Remarks on Correlation Methods in Geophysics

By JAMES E. McDONALD, Institute of Atmospheric Physics, University of Arizona, Tucson, Arizona.

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Abstract

From consideration of several studies of the sampling properties of the product-moment correlation coefficient r it is concluded first that undue concern has been expressed for the problem of non-normality in correlation studies in geophysics, and second, that use of geophysically adequate significance tests and confidence limits for r can almost always be achieved through use of the simple standard error of r. A growing tendency in climatology and hydrology to employ unnecessarily elaborate methods appears to stem from unrealistic emphasis which mathematical statisticians frequently place upon theoretical refinements, emphasis that loses sight of the limits of accuracy inherent in the very type of data usually subjected to statistical analysis.

1. Introduction

This paper has two principal objectives: First, to question a rather common tendency to employ hollow refinements in correlation analyses in geophysics; and second, to display some empirical results in correlation analysis that should be of interest to the geophysicist seeking examples that may guide him through what is to many a rather bewildering array of warnings in the statistical literature on correlation methods.

I shall try to show that in most of the correlation analysis that investigators carry out in geophysical studies especially in the fields of meteorology, climatology and hydrology, statistical elaborations beyond the simplest and most familiar techniques and tests cannot ordinarily be expected to yield refinements of real geophysical significance. In addition, I shall present arguments for concluding that the pitfalls said to be inherent in correlating *non-normally* distributed variates are not likely to warrant serious concern on the part of the geophysicist.

Three references concerned with the applications of statistical methods in geophysical problems (chiefly climatological problems) have recently appeared, that of CONRAD and POLLAK (1950), that of BROOKS and CARRUTHERS (1953) and that of PANOFSKY and BRIER (1958). The first offers only the barest details of the sampling theory of the product-moment coefficient, yet it is the writer's view that, in most if not all climatological and hydrologic correlation analyses, the investagator is probably well-advised to remain little more sophisticated in his statistical treatment of his data than are CONRAD and POLLAK in their treatment of correlation. Specific reasons for this perhaps unorthodox view will next be summarized briefly.

2. The normality problem

First, it must be noted that in none of the three recently published references cited above is it made quite clear that all of the product-moment correlation *theory* discussed applies rigorously only to *normal* bivariate popula-

tions. On the other hand, the non-statistician who consults the literature of mathematical statistics is almost invariably disturbed at every turn by warnings against applications of normal-theory statistics to non-normal populations. However, the reader of these latter references is seldom given the slightest inkling of how large an effect non-normality in his data might have on the final conclusions he may draw, so it is regrettable that this practical question is not confronted in any of the three cited references dealing with applications to meteorology and climatology.

"Effect of non-normality" here takes on two slightly different meanings associated with two statistical questions: First, the investigator wishes to know whether a computed coefficient constitutes an unbiased estimate of the population value of the correlation coefficient if derived from non-normal data, and second he wishes to know whether non-normality can seriously interfere with significance tests if based on normal-distribution theory. STIDD (1953) has suggested, for example, that most *precipitation* data must be regarded as suspect, on grounds of non-normality, with respect to exactly such questions as these two.

Concerning the first of these two questions, that of *bias* in r, it is often suggested, though typically only in vague terms, that possible bias due to non-normality can be avoided by use of one or another "normalizing transformation". STIDD (1953) urges that in correlating precipitation data one use a cube-root transformation to suppress what he reports as a negative bias in r due to the well-known non-normality of most rainfall distributions.

The literature contains nothing approaching a definitive analysis of the advantages to be gained in correlating data normalized by such transformation, so it seemed a useful exercise to carry out at least a modest number of product-moment correlations on some actual geophysical data of markedly non-normal characteristics, first using raw data and then using each of several transformations thereof. Since the data were precipitation data and since the investigator may find in the literature, in turn, suggestions that such data be transformed by square root, or cube root, or logarithmic transformations (among others), I have tested each of those three. The test data were all taken from long-record U.S. Weather Bureau stations in Arizona. It is well known that precipitation frequency-distributions from arid-region stations such as those in Arizona are notoriously non-normal and usually highly skewed. Hence the present analysis should constitute a fairly extreme test of the true value of various normalization transformations.

