Dear Editor:

June 1, 1992

Dr. Stotter and her colleagues at the Royal Marsden Hospital and ourselves have had an ongoing debate about the influence of local recurrence on the risk of metastatic spread from soft tissue sarcoma. We appreciate that Dr. Stotter now accepts that there is "... no evidence of such a link from their (our) data." We would, however, not accept so easily the idea that we have ignored the proper statistical approach to this very difficult problem and allowed ourselves to make misleading statements.

Our data do support that the risk of death from disease in patients with soft tissue sarcoma is from the development of metastatic disease, not from the development of local recurrence. Perhaps the strongest argument against local recurrence influencing outcome in soft tissue sarcoma is our prospective randomized trial examining a modality that decreases local recurrence.[1] In that randomized trial, we demonstrated a statistically significant decrease in local recurrence without subsequent decrease in metastatic disease. No improvement in long-term survival has been demonstrated. We think that this argues against local recurrence being a major feature in the development of metastatic disease in soft tissue sarcoma. Dr. Stotter is correct that the current manuscript was not designed to answer the question of whether local recurrence influences survival. The relationship of this and other papers by ourselves on soft tissue sarcoma cannot, of course, address the issue in breast disease that Dr. Stotter raises, and on which we have no personal data.

In the same way that she criticizes us, we would think her statement, ". . . effective local treatment is likely to determine long-term outcome," is equally unsubstantiated. We appreciate Dr. Stotter's ongoing interest in our work.

Reference

1. Brennan MF, Casper ES, Harrison LB, et al. The role of multimodality therapy in soft tissue sarcoma. Ann Surg 1992; 215:269–275.

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Dear Editor:

May 1, 1992

We read with interest the article by Chen et al., "Human liver regeneration after major hepatectomy," Ann Surg 1991; 213: 227–229. They measured the area of the liver parts by planimetry with the computed tomography (CT) scan film. The volume of the liver and each part were calculated by multiplication of the area of each part by interval thickness and magnification index and addition of the interval volume of each part. They emphasized that the quantitative CT technique was sufficiently accurate for clinical use. We agree with their comment.

Hepatic resection is increasing. Most cases show various degrees of liver dysfunctions. It is, therefore, important to estimate the residual liver volume before surgery, to select the line of resection. It is common practice to use a plotting paper or to measure CT scan area as this article, thereby using computer software to obtain the estimation of the liver volume.

We have developed a simple, rapid, and accurate method of estimating the liver volume from CT scan film.

The method consists of the following three steps: (1) tracing on semitransparent weighing paper the contour of the liver and resection line of each slices of CT scan film; (2) simply cutting them out with scissors along the liver contour and the resection line; and (3) weighing all of them (A) and those of residual liver (B) with a chemical balance. The residual liver volume (per cent) is calculated by a ratio ($B/A \times 100\%$). The merits of our method are as follows;

1. The weighing paper is easily available, inexpensive, and fits the area of each slice of CT scan film.
2. Tracing, cutting, and weighing are simple and easy. No special technique or training is required.
3. The sheets of weighing paper are constant in weight from one to another.
4. CT scan hardware and software are not necessary.
5. Resection lines may be drawn at our own will and may be modified as many times as desired.

We applied the method to many cases. For instance, in a case of hepatoma, which needed an extended right lobectomy for radical operation but in which the liver functions were impaired, the residual liver volume after extended right lobectomy was 19.9% by CT scan area-measuring software and 19.2% by our method, and that after right lobectomy was 29.1% and 28.9%, respectively.

Our method is thus simple, inexpensive, repeatable, time saving, and accurate and need no special training or technique, which can be used in routine clinical work.

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Dear Editor:

June 3, 1992

Thank you for your letter on May 14. As a response to the letter from Dr. Kobayashi et al., my opinion is that the method they describe seems to be simple and feasible, but the quantitative computed tomography (CT) technique has its advantages.

The CT scan software is usually available and ready to use, the area counted by the computer is also very quick and simple. The most important aspect is that the CT-measured area is more accurate to stand for the exact postoperative liver volume than their simple method using preoperative estimation from the section line of CT scan film at their own will.

I agree that if the CT scan software for the planimetry is not available, the simple method such as Dr. Kobayashi et al. describe will be feasible and has more advantages than the method measured by the sonogram. Besides, the simple method is more suitable for the preoperative estimation of residual volume before surgery, as they mention.

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