Letters to the Editor

Discussion of "The Application of Linear Regression to Range-of-Fire Estimates Based on the Spread of Shotgun Pellet Patterns"

Dear Sir:

The article of Heaney and Rowe entitled "The Application of Linear Regression to Range-of-Fire Estimates Based on the Spread of Shotgun Pellet Patterns" (Vol. 28, No. 2, April 1983, pp. 433-436) in the *Journal* presents an interesting and long awaited approach to this problem. However, there appears a conceptual flaw in their application of statistical theory which affects the validity of the conclusions presented. A test for correlation is *not* a test of linearity.

In the Experimental Procedure section of the paper, it was stated that "one shot was fired at each range for a total of nine rounds." Such limited sampling infuses serious problems into the fitting of data to a linear model; not only is it not established that the model is linear, it is not established that the data are even reproducible. Multiple observations are needed at each fixed range x value to test the latter three of the five classical assumptions which must be made *a priori* to the application of linear regression. These assumptions are: (1) fixed range at which test patterns were fired to determine the regression line x_i values; (2) the y_{ij}'s are independent random variables; (3) at each x_i, the distribution of the y_{ij} values is normal; (4) the variance of the variable y_{ij} is the same at all values of x_i; and (5) the association is linear.

The assumption of linearity in (5) above, is not an application of circular thinking. Instead it is a recognition that before linear regression can be applied to a set of data, the data should be tested by partitioning the sums of squares into an ANOVA table and using an F test for linearity. Once again, this demands multiple observations.

The assumption of linearity cannot be tested by looking at the correlation coefficients as implied by Heaney and Rowe, since the value of radius R can be heavily influenced by range restriction and nonrandom sampling [*1*]. Had the authors made multiple observations at each x_i (which could be accomplished simply and quickly using the method proposed by Jauhari [*2*]), they would have discovered that the data might not fulfill another assumption, that of equal variances for the dependent variable y_{ij} at each x_i. Based on Jauhauri's data, the variances appear to increase with increasing distance, that is, range-of-fire. This can be explained by the atypical flight paths of deformed shot. These flight paths tend to lie in parabolic curves that become more curved as the distance increased [*3*], rather than in paths that can be described (in the aggregate) as following the surface of a cone. With unequal variances, a simple linear model cannot be proposed for a set of data.

For the reasons outlined above, a simple linear model may not be realistic in an application to the problem of range-of-fire estimates with shotgun pellet dispersion. However, if the authors restructured their experimental protocol to include multiple observations at each fixed distance, then an approximate statistical, approach could be entertained which may or may not bear out their conclusions.

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Author's Response

Sir:

The criticisms of Rios and Thornton reflect views that we have come to accept because of the greater experience that we now have with the behavior of shotgun pellet patterns. In fact, we would add an additional cautionary note: regardless of the method employed to obtain range-of-fire estimates, the method should be thoroughly validated by blind testing.

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Discussion of "George Armstrong Custer and the Battle of the Little Bighorn: Homicide or Mass Suicide"

Sir:

I read with great interest Jerry D. Spencer's recent article "George Armstrong Custer and the Battle of the Little Bighorn: Homicide or Mass Suicide?," which appeared in the July 1983 issue of the *Journal of Forensic Sciences*. This article inspired me to devote some time to the study of Thomas B. Marquis' book, *Keep the Last Bullet for Yourself: The True Story of Custer's Last Stand* [1] and to a critical evaluation of Marquis' arguments in favor of his hypothesis that a large number of the troopers under the command of Lt. Col. Custer committed suicide. This hypothesis rests on several items of evidence, including the following:

1. Some Indian survivors of the battle told Marquis that they witnessed soldiers shooting each other and themselves.
2. The Indians suffered very few casualties. (Marquis estimated 31 Indian dead, 14 inflicted by Custer's troops and 17 inflicted by the combined troops of Maj. Reno and Capt. Benteen. This estimate was Marquis' average of the various estimates of his Indian informants.)
3. In numerous other engagements with various Indian tribes whites had been able to stand off their attackers even when heavily outnumbered and encircled. The sole exception was the Fetterman Massacre (1866), in which even U.S. authorities believed that some of the whites had committed suicide or shot each other to avoid capture and torture.
4. The 7th Cavalry enjoyed a significant firepower advantage over the Indians in that the 1873 Springfield carbine had a longer range than the heterogeneous collection of muskets, rifled muskets, revolvers, and single-shot breechloaders with which the Indians were armed.
5. The 7th Cavalry contained at the time of the battle a large percentage of green recruits.

