Letters to the Editor

Concerning the Solubility of Sugar in Gasoline

Dear Sir:

It is common folklore that sugar added to an automobile gasoline tank will cause major damage to the engine. However apocryphal this notion is, it is widely believed and has prompted at least some grudge holders to pour table sugar into the gas tanks of automobiles belonging to their enemies.

The premise that sugar added to a gasoline tank will foul an engine seems to imply that sucrose is soluble in gasoline and that the sugar will therefore be carried to the engine by the gasoline. This premise, which the present work tends to dispel, seems to be embraced by vandals and police investigators alike. Police investigators, in pursuit of "sugared" gasoline, are more likely to submit to the laboratory samples of suspected gasoline siphoned from the tank or delivered by the fuel pump to a disconnected fuel line. Rarely will an investigator cause the entire contents of a fuel tank to be submitted to the laboratory.

Sugar can be detected in fluid gasoline only to the extent that it is soluble. Chemical principles of solubility would predict marginal if any solubility of sugar in gasoline. We demonstrate here that this is the case. From the standpoint of criminal responsibility it would make no difference whether the sugar is soluble or not, but from the standpoint of *sampling and testing*, it makes a great deal of difference.

Two experiments were conducted. In the first experiment, a carefully weighed amount of oven dried sucrose was added to gasoline at room temperature and stirred for 30 min. The mixture was filtered through tared filter paper; the filter paper containing undissolved sucrose was oven dried, allowed to cool, and weighed. The recovery of sucrose in replicate runs was, respectively, 97.97% and 99.58%. While this experiment suggests that sucrose is virtually insoluble in gasoline, the limits of sensitivity of the method do not put the issue at rest.

A second experiment was conducted in which a known amount of ^{14}C labeled sucrose was added to a known amount of gasoline. The mixture was equilibrated by stirring and an aliquot taken for scintillation counting. The detected concentration of ^{14}C labeled sucrose in gasoline ranged in replicate experiments between 1.26 mg/L and 1.44 mg/L. If the upper limit is rounded off to 1.5 mg/L, then the total amount of sucrose that would go into solution in a 15 gallon tank of gas would be on the order of 90 mg.

The implications of the solubility of sucrose in gasoline to sampling and analytical considerations are patent. If sugar is added to gasoline, virtually all of it will be found, undissolved, on the bottom of the tank. Even if the gasoline is saturated with sucrose, the concentration of sucrose is too low to be detected by simple means. A 100 mL sample of gasoline, for example, would contain only 150 μg of sucrose. Accordingly, the investigation of cases of motor fouling caused by the suspected addition of sugar to the gasoline must include a sampling of any solid residues in the fuel tank.

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Modifying Stature Estimation from the Femur and Tibia

Dear Sir:

For 40 years Trotter and Gleser's [*1*] set of linear regression equations, based upon a systematic study of the Terry skeletal collection (for females) and World War II remains (for males), has provided American forensic anthropologists a standard method for estimating the living stature of blacks and whites from the maximum length of any one of the six long bones. In the September 1992 issue of the *Journal*, Professor Richard L. Jantz [*2*] recommends changing the Trotter and Gleser formulations used for stature estimation from the femur and tibia in white females. The undeniable weight of Dr. Jantz's experience and expertise notwithstanding, I have very serious reservations about his proposal.

The basis for the suggested modifications is the claim that a new sample of 84 femora and either 79 (Table 1) or 82 (Table 2) tibiae from white females demonstrates differential secular change sufficient to invalidate use of the original presentation. Black female samples show equivalent (tibia) or greater (femur) mean differences from the relevant Trotter and Gleser samples. These differences, also identified by Jantz as secular change, are more proportional, and he recommends retaining the original Trotter and Gleser formulas. All new samples come from the portion of the University of Tennessee's Forensic Anthropology Data Bank said to have been positively identified. The Data Bank is a collection of measurement reports by various persons from around the U.S. on skeletal material derived from casework (as of 1988 there had been 23 laboratory or institutional contributors to the Data Bank, but more than half of the cases came from Dr. J. L. Angel's records [*3*]).

Jantz is not inclined to revise Trotter and Gleser's slope coefficients (the values by which the long bone lengths are multiplied) which would require using the reported stature data [*3*]. The question is whether Jantz's data are sufficiently compelling to warrant changing the intercept values of Trotter and Gleser, which requires him to have both new tibia and femur length means and a new population average living stature. The latter, though the 163 cm figure used bears no relation to his database, is not as much of an issue as the bone lengths. These are based on an exotic population sampling strategy (somewhat akin to ballistics characterization only from bullets that actually killed someone), but more importantly, entail unassessed measuring comparability, unspecified use of unrelated tibiae and femora, and, as described below, undefined criteria for acceptance of new intercept values and uncertain measures of accuracy improvement.

