Letters

Helical CT Angiography of Thoracic Outlet Syndrome

We read with interest the study by Remy-Jardin et al. [1] describing findings of helical CT angiography with thoracic outlet syndrome. Their elegant study of 79 patients shows the usefulness of helical CT in portraying this complex anatomic region. Furthermore, their detailed morphometric analysis helps to objectively quantify the functional changes with arm movement in the region. However, we disagree with their claim that this is the first in vivo evaluation of functional anatomy. In fact, several groups of investigators have published findings using helical CT [2], conventional CT [3], duplex sonography [4], and venography with pressure measurements [5]. All of these studies found vascular compromise in patients when the arm was placed under a functional stress. The findings of the larger French study would better appreciated if discussed in the context of the existing knowledge that it reinforces and builds on.

Our group has also studied helical CT in healthy, asymptomatic volunteers. We found noteworthy compromise of the costoclavicular space, as well as marked venous compression. These findings have been seen in other studies of asymptomatic arms [5–7]. We are surprised that Remy-Jardin et al. have discovered a statistically significant difference between the maximum clavicle and first rib distance in their symptomatic patients. After preliminary measurements of this region in a large group of volunteers, we suggested that these anatomic differences on helical CT are not useful in differentiating patients with and without the thoracic outlet syndrome, and that the assessment of the clinical picture remains essential in the diagnosis of these patients. On this latter point, there seems to be consensus among many groups that objective testing and imaging are useful in portraying the anatomy. However, the clinical examination remains dominant because neurovascular compression is common in asymptomatic individuals whether it is measured by physical examination or by expensive diagnostic imaging.

We appreciate the opportunity to comment on this important study.

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The lack of a difference in meaningful measurable parameters between healthy and symptomatic patients is important because it would suggest that these anatomic differences on helical CT scanning are not useful in differentiating patients with and without the thoracic outlet syndrome, and that the assessment of the entire clinical picture remains essential in the diagnosis of these patients. On this latter point, there seems to be consensus among many groups that objective testing and imaging are useful in portraying the anatomy. However, the clinical examination remains dominant because neurovascular compression is common in asymptomatic individuals whether it is measured by physical examination or by expensive diagnostic imaging.

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Reply

My colleagues and I read with interest the letter by Matsumura et al. in response to our article [1] on the anatomic characteristics of the thoracic outlet in symptomatic patients. We thank them for providing us with references to several articles, mainly published in the surgical literature and dealing with evaluation of the thoracic outlet in both symptomatic patients and asymptomatic volunteers [2–6].

The apparent misunderstanding concerning the first in vivo evaluation of this region seems to be related to the meaning of “functional anatomy.” Our study, using helical CT angiography, was aimed at studying positionally induced modifications of objective measurements of this region in a large group of symptomatic patients. After preliminary experience with asymptomatic subjects, we measured several distances, angles, and vascular diameters before and after postural maneuvers and analyzed postural displacement of the vascular structures in 79 patients with clinical symptoms suggestive of thoracic outlet syndrome. Because none of the articles referenced by Matsumura et al. followed a similar study design, our findings could not be compared with the experience of other groups involved in the difficult clinical treatment of thoracic outlet syndromes. Among these references, we read with particular interest the only one that included CT angiography in the evaluation of thoracic outlet syndrome, namely the book chapter written by Matsumura et al. [2], in which we found major similarities in our respective approaches to this syndrome.
Unfortunately, that chapter did not include detailed descriptions of findings in the 18 consecutive patients referred to their institution for evaluation of possible thoracic outlet syndrome. However, Matsumura et al. are perfectly correct to underline additional diagnostic approaches to this syndrome, including duplex sonography and venography with pressure measurements [5, 6].

