6. Tierney LM, McPhee SJ, Papadakis MA, eds. *2006 Current Medical Diagnosis and Treatment*. New York, NY: Lange Medical Books 2006:1083–1086.

Ear Lobe Crease

Point of Disagreement in Evidence-Based Medicine

To the Editor:

Thirteen years ago we did a pilot study on diagonal ear lobe crease (DELC) in 78 unselected hospitalized patients (average age 60.4 ± 11.4 years, 65.4% men) in Department of Cardiovascular Diseases. At that time, DELC was mainly considered as an accepted marker of coronary artery disease (CAD).[1,2] Our idea was not to check DELC qualitatively (is it good or bad indicator of CAD), but to analyze it quantitatively: (1) Is bilateral DELC a more reliable sign than unilateral? (2) Is deep DELC a better marker of CAD than superficial (<1 mm)? (3) Is a higher total DELC number a better CAD indicator than 1?

The result of our small study was surprising; answers to each of 3 questions were "no." There was nonsignificant correlation between DELC and clinical CAD (not even a trend toward significance), as well as with diabetes mellitus, hypercholesterolemia, both systolic and diastolic BP (actual and maximal BP from history), stable angina, unstable angina, myocardial infarction, coronary artery by-pass graft, and smoking. It was also true for xanthelasma and arcus juvenilis or senilis, far less studied parameters in context of DELC. The single parameter that was statistically significantly related to DELC was age, as in the great majority of articles. Thus, we concluded that DELC was totally unuseful in our examinees, as a potential extracardiac sign of CAD (unpublished data) and stopped the study.

Meanwhile, evidences have been accumulated both pro[3,4] et contra[5] benefit of DELC. There are not many topics in cardiology today with the exactly opposite conclusions like DELC as the CAD marker. We believe in evidence-based medicine and guidelines (in addition to common sense and own experience), but what to do when the results of well-done studies are diametrically different? More research should be done, obviously, with 3 requests at least: (1) A need is imposed to define DELC precisely (consensus). (2) Is DELC good marker of CAD? (3) If it is, DELC should be also proved to add information for CAD risk at least to that we already see in our examinees: age, gender, obesity, smoking habit, etc.

Now, having in mind all the studies that suggested DELC as the valid marker of CAD, despite our own disappointing results, we believe that there are chances to find a subgroup of examinees (according to age, gender, smoking status, etc) in whom it may be useful to check for DELC. In addition, preauricular crease seems promising, according to Miot et al.[6] It is unexpected, but reported positive predictive values for DELC are good[6,7] in comparison with exercise tests.[8]

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REFERENCES

1. Elliott WJ. Ear lobe crease and coronary artery disease. One thousand patients and review of the literature. *Am J Med*. 1983;75:1024–1032.
2. Toyosaki N, Tsuchiya M, Hashimoto T. Earlobe crease and coronary heart disease in Japanese. *Heart Vessels*. 1986;2:161–165.
3. Bahcelioglu M, Isik AF, Demirel B, et al. The diagonal ear-lobe crease. As sign of some diseases. *Saudi Med J*. 2005;26:947–951.
4. Edston E. The earlobe crease, coronary artery disease, and sudden cardiac death: an autopsy study of 520 individuals. *Am J Forensic Med Pathol*. 2006;27:129–133.
5. Kuon E, Pfahlbusch K, Lang E. The diagonal ear lobe crease for evaluating coronary risk. *Z Kardiol*. 1995;84:512–519.
6. Miot HA, de Medeiros LM, de Siqueira CRS, et al. Association between coronary artery disease and the diagonal earlobe and preauricular creases in men. *An Bras Dermatol*. 2006;81:29–33.
7. Evrengül H, Dursunoğlu D, Kaftan A, et al. Bilateral diagonal earlobe crease and coronary artery disease: a significant association. *Dermatology*. 2004;209:271–275.
8. Miller T, Roger V, Milavetz J, et al. Assessment of the exercise electrocardiogram in women versus men using tomographic myocardial perfusion imaging as the reference standard. *Am J Cardiol*. 2001;87:868–873.