The results are displayed in Table 1. Seven station-pairs, with record-lengths varying from 50 to 83 years are included in this sample. Their records were broken down into "winter" and "summer" half-years, "winter" being the period from November through April, and "summer" the balance of the year. The product-moment correlation coefficients for the untransformed (raw) data are indicated by "linear", and the figures in parentheses following these values are the associated standard errors. Table 1 represents correlations computed for fourteen independent bivariate distributions, and of the forty-two coefficients

Table 1. Effect of three normalizing transformations

		Station-pair[1]						
		AB	AC	AD	AE	BC	DE	BE
	No. of years record	64	76	50	83	64	50	63
Summer	Linear	.54 (.09)	.38 (.10)	.44 (.12)	.19 (.11)	.61 (.08)	.63 (.08)	.22 (.12)
	Square root	.55	.41	.47	.24	.65	.62	.36
	Cube root	.57	.42	.46	.26	.65	.63	.42
	Log	.57	.42	.38	.28	.69	.57	.48
Winter	Linear	.78 (.05)	.82 (.04)	.58 (.09)	.65 (.06)	.85 (0.3)	.57 (.10)	.68 (.07)
	Square root	.80	.85	.60	.62	.85	.57	.65
	Cube root	.72	.78	.52	.63	.83	.60	.66
	Log	.74	.76	.59	.61	.81	.62	.64

[1] Key to station pairs: A — Tucson, B — Natural Bridge, C — Phoenix, D — Flagstaff, E — Yuma.

computed from the three types of transformed data it will be seen that only six values are even as much as one standard error away from the linear r. Only one is more than two standard errors away from the linear value (Natural Bridge-Yuma log data). The results of Table 1 argue strongly that only negligible gain is to be achieved in subjecting even highly non-normal data to normalization transformations as far as any effects on r are concerned. If such minor changes in r-values are produced by applying these several normalizing transformations to arid-zone precipitation data, it seems clear that in analysis involving less skewed distributions from more humid regions, no significant changes in the value of r could result from use of these transformations. STIDD (1953) took note only of the fact (confirmed in the present studies) that the cube-root transformation does symmetrize the frequency-distribution noticeably, but he seems only to have inferred that this essentially graphical change would be accompanied by significant changes in values of r computed from transformed data. The present results indicate that the latter changes are of no consequence.[1]

With respect to the second question, cited above, that of making significance tests for correlation coefficients derived from non-normal bivariate populations, the geophysicist should realize that *theory* is at present incapable of yielding decisive answers. Statisticians have consequently resorted to *experimental* sampling to secure estimates of this effect of non-normality; but this work was done so many years ago that it seems to have been lost to the present attention of most geophysicists. Hence I feel that a few examples of such studies warrant brief recapitulation. A substantial amount of such experimental sampling was, for example, done by E. S. Pearson and his collaborators early in the 1930's, with the objective of testing non-normality effects on a number of statistics, including the product-moment correlation coefficient.

In one interesting study PEARSON (1931) set up four different parent bivariate populations of pure numbers distributed with varying degrees of non-normality. Although all of his distributions were much smoother, than, say, typical precipitation or streamflow frequency-distributions, they exhibited skewness and kurtosis of a degree that the climatologist would regard as rather severe if found, for example, in precipitation data. From these synthetic populations, Pearson experimentally drew 250 successive samples of 10 pairs each and then 250 more of 30 pairs each, except for one case where he used 395 samples of each of these two sizes. In each instance the sampling scheme corresponded to sampling from a parent bivariate non-normal population of *zero* correlation. From his eight series of samples he formed frequency-distributions of r and from these he determined the experimental "standard error" for comparison with the corresponding value computed on the assumption that normal theory could be used. For samples of 10 items and a "normal theory" standard error of 0.3333 (the choice of using four digits is Pearson's, not the writer's), Pearson found *experimental* values of 0.3234, 0.3487, 0.3238, and 0.3181. Similarly, for the samples of 20 items each and a "normal theory" standard error of 0.2294, he found *experimental* values of 0.2413, 0.2405, 0.2289, and 0.2308. His conclusion should be familiar to geophysicists who make use of correlation techniques: "Taken as a whole, the extent of agreement has proved considerably greater than was anticipated before the experiment was commenced, and it seems to indicate that the coefficient of correlation is another of the criteria based on *ratios*, whose distribution even in very small samples is remarkably insensitive to changes in the form of the population." Indeed, one must ask, what geophysicist would not view the above agreement between Pearson's normal-theory and experimental standard errors as near-perfect?

