syncope, and not overdose of chloroform. The exact cause of this type of sudden death—fear combined with light chloroform anesthesia producing ventricular fibrillation—was elucidated by Levy in 1912.[6]

By September 1848, 6 months after Hannah Greener's death, Simpson[7] had collected 743 cases of chloroform administration in his own and his colleagues' obstetric practices. There had been no deaths from any cause. This could have been due to underreporting, although, in view of religious and medical opposition[8] to the use of anesthetics in obstetrics in 1847–1848, it would be surprising if a maternal death under chloroform escaped notice. The first detailed description of death from inhalation of gastric contents was published in 1862,[9] 14 yr after Hannah Greener's death. Even with untrained anesthesiologists, therefore, and in the absence of tracheal intubation, death from inhalation of gastric contents was uncommon. It is now a very rare occurrence, and Coté is right to question the enormous amount of time and effort that has been expended to prevent a problem which, in healthy elective patients, may not be clinically important.

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*In Reply:*—Dr. Maltby is certainly correct that Hannah Greener did not aspirate gastric contents.[1] This case is an indication for how many years anesthesiologists have been concerned with pulmonary aspiration, whatever the etiology. As I wrote, the patient may have aspirated brandy, since that was suggested by the words used by Simpson; my reference to this case was a clarification of what has been reported as pulmonary aspiration.[2,3] It is always interesting to follow-up index cases such as that of Hannah Greener. She apparently also could have been the victim of ventricular fibrillation associated with light chloroform anesthesia.

In the original report, Simpson quoted Mr. Meggison, "I gave her some brandy, a little of which she swallowed with some difficulty." Her father testified that "she moaned after the nail was off; he (Mr. Meggison) afterwards put some brandy in her mouth, and she rattled in her throat." Simpson went on to state, "The attempt at swallowing was . . . I have no doubt an attempt at breathing . . . but it was impossible . . . in her weak and torpid state . . . to inspire through a medium of water and brandy . . . and . . . the liquid would be partially drawn into the larynx (she rattled in her throat)."[1] The autopsy reported "lungs in a high state of congestion . . . bronchi filled with bloody froth . . . mixed with mucus, and a reddened larynx and epiglottis." These findings could be consistent with either aspiration of the brandy or pulmonary edema secondary to attempts at inspiration against an obstructed airway.[4]

The true cause of death in the Hannah Greener case still remains speculative. The bottom line is that anesthesiologists have been appropriately concerned with airway-related events for over 100 years. However, many of our best lessons are learned by reexamining previous experience.

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# Linear Regression Is a Poor Descriptor of Accuracy

*To the Editor:*—Urbanowicz and colleagues erroneously conclude that transesophageal echocardiography (TEE) provides a reasonable estimate of ejection fraction (EF) after cardiac surgery.[1] This incorrect conclusion is based primarily on the common yet mistaken impression that good association implies good accuracy.

Linear regression is a poor descriptor of accuracy.[2–4] Its proper uses include: 1) determination of causal relationships and 2) derivation of predictive algorithms. The correlation coefficient (r) does not quantify *agreement;* rather, it describes the strength of the association between the two sets of data. For example, suppose Urbanowicz *et al.* found that EF by TEE was always *exactly twice* EF derived by scintigraphy. The r value would be 1.0, indicating a very strong association between the behavior of these two measurements, and yet TEE would hardly be considered a good measurement of EF. The scatterplot in figure 2 of their paper shows the poor relationship between the line of identity and the regression line. One can see without any statistical treatment at all that TEE is most assuredly neither a reliable nor a reasonable estimate of EF by scintigraphy. Their regression analysis simply tells us that if we multiply the scintigraphic EF by 0.5 and add a fudge factor, we can to a certain extent ($r = 0.82$ is hardly commendable) predict the value of EF by TEE. Is this accuracy?

The statistic we really are interested in is the error, or difference between the standard (scintigraphy) and the experimental value (TEE). In this regard, it would have been better to describe the mean error, and the magnitude of the mean error. Two values are necessary to provide optimal description.[2] If the errors are evenly distributed above and below zero, the mean error will be close to zero, leading to the erroneous conclusion that the device is extremely accurate. What the mean error yields is the tendency to over- or underestimate the standard value. If one converts all errors to their absolute values, the mean of these absolute errors provides an estimate of the typical magnitude of the error. Using the mean error and mean absolute error, one would be able to state quantitatively what is subjectively apparent from the relationship of the data to the line of identity in the aforementioned figure 2: that TEE is close to scintigraphy when EF is low ($< 0.5$), but underestimates it when EF is normal or high ($>0.6$).

