Letters to the Editor

Letters to the Editor will be confined to discussions of papers which have appeared in *The American Statistician* and of important issues facing the statistical community. Letters discussing papers in *The American Statistician* must be received within two months of publication of the paper; the author of the paper will then be given an opportunity to reply and the letters and reply will be published together. All letters to the Editor will be refereed. Corrections of errors which have been noted in papers published in *The American Statistician* will be listed in Corrigenda at the end of this section.

THE PROBLEM OF NUMERACY: MOUNT EVEREST SHRINKS

A.S.C. Ehrenberg (1981) argues that rounding to two digits may increase numeracy, i.e., people's ability to cope with numbers. However, rounding large numbers to two significant digits can present problems in how these numbers are presented and used. I found the following to be an interesting example of dealing with "nice" numbers.

The following filler with a United Press International credit line was printed in the *Pittsburgh Press* (May 19, 1981, p. B-11):

> Mount Everest Shrinks
>
> Mount Everest is exactly 29,000 feet high but surveyors worried that the public would consider that an estimate, so they falsely reported the height as 29,002, a figure accepted since 1850.

The use of the word "exactly" did not imply rounding, and I wondered where UPI got the number 29,000. A quick check of *Whitaker's Almanack* (1981, p. 205) and the *New Encyclopaedia Britannica Macropaedia* (1981, Vol. 12, p. 585) produced a figure of 29,028. The *Columbia Viking Desk Encyclopedia* (1960, p. 430) gave a value of 29,141 feet for the height of Mount Everest.

According to the *World Almanac* (1981, p. 440), the number 29,002 was produced by a survey team using a triangulation method in the middle 1800's and is still used by many mountaineering groups. The present official height, based on a 1954 Indian government survey, is 29,028 feet, plus or minus 10 feet because of snow. This figure is also accepted by the National Geographic Society.

This still left the 29,000 figure given in the UPI article. A call to the UPI newsdesk in New York identified the source as the *People's Almanac* (1978, p. 605). Part of the entry from this almanac helps complete the story and is quoted below.

> The first official survey of Everest took place in 1852. The surveyors took measurements in six places and derived an average figure of 29,000 ft. This seemed too much like a round-number estimate for an official report, so they added 2 ft. to their published finding to make the height 29,002. An Indian team surveying in 1954 found the mountain to be 29,028 ft., 26 ft. higher than the 1852 "estimate."

Therefore, there does not seem to be a source that indicates Mount Everest is *exactly* 29,000 feet. In conclusion, it seems the original British surveyors were not interested in people's numeracy, but rather their basic skepticism of "nice" numbers.

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References

EHRENBERG, A.S.C. (1981), "The Problem of Numeracy," *The American Statistician*, 35, 67–71.

The Columbia-Viking Desk Encyclopedia (1960, 2nd ed.), New York: Viking.

The New Encyclopaedia Britannica Macropaedia (1981), "Mountaineering."

The People's Almanac #2 (1978), "Mount Everest."

World Almanac and Book of Facts (1981), "Height of Mount Everest."

World Book Encyclopedia (1978), "Mt. Everest."

NUMERACY AGAIN

Professor Ehrenberg's rule (4) (Ehrenberg 1981) of rounding of two effective digits is not new. Although he does not state the rule explicitly, Playfair (1801) has presented his data mostly to two digits and makes the following comment.

> As statistical results never can be made out with minute accuracy, and that, if they were, it would add little to their utility, from the changes that are perpetually taking place, it has been thought proper in this work to omit the customary ostentation of inserting what may be termed fractional parts, in calculating great numbers, as they only confuse the mind and are in themselves an absurdity.

It is a pity that the statistical community largely has not taken this suggestion to heart and that we are in the same state of affairs 180 years later. Let us hope that this latest advice is followed.

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References

EHRENBERG, A.S.C. (1981), "The Problem of Numeracy," *The American Statistician*, 35, 67–71.

PLAYFAIR, WILLIAM (1801), *Statistical Breviary*, London: Bensley.

REPLY

Most people know that exactly 29,000 ft. means about 29,000 ft. So Dr. Stegman need not worry unduly. The rounding rule also explicitly avoids over-rounding for measurements like 29,002, 20,028, and so on by saying that we should round to two *effective* digits (i.e., ones that vary in that context), not to two *significant* ones.

But for most purposes the height of Mt. Everest is about 29,000 ft.; for example, when climbing the last few feet (. . .; 29,000; 29,001; 29,002) and even when answering Sellar and Yeatman's (1932) old exam question: "Mt. Everest is 29,002 ft. high. Do you consider this sufficient?"