In a somewhat similar experimental sampling study, BAKER (1930) drew 50 samples of 40 items (pairs) each taken from a *highly skewed* population. This case, like the one just cited, constitutes a test of the case of *zero* correlation in the parent population. Baker drew the conclusion that his resultant distribution of r-values was "very skew", but the geophysicist must not be too quickly misled by such mathematical-statistical pessimism, since inspection of his results reveals that 29 of the 50 lay within one theoretical standard error of

[1] For additional data supporting this conclusion, see the recently published analysis by Huff and Neil, *J. Geophys. Res.* **64**, pp. 541—547 (1959).

zero, and an additional 15 cases lay within two standard errors of zero. This constitutes discernible discrepancy but hardly *geophysically* alarming asymmetry. The remaining six of the fifty samples fell above (more positive than) two standard errors in excess of zero, presumably a consequence of the high probability of independently drawing two items such that both lie below the mean of a positively skewed parent population. The conclusion of relevance here is that Baker's study tends clearly to support the view that non-normality of even a marked degree is not so large that it would seriously mislead the geophysicist in most problems wherein correlations are used, even though Baker, as a statistician, took his results as empirical evidence of the seriousness of the non-normality problem.

HALDANE (1949), in a note on the problem of non-normality in correlation, summarizes his and others' sampling studies as having led to the conclusion that "the normal bivariate surface may be distorted and mutilated to a remarkable degree without seriously influencing the frequency-distribution of "r" in the case of small parent-population correlation (i.e., in the case of importance in making significance tests for mere *existence* of correlation).

To see the kind of results that arise in experimental sampling from parent bivariate non-normal populations with *non-zero* correlation, geophysicists should refer, for example, to the work of CHESIRE, OLDIS, and PEARSON (1932). For the explanation of their sampling model, the original paper must be consulted; here it is only necessary to note that these workers set up two populations with known correlation coefficients equal to 0.5000 and 0.5462 (number of digits chosen by those authors) and drew 1,000 samples of 5 pairs each, 500 samples of 10 pairs each, and 250 samples of 20 pairs each. One population had symmetric triangular frequency-distributions for both variates, the other population had one triangular symmetric and one asymmetric distribution. "Normal theory" values of r and of its standard error were computed and compared with experimental values. Because the implications of these results are of practical concern to geophysicists and to the thesis of the present paper but are to be found only in a paper that is probably not generally familiar, comparative values for just the cases of 10 and 20 pairs per sample are displayed in Table 2.

Table 2. Observed and theoretical values of correlation coefficient r, and standard error, σ_r for 500 samples of 10 pairs and 250 samples of 20 pairs drawn by CHESIRE et al. (1932).

		Samples of 10 pairs		Samples of 20 pairs	
		I[1]	II	I	II
Mean r	Obs.	.4871	.5289	.4896	.5460
	Th.	.4787	.5242	.4900	.5359
σ_r	Obs.	.2371	.2383	.1701	.1503
	Th.	.2671	.2534	.1780	.1677

[1] I and II designate the two populations samples, II being composed of a triangular asymmetric and an asymmetric subpopulation (see text). I and II had parent-population correlation coefficients of 0.5000 and 0.5462, respectively. For deduction of "theoretical" sampling values see original paper.