As to the postural changes observed at the level of the costoclavicular space, we must analyze the available data according to the methodology followed in each study. In normal subjects, only two studies can be compared, both using helical CT and published by our respective groups in 1997 [7, 8]. Our study failed to observe any significant difference in the distribution of the measurements on sagittal reformations before and after postural maneuver; Matsumura et al. observed that the costoclavicular distance was reduced by 50% on similar reformats. Measurements of different distances could account for different conclusions in these two studies: One group [7] measured the maximum and minimum distances between the inferior border of the clavicle and the superior margin of the first rib, whereas the other [8] defined the clavicle-to-first-rib distance as the shortest distance between these bones at the lateral convexity of the first rib. Because the subclavian vascular bundle is always located posterior to the narrowest portion of the costoclavicular space in the neutral position and after postural maneuver, we do not think that the minimum distance between the clavicle and first rib could be correlated with vascular compromise, as suggested by Matsumura et al. in their letter.

Despite discrepancies between our respective results, we would like to emphasize the consensus between our groups concerning the usefulness of CT angiography in the treatment of thoracic outlet syndrome. As previously underlined by Matsumura et al. [2], this technique represents the unique noninvasive means of depicting vascular stenosis, as well as anomalies of the surrounding musculoskeletal structures. In addition, radiologists can provide surgeons with an objective means of evaluating postoperative results [9].

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Effect of Knowledge of Actual Age on Bone Age Determination

Unless I misunderstand, there are errors in arithmetic in Table 4 of the article by Berst et al. [1] on the effect of knowledge of the actual age of children whose bone age is being determined. I think the line for “all observers” should read 268/428 (63%) and 207/428 (48%). If this is so, when the true age was known, the observers unconsciously shifted even more children into the normal range than the article states. The normal readings rose from 48% of all determinations to 63%. The 15% increase in reported normality was presumably an artifact caused by foreknowledge of the patient’s chronologic age. Had this not been a research situation, the children so shifted would not have received optimal radiologic care.

This observation makes even more emphatic the authors’ very valuable message, “Observers are more likely to interpret the radiograph as showing normal findings when the chronologic age is known” [1]. To me, the deduction is obvious: Objectivity is reduced when the radiologist learns the actual age before he or she has estimated the bone age; accuracy is enhanced when the bone age is determined first. The authors have provided an excellent example of radiologists allowing themselves to be biased by foreknowledge of patient history.

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My colleagues and I certainly appreciate Dr. Griscom’s efforts, thoroughness, and comments on our article [1]. After reviewing our data and calculations, we agree that a mathematical error is present in Table 4. The sum of the two columns represented by “all observers” should read 268/428 (63%) and 207/428 (48%) instead of 248/428 (58%) and 205/428 (48%), respectively. We apologize for this error and agree that this 15% increase in reported normality only strengthens our conclusion that observers are more likely to interpret a radiograph as normal when the chronologic age is known than when it is not known.

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Taking Care of Children

I read with dismay the article by Brenner et al. [1] in the February issue. The claim that using CT in the pediatric population results in an increased risk of cancer is unfounded. Their claim is based on the use of “relative risk models” that have never been proven. Moreover, their calculations are based on a setting of 404 mAs for abdominal CT, much more than is now used for adult CT scanning. This figure was taken from a 1989 survey of CT practice in Britain and does not reflect settings that are used in the United States today. This spurious claim of increased cancer risk has been trumpeted by the media and has resulted in considerable unwarranted anxiety among the parents of our patients.

Certainly, as emphasized in the articles by Peterson et al. [2] and Donnelly et al. [3] in the same issue, we should all use the minimum exposure necessary to obtain a diagnostic examination. This is a good reason for children’s imaging to be done by pediatric radiologists.

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As our article [1] points out, it is important to emphasize that, from a radiation perspective, pediatric CT is very different from adult CT or, indeed, any other radiologic examination. The doses to the organs are much greater than for adults [2], children are much more sensitive to radiation-induced cancer than adults [3], and pediatric CT usage is increasing rapidly, mostly in children who have many years of life in front of them [4].