Keep the Last Bullet for Yourself contains many perspicacious observations leavened with large doses of military naivete. For example, Marquis was probably the first to grasp the significance of the manpower problems that plagued the U.S. Army during the Indian wars.

At any given time, the white units of the U.S. Army contained a large body of recruits and/or inexperienced soldiers. This resulted from the fact that the desertion rate for white enlisted men was about 25% per year [2]. On the other hand, Marquis' discussion of the superiority of the 7th Cavalry's weapons over those of the Indians is marred by his failure to understand that military combat differs from range firing. The 1873 Springfield was indeed a powerful weapon capable of accurate long-range fire; however, when fired by a prone or kneeling trooper peering into the swirling dust and gunsmoke and trying to fire at enemies slithering through the scrub and sage brush on the slopes of Custer Ridge, the 1873 Springfield would have had an effective range little better than the Indian's firearms.

Marquis' comparisons of the Battle of the Little Bighorn with other Indian fights are rendered largely irrelevant by his failure to give adequate weight to the fact that in all the battles cited in which whites successfully held off large numbers of Indians, the whites availed themselves of rifle pits or natural cover to diminish the effectiveness of the Indian's weapons.

A startling omission (considering that Marquis was a physician) is his failure to include wounded in his estimate of the casualties suffered by the Indians. His estimate of 31 Indian dead suggests very limited combat effectiveness on the part of the 7th Cavalry. However, how many Indians were also wounded? Generally in the 18th and 19th century land warfare three or four combatants were wounded for each one killed [*3, 4*]. Using these ratios for the Indians engaged in the Battle of the Little Bighorn we may calculate Table 1. If we assume that all 41 of the dead Indians whose bodies were found in the abandoned village [*5*] were killed in the battle the low estimate of Indian casualties would be increased to 164, while the high estimate would be increased to 205. In any case, the Indians were hurt more than Marquis was prepared to admit.

The Indians killed all 215 men under Custer's command [*6*]. If the Indians suffered 56 to 70 casualties in accomplishing this result, it would seem that the Indians were three to four times as effective as the troopers of the 7th Cavalry. That this is not necessarily the case is shown by applying the *Lanchester equations* to this engagement. F. W. Lanchester [*7*] proposed the following differential equations to describe combat losses in military and naval engagements:

$$\frac{dR}{dt} = -NB \qquad \frac{dB}{dt} = -MR \tag{1}$$

where R and B are the strengths of the red and blue forces, respectively, and N and M are constants reflecting the effectiveness of the blue and red forces, respectively. These equations have been found to represent effectively simple battles where duel tactics are used, that is, each force is in sight of the other [*8*].

We may assume various force levels for the Indians and various relative effectivenesses for the Indian versus the cavalry and then calculate the casualties resulting if one side fights to extinction. The results of such a calculation appear in Table 2. Readers unfamiliar with the Lanchester equations may be puzzled by these results. That an increase in the Indian forces

TABLE 1—*Estimate of total Indian casualties at the Battle of the Little Bighorn.*

U.S. Force Engaged	Killed in Action	Wounded in Action	Total
Custer	14	42-56	56-70
Reno/Benteen	17	51-68	68-85
	31	93-124	124-155

TABLE 2—*Casualties resulting with various force levels and relative effectiveness.*[a]

Indian Forces	Relative Effectiveness (Indians/Cavalry) 2	1	0.5
250	Ind: 51 US: A	Ind: 122 US: A	Ind: A US: 92
500	Ind: 23 US: A	Ind: 48 US: A	Ind: 103 US: A
750	Ind: 15 A	Ind: 31 US: A	Ind: 64 US: A
1000	Ind: 11 US: A	Ind: 23 US: A	Ind: 43 US: A

[a]A = annihilated.

leads to a decrease in the Indian casualties appears paradoxical. However, an increase in the Indian forces engaged leads to a more rapid extinction of the opposing force and a shorter time interval in which that force can inflict casualties on the Indians.