Good precepts such as drastic rounding should reflect best practice, like what William Playfair did in 1801. What may help wider adoption now is (i) repetition, (ii) being more explicit about rounding to *two* digits, and (iii) our better understanding of just how and why more than two digits "confuses the mind"—that we cannot keep longer numbers in our short-term memory when doing mental arithmetic.

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References

SELLAR, W. C., and YEATMAN, R. J. (1932) *And Now All That*, London: Methuen.

IMPRACTICAL BOUNDS ON DIFFERENCES OF ORDER STATISTICS

Fahmy and Proschan (1981) established that a sharp upper bound on the difference between two order statistics x_k and x_l $(k < l)$ is the quantity $[n(n-l+1+k)/k(n-l+1)]^{1/2}\cdot s$, where n is the sample size and s the sample standard deviation. The result follows from considering the minimum possible value for the sample standard deviation given the specific values of the two order statistics. The minimum occurs when all lower order statistics are identical with x_k, all higher ones are identical with x_l, while those in between are at a weighted average of x_k and x_l, weighted so that they do not contribute to $\Sigma(x-\bar{x})^2$.

While the Fahmy-Proschan bounds must be followed by every set of numbers, they bear no necessary relation to any other set. Yet Fahmy and Proschan suggest that their bounds may be useful in connection with testing outliers and for setting conservative waiting times between successive failures.

As they show, for $n = 1{,}000$, the upper limit on the difference between the two middle order statistics is $2.00s$, which is also the general lower limit on the difference between the extreme order statistics. Their upper limit, however, on that extreme difference is $s\cdot(2n)^{1/2}$, or for $n = 1{,}000$, $44.72s$.

Things get more puzzling when we apply the Fahmy-Proschan result to seeking outliers. The upper bound on the difference between the first two or between the last two order statistics is $ns/(n-1)^{1/2}$, (for $n = 1{,}000$, $31.64s$). This seems to be an unusually large gap that we are required to see before we may assert that we have an outlier. But then, it is not much more bizarre than a gap requirement of $2.00s$ at the center. Of course, it wouldn't look like a gap anyway, since whatever observations were made, they would have to obey the Fahmy-Proschan bounds.

Simply put, the Fahmy-Proschan bound cannot be properly used for the purposes they suggest.

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Acknowledgment

This research was supported by Grant RD 102 from the American Cancer Society, and by Grant CA 30205 from the National Cancer Institute.

References

FAHMY, S., and PROSCHAN, F. (1981), "Bounds on Differences of Order Statistics," *The American Statistician*, 35, 46–47.

REPLY

Nathan Mantel points out that our bounds (Fahmy and Proschan 1981) on differences of order statistics are not practical in testing outliers and setting conservative waiting times between successive failures. He is quite right. The bounds should not be used as proposed in Section 2 of our note.

The bounds are correct and are sharp. But they would hold equally well for a sample containing an outlier.

In our note, we reference Thomson (1955) for the sharp lower bound on the sample range given in (1.7). Professor K. R. Nair has kindly furnished an earlier reference for this bound; see Nair (1948).

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References

FAHMY, SALWA, and PROSCHAN, FRANK (1981), "Bounds on Differences of Order Statistics," *The American Statistician*, 35, 46–47.
NAIR, K. R. (1948), "Certain Symmetrical Properties of Unbiased Estimates of Variance and Covariance," *Journal of the Indian Society of Agricultural Statistics*, 1, 162–172.
THOMSON, GEORGE W. (1955), "Bounds for the Ratio of Range to Standard Deviation," *Biometrika*, 42, 268–269.

CORRIGENDUM

The conjecture that the integer-maximization method proposed in Dahiya (1981) is extendable to the case of several integer parameters is not true. Washburn (1975) has also investigated this method and gives an example where this method does not work in two dimensions without some further restrictions on the function.

I would like to thank Bruce L. Golden for bringing the reference to my attention.

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References

DAHIYA, RAM C. (1981), "An Improved Method of Estimating an Integer-Parameter by Maximum Likelihood," *The American Statistician*, 35, 34–37.
WASHBURN, A. (1975), "A Note on Integer Maximization of Unimodal Functions," *Operations Research*, 23, 358–60.

"LEAST SQUARES MEANS" vs. "POPULATION MARGINAL MEANS"

Searle, Speed, and Milliken (1980) have proposed the term "population marginal mean" (PMM) as an alternative to "least squares mean." They write "We emphasize that a PMM is a function of parameters of a linear model. It is this aspect of least squares mean that we find missing in the literature."

