Letters to the Editor

The Editor welcomes submissions for possible publication in the Letters to the Editor section that consist of commentary on an article published in the Journal or other relevant issues. Authors should:

- Include no more than 500 words of text, three authors, and five references
- Type with double-spacing
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The role of propensity scores should be discouraged when there is too little and too early of new surgical techniques

To the Editor:

Propensity scores in medical application have an important role in balancing the confounder effect of recruited variables and help in delivering a sound message. In an editorial written by Donald Rubin (developed propensity scores) entitled "On principles for modeling propensity scores in medical research,"[1] the author emphasized 3 aspects of this work: (1) that diagnostics for the successful design of observational studies should be based on estimated propensity scores by using logistic regression; (2) observational studies should be designed in analogy with the way randomized experiments are designed; and (3) distributional balance is of great importance when covariates are achieved within blocks (strata, subclasses, or matched pairs, as in the work of Kamiya and associates[2]).

In the article by Kamiya and associates,[2] the propensity scores model–based adjustment relating to outcome was dependent on a set of covariates. Although the intentions of the authors satisfied component 2, we are concerned that components 1 and 3 were not satisfied. The propensity score simply took the place of the full set of covariates. Despite the earlier recommendations, it remains disappointing to see that the article selected for review appeared simply to perform a linear covariance adjustment for the estimated propensity scores.[1] This is an inferior approach, and its conclusions are questionable.

The authors state that "In view of the marked and significant difference in patient characteristics between the groups, patient matching seemed necessary to evaluate the genuine effect of temperature on morality and morbidity." But how was that temperature chosen, despite the recent change in protocol for each patient?

After matching 92 patients in each arm (study vs control; total, 184) of the 377 total patients, a considerable amount of missing data existed (approximately 50%), questioning the validity of the conclusions.

I fully agree that there is always room for innovation and change in our field. I am sure that the temperature at which aortic surgery is performed is clinically important and, at different temperatures, variable results are feasible per the published study. However, when application of a new statistic is utilized, adequate data to recruit this statistic is of essence. In this paper about normothermic aortic arch surgery, however, to increase the temperature during aortic arch repair and conclude that it is as safe as hypothermia surgery is not justifiable given the methodology. Too early of a new statistic such as propensity score otherwise would risk making premature conclusions. It would be important to state that in the propensity score matching utilized, the number of events per variable and variable selection method were not performed. There was no mention of how the interaction inclusion criteria such as colinearity were investigated (none were mentioned). If the major goal of publications is to change clinical practice for better, it is pivotal that the methodology both surgi-

cally and statistically is robust and reliable.

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Reply to the Editor:

We are grateful for the opportunity to respond to the letter by Dr Shuhaiber. We appreciate the comments on our recent article reporting moderate hypothermic lower body circulatory arrest with selective cerebral perfusion.[1] His comments can be summarized as follows: (1) the propensity score analysis was not applied properly in our study; (2) too many patients were excluded after matching when using propensity score analysis; and (3) therefore it cannot be concluded that the moderate hypothermic circulatory arrest is safe for aortic arch surgery, and more data should be accumulated for this conclusion.

Regarding the first point, Dr Shuhaiber pointed out that the propensity score simply took the place of the full set of covariates in our study; therefore, it was an inferior approach, and our conclusions were questionable. As Blackstone[2] described, the propensity score analysis is not the best method of comparison because it cannot account for unknown variables affecting outcome that are not correlated strongly with measured variables. Weitzen and associates[3] demonstrated a limitation of propensity score analysis in their systematic literature review and described that many researchers are uncertain as to which criteria for logistic regression modeling used to generate predictions or for estimating adjusted treatment effect estimates are important with respect to estimating a propensity score. We agree with the mentioned limitation of the propensity score analysis and should admit that the factors involved in our study to generate the propensity score might not be all factors that can affect outcome. However, almost all known factors that can affect outcomes of aortic arch surgery were involved in modeling of the propensity score in our study, and we therefore consider that propensity score analysis was properly performed.

Regarding the second point, Dr Shuhaiber stated that the validity of the conclusions are questionable because approximately 50% of patients were excluded from the propensity score analysis in our study. However, Weitzen and associates[3] reviewed 47 studies using propensity score analysis and found that more than 50% of patients were unexposed to the propensity score analysis in 38 studies. Thus we disagree with his statement. Moreover, not only the propensity score analysis but also an analysis of the entire study cohort without matching was performed in our study to avoid small study samples of patients.

Regarding the third point, we should emphasize that our study was designed as a retrospective study. We did not change our intraoperative strategy to write an article; on the contrary, the moderate hypothermic circulatory arrest strategy with selective cerebral perfusion is our ongoing concept, with favorable results at Hannover Medical School.[1,4] When and how our concept should be presented is a philosophical question, and we could wait until the number of patients operated on according to our current strategy would become 1000 instead of 252, as in our article. The number of patients might be too little and the timing of presentation might be too early, as mentioned by Dr Shuhaiber. However, we presented our institutional experience with an analysis using propensity scores because there has been no comparative study of moderate hypothermic circulatory arrest with reasonable control groups in the literature. Here we have obtained evidence by use of propensity score analysis that moderate hypothermic lower body circulatory arrest with selective cerebral perfusion might represent a safe technique.

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