For females of both races the Data Bank has reported stature in 48 cases, cadaver measurements in 6. Trotter and Gleser's females' statures were all measured on cadavers in a standardized, described fashion. Jantz accepts that reported statures (drivers licenses, missing persons reports, relatives, etc.) are "notoriously inaccurate," but uses them anyhow. He implies less variation in reported stature error than exists among 4 of the 5 anthropometric studies cited in Table 3 (the fifth does not contain female reported stature). The women in three of these investigations were asked how tall they were, the fourth used the women's drivers licenses. Their average overreporting was, in ascending order: Fels mothers: 0.3 cm [*4*] (Jantz's erroneous 1 cm amount is taken from the misleading abstract of this reference; its text and Table 1 provide the correct figure); Willey and Falsetti's drivers license study: 0.57 cm [*5*]; the U.S. Army: 1.02 cm [*6*]; and the U.S. Air Force: 2.5 cm [*7*].

Data in Jantz's paper allow the calculation of the bone length means of the two subsamples, "with-height" and "without-height," which comprise total femora and total tibiae for white females. There is over a 1 cm difference between the means of the two subsamples for both bones. The subsample difference in the case of the femora is *greater* than the difference between the total sample's mean and that of Trotter and Gleser's sample. In tibiae the subsample difference is about three-fifths of the difference between the total sample's mean and that of Trotter and Gleser's sample.

Jantz undoubtedly has actual standard deviations for the white female with-height and without-height subsamples of the femora and tibiae, but if they are adequately represented by the total samples' standard deviations given in Table 2 for calculating the standard error of the difference between two means, then the differences between the means of the with-height and without-height subsamples for both bones are significantly different at the $P < 0.05$ level.

Furthermore, because of the subsample difference, intercepts based only on the without-height femora and tibiae means actually predict more closely (mean LIVSTAT–YHAT) the average reported stature for the with-height specimens than do Jantz's recommended intercepts based on the total samples (0.58 vs. 1.77 cm for femora, 0.42 vs. 1.66 cm for tibiae).

These results raise two very serious objections to Jantz's proposed modifications. Why is there an unexplained, apparently significant 1 cm difference between the with-height and without-height subsamples of his total white female samples of femora and tibiae lengths, and what does this say about the reliability of his whole sampling procedure for detecting secular change? Secondly, even if the 1 cm difference is possibly not *statistically* significant, Jantz should explain why he uses the statistically questionable procedure of incorporating his validating sample (that is, with-height subsample) into the sample (that is, total sample) used to determine his new regression intercepts. Because intercepts based on the without-height subsample provide more accurate stature predictions as measured by with-height subsample validation than do intercepts based on the total sample, Jantz's approach is even more difficult to understand.

Jantz also advances no specific criterion for acceptance or rejection of results deriving from Trotter and Gleser's formulas. Even if we accept the dubious proposition that reported stature is an adequate surrogate for measured living or cadaveral stature, the only apparently significant performance difference (mean LIVSTAT–YHAT) between black and white females is that Trotter and Gleser's average estimation for white females from the femur is about 9 mm less accurate than their average for black females. The difference is less than 2 mm for the tibiae. (There is a discrepancy between Table 6 figures and those in the text pages 1233–1234; I use the figures in the table.) In any event, Jantz's revised intercepts provide average predicted statures from femora and tibiae understating the white female subsample's average reported stature by about 1.7 cm (Table 5), not the 0.7 cm discussed on page 1234.

If secular change in stature has favored forensic anthropologists by being simple, or can be simulated as such, the apparent stature increase in women since Trotter and Gleser's work may only mean that today's average case will involve bones yielding an estimated stature as accurately as bones from a tallish woman of 40 years ago. Naturally, the confidence limits of her predicted stature may be broader as a result of the increased error of estimate in that (taller) portion of the stature distribution of Trotter and Gleser's samples [*8*].

Secular change may not be so simple, however; there are sample findings that suggest its quantification may be difficult, its populational differences puzzling, and its progress nonlinear. For example, Greiner and Gordon [*9*] showed recent secular change rates in male stature 2.5 times as great in whites as in blacks. Cline et al. [*10*] uncovered a general secular increase in white female stature except for stasis in the 10-year birth cohort centered around 1935, while Hertzog et al. [*11*] actually found a reverse secular trend for tibia length in some age groups of white females.

The Forensic Anthropology Data Bank assuredly allows Jantz to develop hypotheses and concerns about the particular nature of the secular trend in stature as it may impact the ability of forensic anthropologists to estimate stature dependably from long bones. The results of appropriate tests of these hypotheses may well ultimately require modifications of Trotter and Gleser's work. Until they do, I think professional forensic anthropologists would be better advised to continue using the set of consistent and defensible regression formulations Trotter and Gleser have provided us rather than begin having to pick and choose, by race, sex and bone, from among the old Trotter and Gleser and new, potentially fluctuating databases and then quite likely having to defend the statistical merit of their choice in court.