The authors conclude that there are "real differences between the distribution of r in samples from these two populations and those appropriate for the normal case", but admit that one's view as to whether these differences would be considered serious in practice depends upon the degree of accuracy required in particular investigations. Putting the author's apologies aside as sheer fussiness, I would be forced to ask: What geophysicist would see any significance in the differences displayed by the pairs of observed and theoretical correlation coefficients in Table 2 if he encountered these in his own research?

3. Tests for existence of correlation

If the question of normality has been decided in favor of believing that it may be ignored there next arises the question of whether a computed product-moment coefficient may safely be taken to indicate that the parent population is characterized by non-zero correlation, i.e., one next wishes to make a probability statement relative to the *existence* of correlation (without regard to "true" magnitude). CONRAD and POLLAK (p. 245) quote a rule-of-thumb criterion which used to appear in the literature of meteorology and climatology fairly frequently, namely the rule that a coefficient must equal or exceed twice its standard error if it is to be taken as evidence of the *existence* of correlation in the parent population?[2] We now regard it more fashionable to invoke arguments based on the t-test. To be sure, it takes only a little more time to use a t-test than to argue on the basis of a standard error, yet with reference to the main thesis of the present paper it is relevant to observe the rather unimportant degree of refinement in all but rather small samples (in less than about 20) which this introduces into one's statistical inferences. In Table 3 are shown, for several values of sample size N, values of the product-moment coefficient r that will be just significant at the 95 per cent confidence level based on the t-test, as described, for example, by BROOKS and CARRUTHERS (1953, p. 220). Also shown, for comparison, are corresponding values of r, for the same sample sizes, that would be regarded as significant at about the same level on the basis that r must then equal or exceed twice the standard error as given by

$$\sigma_r = 1/(N-2)^{1/2}$$

Table 3. Values of r significant at the 5 per cent level as derived for two criteria.

Sample Size N	(1)[1]	(2)[2]
10	.63	.71
20	.44	.47
40	.31	.32
60	.25	.26
80	.22	.23
100	.20	.21

[1] Values of r in Column (1) are based on the t-test.
[2] Values of r in Column (2) equal twice their respective standard errors, i.e., are solution of equation (1) for $r = 2\ \sigma_r$.

It can be seen from Table 3 that if one used only the old rule-of-thumb criterion, he would, for all but very small sample-sizes be employing almost exactly the same threshold values that he would derive from the t-test for r applied at the 95 per cent significance level. For samples much smaller than 20 variate-pairs the difference between the two criteria becomes numerically appreciable, but then it seems relevant to note that the basically arbitrary step of selecting 95 per cent rather than 90 or 99 per cent as the working significance-level introduces a difference about as large as the observed difference in threshold r-values of the two columns of Table 3. So light a treatment of the difference between, say, a 95 and a 99 per cent significance-level will, presumably, shock the statistician, but except in contexts where very important decisions hinge on interpretations of statistical results and where the basic data are of high accuracy the statistician cannot really defend, I believe, his typical point of view. I would contend that only in such atypical instances as weather modification experiments does one really have any reason to push statistical sophistication to the limit. And even in that example, how many times the investigator finds himself embarrassed by the usually woeful inadequacy of observational data upon which to work with the more refined statistical tools.

[2] The philosophical meaning of the phrase "parent population" is, of course, obscure when sampling time series, but is to be understood here in common-sense terms. There is evident need for closer grappling with this difficulty that underlies so much of climatology's and hydrology's use of statistics.

4. Determination of confidence limits

The third basis for suggesting that many of the niceties of sampling theory of product-moment correlation coefficients need not greatly trouble the geophysicist who has not been able to become thoroughly familiar with them concerns the final problem of placing *confidence limits* on a given computed correlation coefficient. It is *theoretically* important to distinguish such a process from that just discussed, for the sampling distribution of correlation coefficients drawn from a parent bivariate population of *zero* correlation is itself normal, but the corresponding distribution for a parent population of *non-zero* correlation is skewed and (still speaking theoretically) this affects significance-testing procedures. In almost any reference which the geophysicist may consult for aid in interpreting his correlation analyses, he will be confronted with disquieting admonishments against using a simple standard-error argument in setting his confidence limits on r. If, being sufficiently disturbed by these admonishments, he reads further, he is led to use Fisher's hyperbolic arctangent transformation, usually called simply the z'-transformation.

