reason why PETCO2 rises above the time-averaged PaCO2. It remains controversial whether “anomalous” CO2 exchange or delayed chemical equilibration of CO2 stores in postcapillary blood also contribute to the reversal of CO2 gradient.1

In conclusion, we would invert the question by Liu et al (p 1223) to read: does PET-aCO2 correlate so well with Vd/Vt? We suspect that workload (i.e., metabolic rate) is the main determinant of this relationship under the conditions studied, and that statistical artifact led the authors to a faulty interpretation. The presence of an (overestimated) correlation between CO2 gradient and dead space does not necessarily mean that dead space “determines” P(ET-a)CO2 in the physiologic sense.

A. Ross Hill, MD,
Division of Pulmonary Medicine; and
Peter Homel, PhD,
Scientific and Academic Computing Center,
State University of New York Health Science Center,
Brooklyn

REFERENCES

To the Editor:

We appreciate the thoughtful letter by Hill and Homel regarding our recently published article on the end-tidal arterial PCO2 gradient. Their primary criticism of our article is that a calculation was calculated between two calculated variables, dead space to the tidal volume (Vd/Vt) and the arterial carbon dioxide concentration (P(ET-a)CO2), which share a measured variable (PaCO2). We certainly agree that such a regression analysis can produce a significant correlation when none exists in certain situations. Nevertheless, we believe that our study shows that there is a significant correlation between the P(ET-a)CO2 gradient and the Vd/Vt for the following reasons.

First, the pseudorandom data and subsequent analysis by Hill and Homel is very interesting. They find a correlation coefficient of 0.35 in their generated data. The correlation coefficient of 0.35 indicates that 12% (0.35x0.35) of the variance in the P(ET-a)CO2 can be explained by the use of the shared variables. However, in our study the correlation coefficient was 0.86, which indicates that 74% (0.86x0.86) of the variance in the P(ET-a)CO2 could be explained by the relationship. If we subtract the 12% attributed to the use of the shared variables from the total of 74% explained in our study, 62% of the variance in the P(ET-a)CO2 can still be attributed to the relationship. This would yield a correlation coefficient of 0.78, which would still be higher than any of the other correlation coefficients in Table 2. Second, the relationship between the P(ET-a)CO2 and the Vd/Vt is not what one would expect if the relationship only depended on the shared variables. For example, if the PaCO2 decreased from 40 to 30 mm Hg, then the P(ET-a)CO2 would increase by 10 mm Hg. If the Vd/Vt was 0.50 originally, it would decrease to 0.33 as calculated from Bohr’s equation with the assumption that the mixed expired PCO2 remained constant. However, from Figures 1 and 2 it is seen that with a 10 mm Hg increase in the P(ET-a)CO2, the Vd/Vt is expected to fall to 0.20.

A second criticism by Hill and Homel is that we combined data from two different experimental conditions. However, the results were quite similar with both conditions as we state in the article. Moreover, if the data from only one of the experimental conditions are used, the conclusions will be nearly identical.

The letter also comments on our proposed mechanisms to explain values of PETCO2, which exceed PaCO2. We essentially agree with their comments.

In summary, the primary point of the published article was that changes in the PETCO2 during exercise are not correlated closely with changes in the PaCO2. Therefore, the PETCO2 cannot be used as an accurate index of the PaCO2 during exercise. We never meant to imply that the dead space “determines” the P(ET-a)CO2 gradient in the physiologic sense. Nevertheless, we believe that there is still a relationship between the P(ET-a)CO2 gradient and the Vd/Vt when the possible contributions of shared variables are taken into consideration.

Richard W. Light, MD, FCCP, and
Scott A. Sasse, MD,
University of California, Irvine

ACCP Consensus Conference on Antithrombotic Therapy
Indicate Specific Low Molecular Weight Heparin Product and Dosage

To the Editor:

We would like to commend the editors of the Fourth ACCP Consensus Conference on Antithrombotic Therapy (CHEST 1995; 108[suppl 4]:225S-222S) for another excellent and clinically useful resource. However, we would like to express our dissatisfaction with the recommendations for prevention of venous thromboembolism (CHEST 1995; 108[suppl 4]:3125-344S). Our concern is that recommending low molecular weight heparin (LMWH) for prophylaxis without specifying which agent to use and at what dose is inappropriate. There are at least six different LMWH products being used worldwide, and they are not identical and should not be considered interchangeable. The recommendation to use LMWH is not specific enough to be useful in clinical practice.

As with unfractionated heparin, LMWHs are heterogeneous mixtures of heparin molecules with differing molecular weights, variable antifactor Xa-to-antithrombin activity, and unique pharmacokinetic properties. Clinical studies that demonstrate the efficacy of one LMWH product for an indication do not mean that all LMWHs are equally safe and effective for that indication. Therapeutic equivalency may be demonstrated for many LMWHs in the future, but in the mean time, we should not assume it to be the case. If the ACCP guidelines are intended to “assist clinicians in decision-making for their patients…” and to “identify appropriate dosages of antithrombotic agents…” (CHEST 1996; 108[suppl 4]:225S-222S), then more specific drug and dosing information than LMWH for prophylaxis should be provided. We hope that in future updates of the ACCP guidelines on antithrombotic therapy the participants will indicate the specific LMWH and dosage for each indication and abandon the generic recommendation of LMWH.

Bob L. Lobo, PharmD, and
William L. Greene, PharmD,
Methodist Hospital Central,
Department of Pharmacy,
Memphis, Tennessee

Communications to the Editor