EF by area should underestimate EF by volume except in the unusual case of a contracting cylinder of constant height (see appendix). The relationship depicted by Urbanowicz *et al.* in figure 2 shows that TEE overestimates *radius* changes at low EF. Ironically, the agreement between TEE and scintigraphy at low EF indicates a malfunction of TEE, since they should not agree. The underestimation at high EF is the expected relationship. In other words, at low values of EF, TEE is so bad that it's good, and at normal or high EF, it is so good that it's bad. The problem is, when is it good or bad? Draw a horizontal line across figure 2 at about the point where EF by TEE = 0.55, and then drop vertical lines from the leftmost and rightmost points crossed by the line. It can be seen that when TEE determined the EF to be in the range of 0.55 it actually was somewhere between 0.45 and 0.8 (roughly speaking).

If I have failed thus far to convince the reader that linear regression is indefensible, I offer the following regression-based argument against the conclusion that TEE provides a reasonable estimate of EF by scintigraphy. Figure 2 really does not look like a linear relationship; a second-order regression yields a better value for r (0.89) than does a first-order fit (0.82). Thus, the relationship is better described as nonlinear, and actually curves concave toward the x axis, as simple mathematics predict. No matter how one analyzes these data, they still show that there is no simple linear relationship between EF by TEE and scintigraphy.

I further point out that it is questionable practice to have lumped all data together for analysis, since some data are related to each other and some are not—that is, some data were taken from one patient, and some from another. Since there were unequal numbers in each group, there is great potential for skewing the data. For example, all but one data point for scintigraphic EF greater than 0.6 came from only three patients (5, 7, and 10), and not from the entire group of ten patients. Patient 5 contributed five points to the regression; patient 7 contributed 4; and patient 10, 3 points. Patient 2 contributed a single point to the entire data set. Patient 5 had a far greater influence on the regression results than did patient 2.

I also question the data presentation in figure 1. Here we see a plot of end-diastolic area against length, but it is treated by the authors as if it shows an area–volume relationship. Left ventricular end-diastolic volume index has units of length, and not volume ($\mathrm{ml/M^2} = \mathrm{length^3/length^2} = \mathrm{length}$). It is hardly surprising that a simple linear relationship did not unfold.

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## Appendix

The best case, as mentioned above, is that of a contracting cylinder whose height is unchanged throughout the cardiac cycle. In this unique circumstance, EF by area will equal EF by volume.

Let $r_s$ = end-systolic radius, $r_d$ = end-diastolic radius, and H = cylinder height (constant). Then:

$$EF_{area} = (\pi r_{d^2} - \pi r_{s^2})/\pi r_{d^2} = (r_{d^2} - r_{s^2})/r_{d^2}$$

$$EF_{volume} = (H\pi r_{d^2} - H\pi r_{s^2})/H\pi r_{d^2} = (r_{d^2} - r_{s^2})/r_{d^2}$$

$$EF_{area} = EF_{volume} \quad \text{(If H varies, this simple relationship does not hold).}$$

The worst case would be a contracting sphere:

$$EF_{area} = (\pi r_{d^2} - \pi r_{s^2})/\pi r_{d^2} = (r_{d^2} - r_{s^2})/r_{d^2}$$

$$EF_{volume} = (4/3\pi r_{d^3} - 4/3\pi r_{s^3})/\ 4/3\pi r_{d^3}$$

$$= (r_{d^3} - r_{s^3})/r_{d^3}$$

$$EF_{area}/EF_{volume} = (r_{d^3} - r_d r_{s^2})/(r_{d^3} - r_{s^3})$$

Note that $r_d3$ appears in both the numerator and denominator. Since $r_d > r_s$, $r_d r_s 2 > r_s 3$, the numerator will always be less than the denominator and $EF_{area}$ will always be less than $EF_{volume}$. Ventricular geometry is complex, but it certainly is not a cylinder of constant height. Therefore, $EF_{area}$ will always be less than $EF_{volume}$ since the behavior will lie somewhere between that of a sphere (worst case) and that of a cylinder of constant height (best case).

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