The technique whereby one transforms r into z', places selected confidence limits above and below this value of z' by what amounts to a t-test, and then makes the inverse transformation is well explained in many statistics texts, so it requires no comment here. The interested reader will find particularly clear discussions given by OSTLE (1954, pp. 181—185) and MILLS (1955, pp. 297—309), and a handy graphic aid for effecting the r—z' transformation and its inverse is presented by SNEDECOR (1946, pp. 1952—53). However, before acquiring familiarity with the z'-transformation, the geophysicist should ask whether use of this refinement of sampling theory is justified by the usual levels of precision encountered in his statistical studies. Table 4 provides information that I believe calls for a negative answer. It is evident from Table 4 that using plus and minus twice the standard error of r (strictly speaking one should use 1.96 times the standard error, to secure a 95 per cent half width, but such a 2 per cent refinement is no more materially important than the other refinements here under discussion) yields estimates of the 95

Table 4. Comparison of 95 pct confidence half-widths of r for two sample-sizes N

r	$N = 20$			$N = 80$		
	d[1]	d'	d''	d	d'	d''
0.10	.46	.42	.46	.22	.21	.22
0.20	.44	.39	.47	.21	.20	.22
0.30	.42	.36	.46	.20	.19	.21
0.40	.39	.31	.45	.19	.17	.20
0.50	.35	.27	.43	.17	.15	.20
0.60	.30	.22	.39	.14	.12	.16
0.70	.24	.17	.33	.11	.10	.13
0.80	.17	.12	.25	.08	.07	.10
0.90	.09	.06	.14	.04	.04	.05

[1] $d = 2\sigma_r$. The quantity d' is the upper half-width of the 95 per cent confidence interval as derived from the z'-transformation, and d'' is the lower half-width of the 95 per cent confidence interval as derived from the z'-transformation.

per cent confidence half-widths that are, even in the cases of poorest agreement, close enough to those obtained by using Fisher's z' that geophysical inferences will seldom be seriously distorted by the discrepancy. Since use of the 95 per cent rather than, say the 90 or the 99 per cent confidence level is itself basically an arbitrary choice made by the investigator, and since substituting either one of these levels for the 95 per cent level will alter the half-widths by as much as or more than the replacement of the z' method by the two-standard-errors rule, little practical justification can be given for use of this circuitous transformation in most geophysical studies. The statistician who takes issue with so easy an acceptance of this level of approximation will have to show that there are geophysicists who really do make significantly different decisions depending on whether their significance-levels come out at one per cent or five per cent. I believe they are very rare indeed.

Note that, in general, double the standard error gives a half-width intermediate in value between the upper and the lower half-width so that, in a sense, $2\sigma_r$ constitutes a *single* figure rather nicely summarizing the two asymmetric limits of refined sampling teory. The statistician will, of course, object that it is exactly this asymmetry that makes it necessary to use the z'-transformation to correct for skewness in the r-distribution, and his position is qualitatively invulnerable.

The investigator who has not yet had the opportunity to familiarize himself with Fisher's z'-transformation should not hesitate to continue employing the simple standard error argument unless he feels that he is handling geophysical data of such rare precision and freedom from observational-sampling variability that errors of estimate of a few hundredths in his confidence limits are going to lead him to erroneous inferences. But any such unusual data are likely to be of such functional simplicity as to obviate resort to statistics in the first place.

5. Historical interpretation and summary

All that has been said above can be concisely summarized by saying that investigators dealing with climatological, hydrologic or other complex geophysical data should take a commonsense view of ordinary correlation analysis as seen in the context of data-precision and should, except in unusual cases, feel quite justified in using only the simplest of classical sampling theory for the Pearson product-moment correlation coefficient.