Perhaps the most important point is that the cancer risks associated with pediatric CT, small though they are (in the approximate range from 1 in 1000 to 1 in 10,000, depending on age and exposure setting), are not hypothetical in the sense of being based on “models” or extrapolations. In fact, they are based directly on measured excess radiation-related cancer rates in individuals (atomic bomb survivors), including children, who were exposed to the same range of organ doses as are children who undergo a CT examination.

The organ dose range of relevance for pediatric CT examinations, assuming a range of mAs settings from 60 to 200 mAs [5, 6], and factoring in the frequency of multiple CT examinations [7], is about 5–100 mSv. The numbers of cancers in atomic bomb survivors exposed to this same low-dose range of relevance to pediatric CT (5–100 mSv) are shown in Table 1. In this low-dose range, a statistically significant radiation-related excess of cancers is observed, both for cancer incidence [8] and for cancer mortality [3]; no extrapolation or use of models is needed. Even at the low-dose (5–20 mSv) end of this low-dose region, a significant increase in radiation-related risk exists (Fig. 1). One might also note that the results shown in this figure are for all ages at exposure, and therefore they are underestimates of the risks in children.

In summary, radiation-related cancer risks at the same doses that are appropriate to pediatric CT have been directly measured in a human population. The measured risks are small but are statistically significant. Consequently, it would be hard to defend to the public a position that the risks are speculative or unfounded. That position might have been defensible two decades ago, when the atomic bomb survivor data were less mature, but not today.

Of course, it is too early to directly measure the lifetime effects of pediatric CT examinations. The rapid increase in CT use has occurred only over the past decade, whereas the atomic bomb survivor data indicate that the latency period between low-dose exposure and the appearance of a radiation-induced cancer can be 40 years or more. However, a recent case-control study [9] was done of leukemia incidence (for which the time between exposure and disease is typically only a few years) after any pediatric radiological examination (of which CT constitutes 40–67% of the effective dose [7, 10]). A significantly enhanced leukemia risk was associated with two or more pediatric examinations [9].

Thus, strong direct evidence indicates that radiation risks from pediatric CT examinations, though small, are real. From an individual standpoint, as we have emphasized [1], the benefits of a CT examination far outweigh this risk. From a public health perspective, however, this small individual cancer risk must be multiplied by a large (and increasing) population of children undergoing CT examination. In our article [1], we assumed that 4% of all CT examinations were performed on children; a very recent survey in the United States [7] suggests that this number is now around 11%—corresponding to about 2.7 million pediatric CT examinations per year. Even a very small individual radiation risk, when multiplied by such a large (and increasing) number of children, is likely to produce a significant long-term public health concern.

It has been 15 years since the first article on pediatric CT dose reduction was published [11], and most pediatric CT examinations are still being performed with adult mAs settings [6]. We hope that the current discussions will contribute toward a significant reduction in the collective dose from pediatric CT examinations, both through the more widespread application of appropriately reduced mAs settings and through a somewhat more selective use of pediatric CT, particularly for flank pain, appendicitis, and...
and blunt trauma [4]. The overwhelming weight of evidence is that the long-term public health benefit would be significant.

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CT As a Cause of Cancer: What’s Old Is New Again

The February 2001 issue had three articles on the radiation risks of CT [1–3]. They are interesting, well documented, and valuable. But are they new? In an American College of Radiology publication in 1996, Radiation Risk: A Primer [4], section author Joel Gray, then of the Mayo Clinic, informed thousands of radiologists of the risk of cancer death for those who undergo CT (12.5/10,000 population for each pass of the CT scan through the abdomen; this rate compares with 12.0 cancer deaths from 1 year of smoking/10,000 population). After reading Gray’s section, I contributed a letter to the editor of Radiology [5] with the caveat that CT, with its relatively high effective radiation dose, should not replace a test with a much lower effective radiation dose if the alternative (excretory urography, in my example) works as well. This should, of course, be a general axiom. A second letter to the editor [6] and a response to it [7] followed mine.

