improvement in core recovery and normalization procedures, including adjustment for plate motion; and better knowledge of the effect of diagenetic alteration of volcanic ash layers.

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7. The volcanic ash in the oceanic regions we differentiated may be derived from various sources. The assumption that the Middle Miocene ash peak in the Indonesian section is derived from the occurrence of ash in site 212 south of Indonesia is incorrect; it is based on ash in site 292 near Luzon, Philippine Islands.
8. Ninkovich and Donn do not seem consistent in what they consider to be an ash layer or an ash zone. For example, in site 211 they refer to five Quaternary ash layers, and yet in the leg 22 Initial Report two layers and three zones are distinguished. In contrast, they disregard Early Pliocene ash zones (25 to 75 percent ash) in sites 212 and 213.
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16. Donnelly (*13*, p. 586) interprets the curve of volcanogenic abundance for site 292 as indicating that the volcanic activity of Luzon in the Oligocene was moderate, rose to a maximum near the Oligocene-Miocene boundary, and decreased in the earliest Miocene. A hiatus prevents evaluation of the Early Miocene, but the Middle Miocene is inferred to be a period of moderate volcanic activity, diminishing toward the Late Miocene and increasing to a probable maximum during the Late Miocene (12 to 7 million years ago). Activity began to increase significantly again during the latest Cenozoic at about 3 million years ago and reached a maximum during the Recent.
17. Donnelly (*13*) interprets the curves for site 296 as indicating a peak in volcanic activity in the late Early Miocene to Middle Miocene, declining toward the Late Miocene; a short-lived less significant peak about 9 million years ago; declining activity through most of the Late Miocene; and sharply increased activity about 5 million years ago. This latest episode can possibly be divided into early and late active phases and a middle somewhat less active phase (*13*).
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26. In site 211 the Initial Reports indicate three ash layers in the Late Pliocene; not one as mentioned in Ninkovich and Donn (*1*). Sites 212 and 213 contain Early Pliocene ash zones not mentioned by Ninkovich and Donn.
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Use of Correlation Statistics with Rubidium-Strontium Systematics

Although sample correlations are prominently displayed in their data table, Brooks *et al.* (*1*) do not say just how they have used r in deciphering the role of ancient lithosphere in young continental volcanism. What explanation they offer is relegated to reference 20, which records that "the data have been statistically evaluated by (i) calculating the Pearson correlation coefficient . . ., and (ii) testing the slope of the fitted line for significance against zero" [(*1*, p. 1093); see also (*2*)] In the experimental situations in which it is usually considered relevant, a test of the observed regression coefficient, b, against the hypothesis that the parent $\beta = 0$ reduces to a test of r against the hypothesis that $\rho = 0$ (*3*). Clearly, such a test is meaningful only in the event that ρ would indeed be zero in the absence of nonrandom linear association of the variables. As between variables that are ratios with a common or nearly common denominator, the type used both here and in related earlier work (*4*), however, it has been known at least since 1896 (*5*) that ρ will *not* be zero, and it is also implicit in the argument that $\beta \neq 0$ (*6*), in the absence of nonrandom linear association of the variables.

In most discussions of the Sr-Rb procedure for determining geological age the variables are given as $^{87}Sr/^{86}Sr$ and $Rb/^{86}Sr$, a clear example of correlation between two ratios having a common denominator; even if the three terms of the ratios are completely uncorrelated, the ratios will be positively correlated. In the current work the denominator of the second ratio is Sr rather than ^{86}Sr, which complicates but does not mitigate the situation.

It is possible, of course, that the common element bias is negligibly small, but this is a matter requiring demonstration or convincing rationalization in each specific case. That the denominator may perhaps be serving only as a convenient scaling device provides no escape; use of an uncorrelated scaling variable whose variance is sufficiently large will impose strong positive correlation on *any* pair of variables whose negative correlation is less than perfect. That the scaled data cluster closely about a regression line is then merely a consequence of the choice of scaling variable and says nothing about the nature of the relation between the variables being scaled. Given appropriate information about the sample of distribution of Sr it probably would be possible to generate, whether by exact calculation, approximation, or simulation, null values for ρ and β reflecting the common element effect on relations between the ratios of interest; observed correlations and regressions could then be tested against these, rather than against zero. Or if the bias in either proved very small, it could be ignored. The necessary information about Sr is not given in the article by Brooks *et al.*, however, and may not be available. In its absence appropriate null values for ρ cannot be obtained, but it does not follow from this that they are not needed.

By an analysis of covariance it is sometimes possible to test the significance of differences between regression coefficients without regard to correlation (*7*), and a numerical reduction of this type might provide a useful sample description. But a statistical test of the resulting variance ratio would be valid only if it were reasonable to assume that one of the isotope ratios was independent and the other was dependent upon it. The relation of the variables in question, however, is clearly one of interdependence rather than the dependence-independence implicit in standard regression analysis.