Neck Injuries and Shaken Baby Syndrome

To the Editor:

Several doctors and engineers[1,2] have recently submitted letters to Forensic Science International calling into question the calculations used by Faris Bandak in his article "Shaken Baby Syndrome: A biomechanics analysis of injury mechanism."[3] I fear that these scientists may have missed the main message of Bandak's article.

To summarize the issue, without getting caught up in all of the physical and mathematical calculations, Bandak's original article argues that the forces generated by shaking an infant will exceed the cervical spine's ability to resist such force—in short, if you shake an infant you will injure the neck. Bandak uses a mechanical model to calculate the forces applied to the neck when a child is shaken (neck distraction forces) and compares them to the amount of force the neck is able to withstand before injury is sustained, as generated from previous experiments. Bandak argues that if we consider the practice of obstetrics and automobile crashes, then we know that the neck will fail if significant force is applied.

In Margulies et al,[1] the argument is made that the forces that affect the neck during shaking calculated by Bandak are not as high as he purported and are on the magnitude of 65 N to 2272 N (Bandak's range 1027 N–35,910 N). Rangarajan and Shams[2] make a similar argument and give neck distraction forces during shaking to range from 65 N to 2271 N. However, I fear the authors have missed the forest for the trees, per se. If we assume these authors to be correct and Bandak's original calculations to be incorrect, what would be the outcome? Bandak's article sites previous studies that have calculated the forces on the neck when injury is sustained. These forces included 209 N (baboon), 249 N (goat), and 445 (human neonate).[3] A recent article by Ouyang et al calculated the pediatric cervical spine could withstand tensile forces ranging from 493 N and 725 N.[4] Even using the higher numbers found by Ouyang (which are again tensile strength and not rotational) and the new calculations shown to us by Margulies et al and Rangarajan and Shams, the forces generating by shaking an infant are within and even exceed the range necessary to injure a neonatal neck.

If these infants are truly being shaken to death, where are the neck injuries? Given the calculations above, should we not be seeing at least some injured necks in "Shaken Baby Syndrome"? Margulies et al acknowledge that shaking may cause neck injury.[1] It would seem that both skeptic and proponent alike agree that the forces generated by shaking an infant are in the range necessary to cause neck injury, yet neck injury is not part of the "Shaken Baby" syndrome (which includes the classic triad of encephalopathy, subdural hemorrhage, and retinal hemorrhage). As forensic pathologists are keenly aware, neck injuries in a "shaken" child are a rarity, not a commonality, which, it seems, is the reverse of how it should be, given the previous discussion. Farley recently reviewed 79 cases of infant homicides in which there was evidence of head trauma (subdural hemorrhage/encephalopathy/retinal hemorrhages) in Bexar County Texas and found no cases with injury to the cervical spinal column or spinal cord,[5] thus, yielding an incidence of 0% for neck injuries in potential "shaking" cases.

So I pose a simple question—if a child has the classic triad of Shaken Baby Syndrome, and considering that the forces of shaking are within range of causing neck injuries, yet we virtually never see these injuries, could it simply be that shaking is not the mechanism behind these injuries at all? This is the point Bandak clearly makes in his article that is glossed over by the subsequent reviews.

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REFERENCES

1. Margulies S, Prange M, Myers BS, et al. Shaken Baby Syndrome: a flawed biomechanical analysis *Forensic Sci Int.* 2006;164:278–279.
2. Rangarajan N, Shams T. Letter to the Editor *Forensic Sci Int* 2006;164:280–281.
3. Bandak FA. Shaken baby syndrome: a biomechanics analysis of injury mechanisms. *Forensic Sci Int*. 2005;151:71–79.
4. Ouyang J, Zhu Q, Zhao W, et al. Biomechanil assessment of the pediatric cervical spine under bending and tensile loading. *Spine*. 2005;30: E716–E723.
5. Farley NJ. Personal Communication 12/12/06. Data presented at: The 40th Annual Meeting of the National Association of Medical Examiners. "Where's the Shaking"? October 13, 2006. San Antonio, Tex.