Gray [4] did not confine his calculations to pediatric patients, but his results of more than 4 years ago are similar to the conclusions of Brenner et al. [1]. Gray also strongly advocated that the CT radiation dose be reduced in studies performed on children and small adults, noting that this reduction would have no major effect on diagnostic value. This conclusion was reached more than 4 years before Paterson et al. [2] and Donnelly et al. [3] came to the same, clearly correct, conclusion. None of the three articles in the February 2001 AJR references Gray, my letter [5], or either of the letters that followed mine [6, 7].

More important, none of the three articles [1–3] makes the crucial point that a way exists to reduce radiation exposure from CT far more than would be achieved by appropriately reducing exposure factors during a CT examination. It is the simple expedient of not performing CT unless it is indicated. Obvious? Yes. Yet, do we not perform countless unnecessary CT examinations? Are we coerced by clinicians or, worse yet, by economic exigencies? Unfortunately, the honest answer is probably yes. Do we proffer an alternative test that will give the answer with less risk? Is any additional test, CT or otherwise, really needed? So many CT examinations are performed out of the emergency department that it would almost make sense to replace its door with a CT gantry and the CT table with a conveyor belt. Are these examinations all indicated?

A new trend is developing for obtaining what I call “vanity CAT scans.” These are self-paid “screening” CT scans of most of the body obtained for people who usually have no symptoms. As expected, they produce a very low yield of findings for disease—while still potentially increasing the risk for some cancers. This trend has even made it in the media and is summarized in a May 25, 2000, article from USA Today that is posted on their Web site [8]. Many such examinations are obtained using multibeam scanners that currently give an even higher radiation dose than helical or nonhelical CT [9]. Might this be justified for use on people (not yet patients) more than 50 or 60 years old, in whom the expected yield might be somewhat higher and the radiation risk somewhat lower? I don’t know. Do you? Does anybody? Risk–reward ratio, high. Justification, none. A bad idea.

CT is an extremely valuable tool, and nobody should hesitate to undergo CT when it is indicated. The operative word is “indicated.” When CT is performed, the radiation must be kept to the minimum required dose and reduced appropriately for children and small adults. CT scans should not be obtained for vanity or curiosity, as part of a shotgun approach by an insecure physician, or certainly not for economic reasons. As Osler [10] said, “Physician, first do no harm.”

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References


Call a Spade a Spade

Every year at this time, frantic fourth-year residents are busy learning all the “buzz words” and radiologic manifestations of many diseases that most of them have never seen during their daily clinical work. Recently, residents at our institution viewed a film on the hand that showed the classic manifestations of acromegaly. The tuftal hypertrophy was identified, and one resi-
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dent happily described the finding as the classic “spade-shaped” phalanges.

The finding of tuftal hypertrophy is certainly associated with acromegaly and has been described extensively in the literature [1]. However, “spade-shaped” refers to the enlargement of the hand caused by both soft-tissue and osseous enlargement, not to enlargement of the phalanges. This term was coined by Marie [2] in 1886 before the discovery of X rays, and it refers to the garden spade (Fig. 2A), not the playing card symbol (Fig. 2B). Interestingly, my own musculoskeletal colleagues hold varying opinions on the true meaning of spade-shaped. These opinions highly correlate with whether they are gardeners. I find it amusing that this clinical description, coined before the discovery of X rays, was misinterpreted by some unknowing radiologist along the way and now has proliferated in the minds of radiologists and even in the pages of radiology textbooks. I hope this short letter sets the record straight.

Fig. 2.—Drawings illustrate typical appearance of gardening spade (A) and spade symbol used on playing cards (B).

References


Erratum

An error was made in the article, “Ovarian Carcinoma” [1] in the July 2001 issue of the AJR. The title should have been “Ovarian Carcinosarcoma.” We regret the error.

Reference

1. Mullins ME, Chao S, Ochoa ER, Slanetz PJ. Ova- rian carcinoma. AJR 2001;177:130

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