The term least squares mean is used to denote a least squares estimate of a linear combination of parameters (Harvey 1960) and not a linear combination of parameters themselves, and is evidently used as an abbreviation. However, Searle and Henderson (1978) have written "the least squares mean of an effect is defined as the linear combination of parameters obtained by averaging the right-hand side of the model equation . . . over all subscripts except those pertaining to that effect." Further, they proposed in a SAS GLM context the term "estimated least squares mean." Literally, this would mean "the least squares estimate of the least squares mean"! In Σ-restricted models such as those used by Harvey (1960), least squares means provide estimates of well-defined estimable functions except for functions involving interactions associated with missing cells. The problem, in general, is which linear combination of parameters is being estimated, not whether the estimate is called a least squares "constant," "mean," or something else.

Searle, Speed, and Milliken's use of the concept of "marginality" may be clear, perhaps, when reference is made to some sort of averaging about levels of some classification in a multiway table. However, if one is interested in estimating the mean of a cell in the table, the word marginal is misleading. For example, in the two-way classification with interaction, that is, $E(Y_{ijk}) = \mu + \alpha_i + \beta_j + \gamma_{ij} = \mu_{ij}$, it is difficult to see how labeling μ_{ij} "a population marginal mean for γ_{ij}" can be meaningful or informative. I disagree with the authors' statement that the term least squares mean "carries a strong sense of marginal mean." If interaction is present, it can be argued that one would be interested only in the μ_{ij}'s and, perhaps, in their least squares estimates (or "least squares means"). Again, marginality has little relevance here and the term least squares is more informative than marginal since at least the method of estimation is indicated.

Least squares mean is more general than estimated population marginal mean. For example, in a two-way classification with interaction and filled cells it does not seem sensible to call the estimable function $\alpha_i - \alpha_{i'} + \gamma_{ij} - \gamma_{i'j}$ a population marginal mean. One may choose to estimate this function by least squares if the covariance matrix of the errors is scalar, or by generalized least squares otherwise. The statistical literature has already offered the terms estimable function and best linear unbiased estimate (BLUE) of such a function. Let's stay with this latter unambiguous terminology!

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References

HARVEY, W.R. (1960), "Least Squares Analysis of Data With Unequal Subclass Numbers," Report No. ARS-20-8, Agricultural Research Service, Beltsville, MD: U.S. Department of Agriculture.

SEARLE, S.R., and HENDERSON, H.V. (1978), "Annotated Computer Output for Analysis of Unbalanced Data: SAS GLM," Paper No. BU-641-M, Biometrics Unit, Cornell University.

SEARLE, S.R.; SPEED, F.M.; and MILLIKEN, G.A. (1980), "Population Marginal Means in the Linear Model: An Alternative to Least Squares Means," *The American Statistician,* 34, 216–221.

REPLY

Gianola's letter illustrates the confusions that shroud the term "least squares mean" (hereafter denoted by lsm). Statisticians have never felt a need for the concept involved, and it is only the appearance of lsm as a label on output from the SAS GLM statistical computing package that has prompted the need for an adequate statistical description. Basically, lsm denotes a least squares estimate of a linear combination of parameters. But, and it is an all-important "but," until our paper, there has been no definition of what that linear combination is. Furthermore, this lack of definition has been misleadingly aggravated by using the name of an estimation method (least squares) as part of the name of the up-to-now undefined parametric concept. What the paper does is to provide (a) in the spirit of the apparent meaning of an lsm, a definition of which linear functions of the parameters of a linear model may sometimes be of interest, (b) a name for those functions indicative of their meaning (a property of any good name), and (c) a discussion, with examples, of when those functions are estimable and when they are not, since only when a function is estimable can it be estimated uniquely by the corresponding best linear unbiased estimator (BLUE).

The crux of this procedure is (a): defining appropriate linear functions of parameters. And this is the very aspect of the lsm that has heretofore been ignored, and that has therefore left lsm outside the general body of theory of linear models. For example, careful reading of Harvey (1960), which Gianola refers to twice, reveals at least three places where the definition of lsm is touched on (using α_i here in place of Harvey's a_i):

(i) "the lsm, $\hat{\mu} + \hat{\alpha}_i$," (on p. 5, for the one-way classification).
(ii) "Estimates of the $\mu + \alpha_i$, the lsm for A-classes" (on p. 39, for the two-way classification without interaction).
(iii) "The lsm for the classes of A ... are $\hat{\mu} + \hat{\alpha}_i$," (on p. 58, for the two-way classification with interaction, $E(y_{ijk}) = \mu + \alpha_i + \beta_j + \gamma_{ij}$).