The point here is that contrary to the notions of Marquis and other commentators, Custer's troops could have been annihilated even by a force of Indians, each of whom was substantially less effective in combat than Custer's cavalrymen. All that was required was enough Indians. Another point may be made about the results in Table 2: if a significant number of troopers committed suicide (thus substantially increasing the average effectiveness of the Indians) the 7th Cavalry would have been hard pressed to inflict 56 to 70 casualties on an Indian force numbering several hundred, if not several thousand. Alternatively, we would have to imagine that those cavalrymen who fought to the last were prodigious fighting men.

When a thorough analysis of Marquis' suicide hypothesis is made, most of the secondary evidence offered in its support is found to be less than convincing. Nevertheless, it still seems necessary to accept Indian participants' accounts and conclude at least some of Custer's men did in fact commit suicide. The Indians had little to gain by lying and much to lose psychologically. As Bruce Rosenberg [6] has pointed out, the suicides of Custer and his men would diminish the Indian's victory, in the same way that the suicides of the defenders of Masada diminished the victory of the besieging Romans. Nevertheless, suicides among Custer's troops probably played a small role in the outcome of the Battle of the Little Bighorn.

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Author's Response

Sir:

I really don't have any reply to his (Rowe) opinions regarding the suicide theory of Dr. Marquis. The purpose of my paper was not to validate or support the suicide theory, but to indicate that examination of the skeletons could substantiate or disprove the theory. I am really neutral about whether or not the suicide theory is valid, but it would be interesting to examine some of the skeletons from the Battle of the Little Bighorn.

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The Advantages of Simulated Crime Scenes in Teaching Forensic Anthropology

Sir:

Recently, a course in forensic anthropology was added to the Department of Anthropology curriculum at Memphis State University, Memphis, TN. We had the privilege of initiating this course and fashioned it similar to those described by Iscan [*1*] and Morse et al [*2*]. The course covers the basics of skeletal and hair identification and includes a section on forensic archaeology.

The goal of the forensic archaeology section is to introduce, through lectures and practical experience, fundamental archaeological techniques and to demonstrate the importance of these techniques in the recovery of forensic skeletal remains. To accomplish this, artificial crime scenes consisting of physical evidence and surface and subsurface biological remains were established before the course. Four large dogs, officially killed by the Shelby County Animal Shelter, were donated for this project. Each dog was given an apparent cause of death (for example, shot, stabbed, or bludgeoned) and placed with associated evidence (for example, articles of paper, cloth, plastic, and metal) in a secluded area of northeast Shelby County, TN.

Approximately three months later the crime scenes were investigated and the remains were recovered by students with limited assistance from the instructors. This exercise stressed recovery techniques, documentation, maintenence of the chain of evidence, and provided practical "hands on" experience for the participants. Students were availed the opportunity to experience the olfactory and visual sensations as well as the overall frustrations characteristic of actual forensic science cases.

The forensic archaeology project provided a means to test the efficiency of recovery and interpretive powers offered through archaeological technique. Students were able to recover a significant amount of the physical evidence including shell casings, fired bullets, credit card invoices, cigarette butts, and so forth. Shoeprints were also discovered from within the burials. Questions such as the time and cause of death, position of the body, postmortem changes, and the identity of the assailant were assessed with an appealing degree of accuracy. These assessments were significant considering few of the students had prior archaeological experience.