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Author's Response

Dear Sir:

Professor Eugene Giles advises forensic anthropologists to continue using Trotter and Gleser's [1] regression equations for females rather than adopt my modifications [2]. His advice is based on problems he perceives in my approach as well as the belief that the Trotter and Gleser regressions remain "consistent and defensible." I should say at the outset that I do not regard my proposed modifications as the last word on the issue, any more than did Trotter and Gleser, who pointed out that secular trend demands the derivation of new formulae at opportune intervals [3]. My proposed modifications rest on the availability of fresh data and the strong indication that Trotter and Gleser's formulae are inappropriate for modern white females.

One of Professor Giles's concerns seems to be with my sampling strategy, which he terms "exotic." If by the strained analogy, (likening it to "ballistics characterization only from bullets that actually killed someone") he means I sampled forensic cases rather than the general population, then I assert that to be a strength rather than a deficiency. Sampling strategies typically attempt to target populations of interest for particular purposes (why else do pollsters target likely voters?). Since here we are interested in estimating stature in a forensic context, the Forensic Data Bank provides an appropriate sample, certainly more appropriate than the Terry Collection, consisting of St. Louis indigents unable to afford other more expensive ways to dispose of their bodies.

Another of Giles' concerns with the Forensic Data Base as a sampling device comes from the necessity of combining bones measured by different observers, raising the question of comparability among observers. Giles is correct in terming this "unassessed"; at this point we really have no way of dealing with that issue. However, Giles apparently considers this to be a more serious problem than I do. We have had standardized definitions since the late 19th century for the very reason that measurements made by different observers could be combined. We use those of Martin [*4*], English translations of which have been incorporated into our data collection manual [*5*]. Trotter and Gleser also carefully describe how the long bones should be measured. And of course the very application of the regression formulae to skeletons whose stature in life is to be estimated depends on the ability of forensic anthropologists to measure the bones in the same way as Trotter and Gleser. If the contributors to the Data Bank are incapable of reporting comparable bone lengths, then they are also incapable of properly applying the formulae.

Giles also calls attention to "unspecified use of unrelated tibiae and femora." I did not properly specify the tibia and femora samples, but they are not unrelated. I used all that were available that met the criteria specified on p. 1231 of my paper. There were 84 femora and 79 tibia; in 77 individuals both bones were present.

A more critical issue concerning sampling is Giles' identification of differences between the with-height and without-height subsamples. He is correct in observing that these two subsamples differ in bone length by approximately 1 cm. My figures (in mm.) are as follows:

	Femur			Tibia		
	N	Mean	S.D.	N	Mean	S.D.
With-height	38	444.184	20.175	34	363.794	22.603
Without-height	46	433.478	20.520	45	353.289	18.366

The differences are significant by t-test for both bones below the 0.05 level of probability (t = 2.39 and 2.28 for femur and tibia respectively). The reason for the subsample difference itself relates to secular trend. Birthdates in my sample range from the turn of the century (five were born in the 1890s) through 1970. Those for whom height information is available are a more recent subset than those for whom the information is not available. The average birth year for those with-height is 1945; for those without, it is 1927. One would have to conclude that secular changes are occurring over the 70+ years represented by the birth years in the sample. That issue has recently been addressed in males [*6*], but not yet in females. That aspect of the sample should have been dealt with in my paper; failing to do so misled Giles and possibly other readers concerning the nature of the two subsamples. The points raised by Giles cannot be fully addressed without a more detailed analysis of secular trend within the Forensic Data Bank sample, but I offer the following remarks by way of clarification.

Professor Giles observes that differences between subsamples is greater than the difference between the total sample and the Terry collection mean. Trotter and Gleser [*7*] were unable to identify secular change in the Terry collection skeletons, in which the year of birth ranges from 1840 to 1909. However, as demonstrated by our analysis of males, secular change begins in the early 20th century and continues to those having birth years up through 1970. Giles' observation just means that secular trend differentiates the early part of my sample from the later part. The issue of absolute size partly misses the point of why the Terry collection provides inadequate parameters for estimating height of modern people. Tibia-femur proportions have also changed. We can observe that the tibia-femur ratio in the Terry female sample is 0.792, SD = 0.0213, while in the forensic sample it is 0.817, SD = 0.0206. The standard deviation for the forensic sample was computed from the 77 individuals possessing both bones, while for the Terry data it was estimated from an approximation presented by Korey [*8*]. The Terry-Forensic difference in the ratio is highly significant while the two forensic subsamples are homogeneous, yielding tibia-femur ratios of 0.819 and 0.815 for the with-height and without-height samples, respectively.

