very straightforward and precise design. We appreciate the structure of the algorithm even though the progression of the therapeutic options, analyzed step by step, is quite different from the one chosen by our group.

However we disagree with one of the two inclusion criteria outlined. We consider the second criterion reported—“intraoperative or postoperative blood loss exceeding 250 ml/h or 50 ml/10 min”—is very precise because it leads to a reproducible choice of the sample from the population. On the contrary, the first criterion is absolutely dependent on the personal assessment by the singular physician looking after the patient: “diffuse bleeding from capillary beds at wound surfaces requiring haemostatic therapy.” Moreover, it seems to be in conflict with the following algorithm of management that aims to investigate if the patients need treatment for bleeding issues and to assess which therapeutic option to choose.

Our second point of criticism is on the description of the results regarding numerous different secondary outcomes. We agree that many of them may be considered very interesting and probably close to statistically significant results, such as the decreased number of fresh frozen plasma and platelet concentrate units transfused in the point-of-care group. We always have to consider that the sample size analysis and the interim analysis reveal the sample size required to statistically demonstrate the primary outcome, and not other outcomes.

Regarding this, we think that publishing the Kaplan-Meier curve is misleading. In this article, this curve demonstrates a survival rate completely different from the one reported in a lot of other articles on the mortality in cardiac surgical patients.2,3

According to this Kaplan-Meier curve, the mortality rate after six months in complex cardiac surgery reaches the value of 20%.1 On the other hand, we noticed that the authors have chosen another important study as a main reference published in 2007 in Circulation.2 In this article, the Kaplan-Meier curve shows a mortality rate around 6%.2

We think that proving a statistically significant reduction in exposure to allogenic blood products in patients treated with hemostatic therapy based on point-of-care testing is interesting, as some previous studies demonstrate an increased mortality in patients transfused in the same surgical context.

Nevertheless, we think that publishing a misleading graph may not help the reader or give value to the notion demonstrated.

Giovanna Colombo M.D.,* Arshad Ghori M.D., *Royal Brompton Hospital-NHS-London, London, United Kingdom. giocolombogio@libero.it

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