Since the determination of the time of death from decomposed remains is problematic, the exercise provided an opportunity to examine the variables influencing the rate of decomposition in this geographic area. The project was begun early in July 1982. Two dogs were placed on the surface of the ground and two others were buried in shallow graves. The decomposition rate of the surface remains was recorded and photographed every eight to ten days until they were recovered by the class in mid October, at which time they were compared to the subsurface remains. Temperature, rainfall, and insect and scavenger activities were monitored throughout the project. These observations contribute to a more concrete understanding of decomposition rates and enhance the ability to estimate the time of death in actual forensic science cases. In addition, the effects of trauma, such as fractures and knife and gunshot wounds, were demonstrated and the morphology resulting from injuries produced by high and low caliber weapons and shotguns was studied.

In conclusion, we recommend that when teaching forensic anthropology, the instructors include a section on forensic archaeology using simulated crime scenes and freshly killed animals. This provides an excellent teaching tool and practical experience for the students. It also provides a working laboratory from which the investigator can examine the efficiency of archaeological recovery strategies, the rate of decomposition, and the effects of various types of trauma to the skeleton.

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Discussion of "Minimal Velocities Necessary for Perforation of Skin by Air Gun Pellets and Bullets"

Sir:

It is always a professional pleasure to come across the forensic ballistics studies of Dr. DiMaio and collaborators. His wealth of data on energy dissipation in wound production by small arms ammunition, using the 20% gelatin gel targets [1,2] were duly correlated by us in the context of an earlier paper "Analysis of Gunshot-Wound Production" [3] whence

$$E_w \simeq 2klE_0, \qquad V \simeq k^+E_w, \quad \text{and} \quad (E_w/l)(m/d^2) = \Gamma E_0$$

where E_w = energy dissipated in wound production; E_0 = striking energy; l = length of the passage through the target; k = retardation factor; k^+ the expansion (cavity formation) coefficient; V = volume of temporary (pulsating) cavity, which is a small fraction of the permanent wound cavity; m = mass of the missile; d = its sectional diameter; and Γ = correlation coefficient.

The present article "Minimal Velocities Necessary for Perforation of Skin by Air Gun Pellets and Bullets" [4] is certainly additional useful data from Dr. DiMaio et al, but necessary clarification, more by way of correlating their data to the facts already available, though not all referred to in the article, is called for. This, I trust, will be of interest to the specialists in wound ballistics.

Incidentally, a few minor points may be dealt with first. While General Journée's data (1907) quoted by Professor Sellier [5] as "those (lead spheres of different diameters) over 50 m/s striking velocity perforate (d) the skin, independent of their size and energy. From a theoretical stand point of view the independence of (threshold) velocity from mass and energy cannot be comprehended," Dr. DiMaio et al quoted from the same Journée's article that 11.25-mm diameter lead balls (8.5 g) at 60 m/s produced only superficial skin damage on human cadavers (no penetration) but with those at 70 m/s, skin was perforated. In view of specific details and in the absence of us seeing the original (historical) article, we presume the latter to be a more precise enumeration of the celebrated general's experimental findings. It would appear from Dr. DiMaio's paper that mass effects on the threshold velocity for penetration have not been determined so far, which is not valid. A relationship has already been quoted by Sellier [5] namely,

$$V_{th} = 125/S + 22 \text{ m/s} \tag{1}$$

where S is the sectional density (mass/sectional area in g/cm^2). This relationship as shown by him gives unduly low V_{th} values and may not be examined further here.

More importantly, it is known that specific threshold energy = E_{th}/a is constant [5], dependent, naturally, on the skin thickness, age, and so forth. For normal adult skin thickness, on which various experimental studies have been carried out, a reliable value of E_{th}/a would itself yield the value of V_{th} for any given bullet or shot. This will be shown now feasible and in fact all the recent data of Wagner (quoted by Sellier [5]), DiMaio [4], and ours [6, 7] fits in well and the present DiMaio's data on .177, .22, and .380 air-rifle/pistol/revolver shots confirm the predicted values.