Since this thesis may seem retrogressive to some, it may be relevant to attempt an explanation here of why so many non-statisticians have acquired the tendency towards overelaboration that so often appears in the statistical discussions in geophysical papers. It seems to the writer, as a result of having recently had the curiosity to scrutinize about two dozen statistics texts and references in relation to this point, that a difficulty arises from the fact that statistics texts reflect too heavily the viewpoint of the theoreticians, and I believe it is fair to say that the latter have in recent decades (especially since the early work of R. A. Fisher) simply pushed their sampling theories in directions and to degrees of refinement that are often entirely out of proportion to levels of data-precision typical of those very areas of research which, for reasons of inherent complexity and inherent variability of subject phenomena, must involve statistical methods. (This historical speculation must not be taken too broadly, for there has certainly been much recent theoretical work designed to meet more practical investigative needs. Since these more recent advances are not well covered in the references typically contained in geophysical bibliographies, one sees a need for critiques on the application of these newer techniques in the geophysical sciences.)

By way of illustration of my above assertion that many statisticians have tended to work and write in an atmosphere of false precision, I would cite the following: In the course of preparation of this paper, as I examined increasing numbers of statistics texts and references, I became aware of a surprisingly general tendency for writers of statistics texts to compute and cite correlation coefficients to more than two significant digits (surely the limit that a sense of numerical proportion would seem to indicate). Systematic inspection of fifteen widely used and quoted statistics texts revealed, in fact, that eleven out of fifteen writers used three or more digits for r, and fully a third gave coefficients to *four or more digits!* Such practices may offer indirect explanation of an almost incredible but true instance wherein a non-statistician associated with a hydrologic project came to the writer with a fifty-year basinwide rainfall-runoff correlation equal to 0.946 when all years were included, and 0.957 when a single very wet year was excluded, and asked if this might be taken to indicate that a proposed major hydrologic experiment on that basin could be tested statistically by comparing the correlation coefficient for all years up to but not including the year of treatment with the coefficient found for that data plus the data for the year immediately following treatment! That statisticians ought not too quickly smile at such an interpretation of the significance of the digits in r is shown not only by the curious results of the previously-mentioned tally of fifteen textual practices on significant digits but also by many statistically recommended but almost meaningless practices such as those of the application of "attenuation corrections" to r, or adjusting r via Sheppard's correction, or a particularly astonishing case which is to be found in a text by SMITH and DUNCAN (1945, p. 303) wherein the reader is shown how an original correlation coefficient of 0.89576 can, by suitable algebraic manipulation, be converted into the Fisherian "maximum-likelihood estimate" of 0.89465! Clearly, statisticians who would write out r to five digits and then theorize on a means of improving it by one part in nine hundred are not addressing themselves to the problem of statistical manipulation of raingauge readings taken in 1908 under a rapidly growing shade tree or of streamflow data for a month containing a record flood which the observer was unable to gauge at crest because the access roads were submerged.

Within the literature of geophysics itself, there is no difficulty in finding writers who quote r to four places, but as a recent glaring instance of unreasonable statistical purism within our own field, I would cite viewpoints taken by LEVERT (1958), who criticizes another investigator's failure to test for normality and failure to "take care not to use the common Fisher's significance table" in a hydrometeorological study that yielded $r = 0.99$ for a sample of 140 pairs! It is very easy, and I speak with some experience here, to fall into the error of pursuing statistical refinement far beyond one's original goal. But how often does the investigator or his informed reader have any basis for looking beyond the first decimal place in r?

The geophysicist who has been consciously or unconsciously influenced by schools of statistical thought so divorced from awareness of basic physical and observational uncertainties may be well-advised to review and simplify all of his statistical practices along lines similar

to those here followed for the product-moment correlation coefficient and its sampling properties. If for no other reason than to reduce the burden of methodology to be assimilated by younger workers in the field of geophysics sciences, all *superficial* complexity of statistical methods in geophysics is to be discouraged. Where refinements are truly in order, as for example, in the evaluation of cloud-modification experiments, they deserve careful attention. But only confusion arises from over-sophistication in statistical manipulation of data in most geophysical studies.

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