The authors' choice of a form of regression analysis suitable for interdependent variables is certainly appropriate, but if they then want to argue along the lines of reference 20 in (*1*), they are obligated both to demonstrate that the particular regression coefficient they use is free of common element bias and to explain just how r measures goodness of fit about their chosen regression line. They seem unaware of either obligation.

Their silence on both matters raises the question of just what role the product-moment correlation may play in their argument. There would appear to be only two possibilities; either they are not using it at all or they are using it without appropriate null values for ρ. In either case, and for as long as such values are unavailable, I believe it would be preferable to entirely eliminate correlation statistics, and arguments based upon them, from research and reportage on the problem they discuss. What would then remain of their current interpretation of regression coefficients would be essentially an argument from authority. In the circumstances this would perhaps be not altogether undesirable; an argument from authority is usually less confusing and less susceptible of misunderstanding than one based on a false premise, and that is what they now offer.

From time to time in every actively developing branch of natural history the ability to accumulate data outstrips the ability to evaluate them. Pending the development of suitable testing procedures the naturalist who wishes to speculate is then obliged to argue from a combination of authority, fashion, and subjective plausibility. To the extent that it rests on the evaluation of differences between regression angles, this, it seems to me, is the status of current speculation about the role of isotope and trace element ratios in deciphering the influence of ancient lithosphere on young continental volcanism.

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Chayes has resurrected Pearson's (*1*) "spurious correlation" effect in criticizing the statistical treatments we recently applied to Rb-Sr isotope data (*2, 3*). Chayes's criticism, on face value, not only threatens to undermine the manner in which we treated Rb-Sr mantle isochrons, but puts in question the very essence of Rb-Sr geochronology—namely, the regression treatment of Rb-Sr isochrons. Thus, in replying to Chayes, we must defend not only our recent Rb-Sr mantle isochron proposal but also the Rb-Sr isochron concept in general. We believe that we can successfully do both.

Chayes's criticism for using ratio correlations without providing reference values for the expected slopes and slope deviations is well taken. With little information available on the distribution of regression coefficients in a situation involving a common denominator in both variables, we performed Monte Carlo studies closely modeling the parameters of the geological samples in our recent papers (*2, 3*). Three by one thousand small samples ($N = 10$) taken from a standard normal deviate (0, 1)—that is, random samples with a correlation coefficient (r) of zero—were transformed to ratios X_1/X_2 and X_3/X_2 approximating the empirical sample parameters of (7, 7), (10, 1), and (0.264, 0.153).

With coefficients of variation of that size the derivations for the expected values of ρ could not be used. The predicted positive bias, however, turned out to be quite small and very unstable. Only 57 percent in each of the 3 × 1000 samples showed a shift toward a positive correlation with an average increase in r of 0.07; the average decrease observed in r was −0.06. Accordingly, ratio correlations of the 3 × 1000 samples stayed within ± 0.01 of the zero expectation for random samples.

Theoretically, the ratio correlation bias should tend to zero with increasing correlation in the variables before transformation. Consequently, we performed a further Monte Carlo study similar to that above but using a sample population in which correlation (of approximately + 0.5) existed before ratio transformation. In these cases the ratio correlation effect hardly modified the original correlation coefficient at all.

It remains to apply these observations to Rb-Sr isochrons and Rb-Sr mantle isochrons. The framework of Rb-Sr systematics is model-dependent. The variables involved are considered to be highly interdependent, in view of the decay relationship between ^{87}Rb and ^{87}Sr, fixed isotopic ratio between ^{86}Sr and ^{88}Sr, and initial ratios between ^{87}Sr and ^{86}Sr at the time of rock formation. Hence, for Rb-Sr isochrons the sample population is assumed to be perfectly positively correlated, independent of any ratio correlation effect. Most Rb-Sr isochrons [for example, Heemskirk Granite (*4*)] do reflect this, with correlation coefficients of + 0.999 or more being typical. Thus Chayes's ratio correlation argument has no effect on the statistical parameters of isochrons. For Rb-Sr mantle isochrons we have interpreted the data according to the constraints of the physical processes assumed to govern Rb-Sr systematics. The facts that the data generally show high degrees of correlation (commonly above + 0.8) and that the ratio correlation effect under these circumstances is trivial indicate that Chayes's criticism can also be safely ignored for most Rb-Sr mantle isochrons. However, in the interpretation of slopes no normative statistical statement should be accepted, but each plot should be examined in the light of all available information.

In conclusion, Chayes's criticism, although well founded, damages neither the statistical treatment we used in appraising Rb-Sr mantle isochrons nor the statistical treatments applied to Rb-Sr isochrons in general. Pearson correlation coefficients may be safely used if desired, but the danger of spurious correlations for Rb-Sr data that are *poorly* correlated to begin with must not be overlooked. We thank Chayes for spurring our efforts in this direction, a direction that we had not previously thoroughly explored.

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