In pointing out that intercepts computed from the without-height sample predict LIVSTAT more closely than my intercepts derived from the total sample, Giles assumes curiously inconsistent positions. First he criticizes me for acknowledging the notorious inaccuracy of reported statures, but using them anyway, but then uses them himself to raise questions about my samples. I was at pains to point out that these are likely overestimates and Giles makes the same point. Correcting my erroneous 1 cm over-reporting for the Fels mothers [*9*], Giles gives ranges of over-reporting from 0.3 to 2.5 cm. That the without-height intercepts yield a closer approximation to reported height than intercepts based on the total sample only means that the former yield higher stature estimates.

My use of the reported statures was to assess tibia-femur consistency, rather than using them as an absolute scale. Hence it is not necessary to "accept the dubious proposition that reported stature is an adequate surrogate for measured living or cadaveral stature . . . ," which yields the 9 mm difference in performance between blacks and whites that Giles refers to. My criterion for rejecting Trotter and Gleser's formulae stems from their inability to produce similar estimates for the femur and tibia. As is stated on p. 1232 of my paper, Trotter and Gleser's femur formula yields a height estimate over 3 cm greater than the tibia. Both bones should yield similar estimates on the same people if the regressions are performing satisfactorily. The 3 cm difference is unacceptable; my adjustments in intercepts remedy the disparity.

Giles characterizes Trotter and Gleser's formulae as consistent and defensible, but as far as I am aware they have not been tested in a systematic manner on modern people until now. Giles, and perhaps others may be more comfortable with Trotter and Gleser's Terry derived formulae, even though the Terry collection birth years are predominately mid-19th century, a time when American statures were lower than at any time since 1710 [*10*]. The difference between Terry and modern forensic cases in tibia-femur ratio, as well as several additional rather marked differences, both cranial and postcranial, [*11*] make it inadvisable to continue to use the Terry collection as a reference series, at least for metric data.

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A Simple Technique for Age Estimation in Adult Corpses: The Two Criteria Dental Method

Dear Sir:

In their article, Lamendin et al. [1] are to be commended for attempting to improve methods for age estimation in unknown decedents. However, they cite two criteria but they have included one which is clinically unreliable.

They use root transparency seen in the axial sectioning of human teeth and "periodontosis," which they define as "gingival recession." It is my objection to "gingival recession" as a valid criterion, to which I address my remarks as follows.

1. They measure from the cementoenamel junction to the soft tissue attachment. This measurement is inaccurate and impossible in a badly decomposed or skeletonized body.
2. Gingival recession when it occurs, can vary markedly from one tooth to the next, and it may or may not be present in all of the teeth in the same individual.
3. It is a clinical fact, and one that those of us in clinical practice see on a daily basis, that age does not necessarily influence gingival recession that frequently is the result of improper brushing techniques or neglect. Periodontal disease *is* influenced by the health of the patient, bacteria present in the oral cavity and a wide range of other factors. To imply that age is a factor, to the exclusion of other factors, equates to a misunderstanding of the clinical periodontal disease syndrome.

There is certainly merit in employing root transparency in estimating age as well as other histological observations, but the factors of periodontal attachment and occlusal abrasion have long ago been discarded by those of us, who recognize that these two entities are closely influenced by the habits, diet and health of individuals and not by their age.

I am surprised that the reviewer(s) did not critique or comment on the inclusion of periodontal attachment as a factor in the authors' suggested technique. The authors would do better by concentrating on root transparency and other *internal* observations of the human dentition in a future study.

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Reference

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Author's Response

Dear Sir:

In his letter Norman (Skip) Sperber mainly disputed the validity of periodontosis as an age indicator; we agree with him! But this was not the subject of our paper. We [*1*] and other authors [*2*] previously compared the value of various dental features for age determination; periodontosis is clearly and definitely not the best when used alone. However, by using multiple regression analysis, we found that the combined measurements of root transparency and periodontosis provided better age estimates than any other combination, including root transparency plus secondary dentin, even if the latter happens to be a better age indicator than periodontosis when these variables are evaluated individually. This is due to the fact that periodontosis is statistically independent from the major variable (i.e., root transparency) which is not the case for secondary dentin. This is not magic but statistics and it illustrates what these methods have been designed for: to palliate the insufficiencies of the so-called personal clinical experience and "feelings" in the field of medical research.

Actually, most of these arguments were already presented in our paper. There are a few other clues to suggest that Dr. Sperber maybe did not read it attentively enough. We did not define periodontosis as "gingival recession" but "regression." More confusing are his comments about "concentrating" on "other internal observations of the human dentition" as well as his mention of "root transparency seen in axial sectioning of teeth." It seems that we did not insist enough on the fact that we did not use sections but rather measurements made on the entire and preserved tooth. As mentioned in the title our goal was to propose a simple technique (even if based on complicated statistics) for age determination.

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