These quotes illustrate the confusion associated with the name lsm. Sometimes (as in (i) and (iii), and in output from SAS HARVEY) it refers to functions of solutions to normal equations, and sometimes (as in (ii)) it refers to functions of parameters. Output from SAS GLM uses it both ways. And, although SAS GLM sometimes describes an lsm as nonestimable, nowhere else have we seen, prior to our paper, any mention of the estimability of an lsm. For example, all three of (i), (ii), and (iii) give the impression of defining the lsm for the ith A-effect as $\mu + \alpha_i$. This is fine for (i) the one-way classification, where $\mu + \alpha_i$ is estimable. It is also estimable in (ii) if the β_j's are defined to satisfy $\Sigma_{j=1}^{b}\beta_j = 0$ (a Σ-restriction); and it is estimable in (iii) provided there are also no empty cells in the ith level of the A-factor and provided the β_j's and γ_{ij}'s are defined to satisfy $\Sigma_{j=1}^{b}\beta_j = 0$ and $\Sigma_{j=1}^{b}\gamma_{ij} = 0$. Thus what might appear to be a uniformly applicable definition is in reality confounded by different conditions for different models, insofar as estimability is concerned. The definition of our paper avoids this, it includes the three cases implicit in (i), (ii) and (iii), it reveals weaknesses therein, it applies to more general cases, and it provides a general understanding of the inherent concept.

For example, in (ii), the parametric function being estimated is actually

$$\mu + \alpha_i + \sum_{j=1}^{b}\beta_j/b. \tag{1}$$

Since (1) is estimable, its BLUE exists and is obtainable from any set of solutions μ^0, α_i^0, and β_j^0 to the normal equations whether $\Sigma\beta_j^0 = 0$ is satisfied or not. Furthermore, no matter what solutions β_j^0 are used, defining the β_j's to satisfy $\Sigma\beta_j = 0$ is only game-playing; it is not a necessary condition for estimating (1), and the resulting BLUE is the BLUE of μ plus α_i plus the mean of the β_j's in the data. The same sort of thing is true for (iii):

$$\mu + \alpha_i + \sum_{j=1}^{b}(\beta_j + \gamma_{ij})/b \tag{2}$$

is an estimable function provided there are data in every B-class in the ith A-class (i.e., no empty cells in that A-class).

Gianola is at complete liberty to disagree with our idea of marginal mean, but in doing so he chooses to deny hard facts: for the model without interaction, (1) is exactly the marginal mean of $E(y_{ij})$ for the ith A-class. The idea of marginality *is* therefore inherent in the (heretofore unstated) ideas of the lsm. What else could it possibly be conceived as? This being so, and because (as already explained) the name lsm is an anachronism in its involving the name of an estimation method as part of the name for a function of parameters, it seems appropriate to have a name that incorporates this basic concept, namely that of a population marginal mean; ergo, the name PMM.

Not only is least squares totally inaccurate as part of the name for a parametric function, but Gianola's suggestion that "at least the method of estimation is indicated" is redundant because this is the only method of estimation usually associated with linear models. "Least squares" is therefore both wrong and unnecessary for naming a function of parameters.

We would agree that a PMM for γ_{ij} in the model $E(y_{ij}) = \mu + \alpha_i + \beta_j + \gamma_{ij}$ is not very informative. But since it is generated as computer output by SAS GLM it warrants explanation, and as such it is totally consistent with our definition of PMM. Furthermore, in models with more than two factors, such as $E(y_{ijkl}) = \mu + \alpha_i + \beta_j + \gamma_k + \delta_{jk}$ of (6.14) of the paper, $\text{PMM}(\delta_{jk}) = \mu + \bar{\alpha} + \beta_j + \gamma_k + \delta_{jk}$ is well defined, is part of the general definition of PMM, and may be useful in some circumstances.

Gianola's last paragraph is very confusing. Since lsm has no firm definition, how can it be described as more general than a PMM? By his own admission this cannot be so because lsm provides "estimates of estimable functions except for functions involving interactions associated with empty cells." In contrast a PMM is always defined; it may, of course, not always be estimable. And the example $\alpha_i - \alpha_{i'} + \gamma_{ij} - \gamma_{i'j}$ is a complete non sequitur; no one has ever suggested that such a thing be called an lsm or a PMM. The only part of this last paragraph of the letter that merits attention is its penultimate sentence. Estimable functions are well defined, as are their BLUE's. And PMM's are particular linear combinations of parameters that are sometimes of interest: in some cases they are estimable, with corresponding BLUE's. And sometimes they are not estimable.

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