Sellier had fitted Wagner's and our data into a specific threshold energy ≥ 15 J/cm^2 (Fig. 3 of his cited article [5]). We have reexamined [7] our earlier data [6] in terms of the logarithmic dependence of penetration (that is, penetration $\propto \log V_{striking}/V_{th}$) derived by us earlier [8] and emphasized by Sellier [9]) and from there we have deduced the following:

$$E_{th}/a = 16.7 \text{ J/cm}^2 \tag{2}$$

$$\text{Drag coefficient} = 0.42 \tag{3}$$

and penetration l, into human soft tissue covered by skin,

$$l = 81.14\, R \log V_s/65 \tag{4}$$

where l is in centimetres, R the missile sectional radius in centimetres, V_s the striking velocity in metres per seconds, and 65 m/s is the threshold-penetration velocity measured by us for Buck 000 (≡ British LG of inverse sectional density of 0.126).

We shall now predict V_{th} values for missiles of different mass, diameter, or sectional density. From Eq 2 we have

$$\frac{m\, V_{th}^2}{2a} = 16.7 \times 10^7 \text{ ergs/cm}^2 = \text{constant}$$

TABLE 1—*Predicted and observed* V_{th} *for different missiles.*

Shot/Bullet	Inverse Sectional Density $1/S = a/m$, cm^2/g	V_{th}, m/s Calculated (Eq 5)	Observed	Author(s)
Buck 000 (≡LG) 4.5g 0.85 cm ϕ	0.126	64.9	65	Mattoo et al [6, 7]
0.38 Rev. 0.912 cm ϕ	0.0892	54.6	58	DiMaio et al [4]
0.177 air rifle 0.437 cm ϕ	0.280	96.7	101	DiMaio et al [4]
0.22 pump air-pistol 0.546 cm ϕ	0.219	85.5	75	Di Maio et al [4]
...	0.057	43.63	40.00	Wagner quoted by Sellier [5]
...	0.107	59.8	62	Wagner quoted by Sellier [5]
8.5 g lead shot 1.125 cm ϕ	0.117	62.5	more than 60 but less than 70[a]	Journée quoted by DiMaio [4]

[a]Estimated ($70 > V_{th} > 60$) but not precisely determined.

TABLE 2—*Evidence for constancy of* E_{th}/a.

Shot/Bullet	Inverse Sectional Density, cm^2/g	E_{th}/a, J/cm^2	Author(s)
8.5 g 1.125 cm ϕ lead shot	0.117	(<21.0 >15.4)[b]	Jounée quoted by DiMaio [4]
...	0.057	14.0[c]	Wagner quoted by Sellier [5]
...	0.107	18.0[c]	
Buck 000 (≡LG) 4.5 g 0.85 cm ϕ	0.126	16.7	Mattoo et al [6, 7]
0.38 Rev. 113 gr lead 0.912 cm ϕ	0.0892	19.0	DiMaio et al [4]
0.177 air rifle 8.25 gr 0.437 cm ϕ	0.280	18.1	DiMaio et al [4]
0.22 air pistol 16.5 gr 0.546 cm ϕ	0.219	12.7_5	DiMaio et al [4]

[a]Mean = 16.6_5, S.D. = $\pm 2.7_5$, and C.V. ~ 16.5%.
[b]See footnote in Table 1.
[c]Two of Wagner's data points on either side of 15-J/cm² plot drawn by Sellier [5], other data points on higher side.

where

$$V_{th} = \sqrt{334a/m} \times 10^3 \text{ cm/s} \qquad (5)$$
$$= \sqrt{334/S} \times 10 \text{ m/s}$$

where S is the sectional density of the missile, as defined above. Table 1 gives the calculated values of V_{th} compared with the experimentally observed values by different experimenters and Table 2 shows the constancy of E_{th}/a for varying sizes of shots/bullets (coefficient of variation ~ 16.5%).

Finally, it is of interest to add that while the various experimental data over the last three quarters of the century so beautifully fit in within the expected variance, an attempt to derive the V_{th} and from that the specific threshold energy E_{th}/a has been cited [*5,10*]. Sellier [*5*] worked out from Jauhari's integration [*10*] of the stress-strain curve of skin [*11*] E_{th}/a for 4 mm of human skin of the order of 21 to 30 J/cm^2, but on the *wrong* presumption of Jauhari's values which Sellier mistook as foot-pounds per cubic inch, while these actually were foot-poundals per cubic inch as reported by Jauhari and confirmed so by us. Thus the theoretically derived value for specific threshold energy E_{th}/a with Jauhari's approach works out to an unacceptable estimate of 1/32 (21-30) $\leq$ 1 J/cm^2 and therefore merits a fresh look into any theoretical evaluation thereof.

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Electrophoresis: A Continuation of the Discussion

Sir:

In a recent letter to the editor, I detailed some of the reasons why I feel that certain genetic analyses in forensic science should be viewed with scepticism (*Journal of Forensic Sciences*, Vol. 29, No. 1, Jan. 1984, pp. 8-12).

The basic point of my letter was that, if forensic scientists wish to have their work accepted scientifically, then they must follow accepted scientific procedures when they test samples in crime laboratories. Otherwise, their findings are suspect. These procedures are:

1. The use of controls in the fullest meaning of the term. The manual put out by the National Institute of Law Enforcement and Criminal Justice [*1*] recommends no controls for routine forensic science work for some enzymes. Furthermore, since identical samples treated in the same manner have been found to react differently to a given treatment [*1-3*], it is questionable whether true controls can exist for old, nonsterile samples with an unknown history.
2. In scientific laboratories, crucial tests are always repeated. Although the results of criminal laboratories have an effect on the accused's life and/or liberty, I have never seen a crime lab report that gives more than one test result on a given sample.
3. Proper attention must be paid to the effects of additives. Although the crime manual referred to earlier notes that the use of sodium fluoride as a preservative causes distortion of bands and the false appearance of a new band for some enzymes, it still recommends the use of it. This can lead to misidentification, as I stated in my letter [*4*].
4. Proper attention must be paid to the possibility of contamination. Despite what Dr. Sensabaugh states, I have never seen a crime lab report that mentions testing of possible contaminants. Misidentification is very possible [*4*] and should always be considered.
5. Pictures should be taken of the results so that leisurely examination of them and independent verification may be conducted. These pictures constitute the raw data which is interpreted by the person doing the analysis; by scientific standards, their absence means that there is no data. It is never sufficient to write PGM-1 or PGM 2-1 on a piece of paper. I have never seen a crime lab report that includes pictures of electrophoresis results.

I have seen a crime lab report in which a result was given on the testing of a drop of blood from a tennis shoe six months after a crime was committed. Given that aging effects [*5-10*] occur and that sweat changes the properties of blood [*11*], to say nothing of contamination effects [*12-16*], no attempt to test should have been made. In a rape case, I saw a crime lab report from which only two conclusions could be drawn: either the victim's vagina was not genetically related to her or there was an error in the test. The suspect still went to trial, and the crime lab report was part of the evidence against him. In the first case, the analyst did not properly respect the limits of testing and, in the second, he did not recognize that an error had been committed. I could cite more examples in which poor judgement by crime lab analysts has been displayed.

Following Dr. Sensabaugh's lead, I would like to add to this debate by introducing a new element, the statistical interpretation of the results. One often sees in crime lab reports that a given isozyme result is found in X% of the population. Results of the testing of several enzymes are then multiplied to yield a final probability that the suspect is the source of a particular sample. While many questions could be raised about this practice, I shall ask only one at the moment: in what way does the population of California, the source of much of the reference data, match the population in states like Georgia (which has a very small oriental population as compared to California) or Connecticut (which has a very high Italian population as compared to California)? Obviously, the California data is nonapplicable to these and other states. Crime lab analysts, whom Dr. Sensabaugh says are highly trained, routinely make probability statements concerning their results. It is absolutely incorrect and very misleading to do so.

In conclusion, I stick by my initial assertion: the electrophoresis data coming out of crime laboratories is highly suspect for a variety of reasons.

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